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[THIRD SERIES.]

ART. I.—*Results derived from an examination of the United States Weather Maps for* 1872 *and* 1873; by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College. SECOND PAPER. With Plate I.

(Read before the National Academy of Sciences, Philadelphia, Nov. 3, 1874.)

Direction and velocity of the wind within areas of maximum pressure.

IN order to deduce from the weather maps the laws of the wind's motion within an area of maximum pressure, I proceeded in substantially the same manner as in the case of areas of low barometer, described in my first article, page 6 (see this Journal, July, 1874). I selected all those cases in which an area of maximum pressure, or high barometer, was so situated that the direction and velocity of the wind were given at a considerable number of stations for at least half of the entire area. Then placing a wire cross upon one of the weather maps over the center of an area of high barometer, with the wires pointing northeast and southwest, the area was divided into four quadrants, which are designated as the north, east, south and west quadrants. Then beginning with the west quadrant, I counted the number of stations at which the wind was reported from the north, also the number of observations from the northeast, the east, southeast, etc.; and in like manner for each of the four quadrants. The velocity of the wind for the stations of observation in the different quadrants was also noted. The same was done with each of the weather maps which furnished an example suited to this comparison. The total number of cases derived from the weather maps of two years (1872–3) was 188. All the observations made near the point of maximum pressure were rejected; generally all the stations included with the first isobar. Also no observations were employed beyond the isobar 30·00, and generally none beyond the isobar 30·10. I then found by addition the aggregate number of observations for each direction of the wind in the several quadrants, and from these numbers computed the wind's average direction for each quadrant. The average velocity of the wind for each quadrant was also determined. The following table shows the resulting velocities and directions, and I have added a line showing the angle which the wind's direction makes with a radius drawn from the point of greatest pressure.

For high barometer.

	West quadrant.	South quadrant.	East quadrant.	North quadrant.
Velocity of wind,	5·22 miles.	6·02 miles.	6·48 miles.	6·13 miles.
Direction of wind,	S. 63° 0′ E.	N. 29° 55′ E.	N. 36° 59′ W.	S. 59° 15′ W.
Angle with radius,	27° 0′	29° 55′	53° 1′	59° 15′

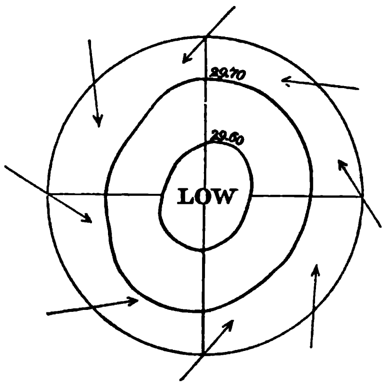
The importance of these results will be more apparent if we contrast them with the results obtained for areas of low barometer as given in my former article, pages 6–9. These results are as follows:

For low barometer.

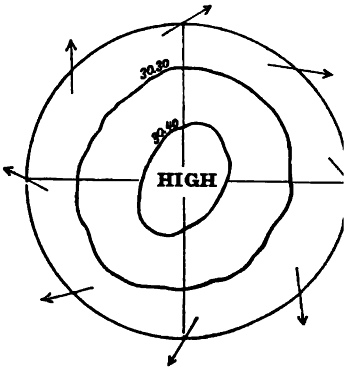
	West quadrant.	South quadrant.	East quadrant.	North quadrant.
Velocity of wind,	10·1 miles.	8·8 miles.	8·3 miles.	7·6 miles.
Direction of wind,	N. 58° 48′ W.	S. 40° 25′ W.	S. 32° 6′ E.	N. 42° 33′ E.
Angle with radius,	31° 12′	40° 25′	57° 54′	42° 33′

In order to exhibit these results palpably to the eye, I have constructed the two following figures, in which the arrows at

No. 1. For Low Barometer.



No. 2. For High Barometer.



the four cardinal points show the average direction of the wind according to the preceding tables, and the four intermediate arrows show the direction at the corresponding points as interpolated from the first four numbers. The lengths of the arrows are proportioned to the velocity of the different winds.

From these diagrams it will be apparent that in each quadrant the direction of the wind for high barometer is nearly opposite to the direction in the same quadrant for low barometer, the differences in the several quadrants being as follows:

West quadrant.	**South quadrant.**	**East quadrant.**	**North quadrant.**
4° 12′	10° 30′	4° 53′	16° 42′

For high barometer the average angle which the winds make with radii drawn from the point of greatest pressure is 42° 18′; for low barometer the average angle which the winds make with radii drawn from the point of least pressure is 43° 1′. In each case, therefore, the wind's direction is almost exactly midway between a tangential and a radial movement, but approaching somewhat nearer to the latter.

In order to determine to what height above the level of the sea the law of movement shown in the preceding diagrams prevails, I discussed the observations made on the summit of Mt. Washington at an elevation of 6,285 feet above the level of the sea, in the same manner as I had already discussed the observations at the other stations, and obtained the following results:

For low barometer on Mt. Washington.

	West quadrant.	**South quadrant.**	**East quadrant.**	**North quadrant.**
Velocity of wind,	49 miles.	44 miles.	37 miles.	32 miles.
Direction of wind,	N. 55° 7′ W.	N. 76° 35′ W.	S. 53° 44′ W.	N. 20° 6′ E.
Height of barometer reduc. to sea-level,	29·54	29·60	29·76	29·76

For high barometer on Mt. Washington.

	West quadrant.	**South quadrant.**	**East quadrant.**	**North quadrant.**
Velocity of wind,	32 miles.	18 miles.	38 miles.	32 miles.
Direction of wind,	S. 14° 37′ E.	N. 4° 8′ W.	N. 54° 48′ W.	N. 57° 52′ W.
Height of barometer reduc. to sea-level,	30·28	30·21	30·03	30·11

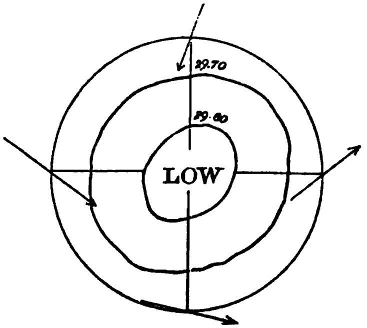
These results are graphically represented by the following diagrams, Nos. 3 and 4, in which, for convenience, the force of the wind is represented upon a scale only one-fourth as great as in the two previous diagrams.

The number of observations from which the results in Nos. 3 and 4 are derived is much less than for Nos. 1 and 2, and this will probably explain some anomalies which might be expected to disappear in the average of a greater number of observations.

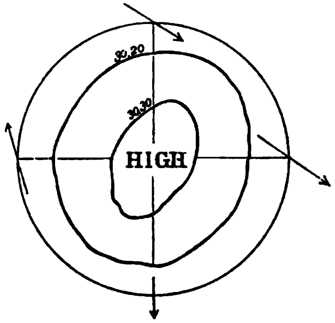
A careful comparison of these diagrams will show that the winds represented in Nos. 3 and 4 correspond pretty well with what might be expected from the winds represented in Nos. 1

and 2 struggling against an upper current from some N.W. quarter. The average direction of the wind on the summit of Mt. Washington in 1873 was from N. 76° W. If we regard this as the normal wind, and the winds shown in Nos. 1 and 2 to be disturbing forces tending to interfere with the normal wind, and we attempt to construct a triangle in which two of the sides shall represent these two forces, and the third shall represent the wind actually observed, we shall obtain a satisfactory construction in six out of the eight cases. In two cases the construction fails, viz., for low barometer in the west quadrant and for high barometer in the north quadrant, but a small change in the value of one of the angles would render the construction possible. These figures indicate that the average amount of the disturbing force is about equal to the normal force; in other words, the force of the winds shown in Nos. 1 and 2, at the height of 6,285 feet, is about equal to the force of the normal current which prevails at that elevation. It seems, therefore, reasonable to infer that at the height of 5,000 feet, the winds shown in Nos. 1 and 2 must generally be strong enough entirely to displace the normal current at that height.

No. 3. Low Barometer on Mt. W.



No. 4. High Barometer on Mt. W.



It will be noticed that "for high barometer on Mt. Washington," the average height of the barometer was small, particularly for the east quadrant. But for the same dates, the average height at Burlington, Vt., was 30·35 inches, which suggests the idea that in those cases the reduction of the Mt. Washington observations to the level of the sea was too great.

A comparison of diagrams 1 and 2 cannot fail to suggest the idea that a high barometer must exert an important influence upon a neighboring low barometer. It will be noticed that when the high barometer is on the east side of the low barometer, both systems tend to impress nearly the same direction upon the wind at a point intermediate between them, and the same is true upon whichever side of the low barometer the high barometer is situated. It seems natural to conclude that the result must be an increased steadiness and force of the winds in question. How far this conclusion is confirmed by the observations, and what may be its effect upon the direction and velocity of a storm's progress, I propose to consider hereafter.

Consequences of the outward flow of air from an area of high barometer.

We have found that from an area of high barometer there is an outward movement of the air having an average inclination of 42° 18′ to a line drawn from the point of greatest pressure. Near the surface of the earth, the average velocity of this movement is 5·96 miles per hour, but as we ascend above the surface, this velocity is very much increased. Suppose now that the area of high barometer is a circle 1,500 miles in diameter; that the barometer at the center of this area stands at 30·50 inches, and that the average velocity of the entire column of air is 18 miles an hour, which is the mean between the velocities observed near the level of the sea and on the top of Mt. Washington. Assuming that the average inclination to the radius is 42° 18′, we find the resolved portion of the wind's motion in the direction of radius is 13·3 miles per hour. What must be the effect upon the height of the barometer resulting from this outward movement from the area of maximum pressure, the flow being supposed to extend to the height of 5,000 feet? In less than three hours the barometer would be reduced to thirty inches, if there were no counter movement in the upper regions of the atmosphere. Now we know that these areas of high barometer have considerable permanence. It is quite common for them to remain nearly stationary for 24 hours; frequently they continue thus nearly stationary and without much change for 48 hours; and cases sometimes occur in which the barometer remains above 30·25 inches for four or five successive days and even longer. Thus from Dec. 29, 1872, to Jan. 2, 1873, the barometer at New Haven was never below 30·30; from Nov. 29 to Dec. 3, 1873, the barometer was never below 30·35; and from Dec. 31, 1873, to Jan. 6, 1874, it was never below 30·25.

These facts appear to me to prove that areas of high barometer are maintained by a continued accession of air in the upper regions of the atmosphere, and this accession is probably derived from the air which ascends near the center of an area of low barometer. In my former article, page 10, I have shown that near the center of an area of low barometer there is a strong *upward* movement. The facts which I have now presented appear to me to demonstrate that near the center of an area of high barometer there is a *downward* movement, not generally violent, but steady and of long continuance. The result of such a downward movement must generally be a considerable fall of the thermometer at the surface of the earth. The atmosphere generally loses heat most rapidly at the top, in consequence of radiation into space, and it receives its heat chiefly at the bottom from the effect of the sun's rays upon the surface of the earth, so that at an elevation of a few miles a pound of air usually contains a less amount of heat than a pound of air near the earth's surface. Hence we conclude that within an area of high barometer the thermometer must generally stand below its mean height.

Monthly minima of temperature.

In order to test the preceding conclusion, I have taken the monthly minima of temperature as observed at New Haven for the years 1872, 3, and 4, and have compared them with the state of the barometer and such other circumstances as could be supposed to influence the result. These observations of temperature were all made with a self-registering thermometer. The precise hour of minimum is therefore unknown, but it occurred sometime during the night preceding the date given in column first. In the following table, column first shows the date of minimum temperature for each month; column second shows the lowest temperature recorded; column third shows the direction of the wind as recorded at the morning observation, which in summer was generally 6 A. M., and in winter about 7 A. M.; column fourth shows the degree of cloudiness at the same hour; column fifth shows the height of the barometer at New Haven; column sixth shows the position of the nearest center of high barometer as indicated by the U. S. weather maps; and column seventh shows the highest isobar represented on the map for the corresponding date.

Upon comparing these observations, we perceive that in all but five cases the sky at the date mentioned was cloudless, and in only two cases did the clouds cover as much as one-half of the sky. It is probable that in each of these cases, during a portion of the preceding night the sky had been entirely cloudless. We must conclude, then, that these monthly minima of temperature were in part the result of *radiation;* but this cause alone will not account for the very low temperature observed for these observations were very much below the average temperature of the same months on cloudless nights. We find that in about two-thirds of the cases, New Haven was included within the area of a high barometer; that in several of the remaining cases an area of high barometer existed at the west and was rapidly approaching New Haven, although the barom ter at the latter place had not yet risen above 30 inches; and hat in the few remaining cases there was at no great distance from New Haven an area of *relatively high* barometer, lthough the actual height of the barometer did not much exceed 30 inches.

Least temperature for each month at New Haven, Conn., for the years 1872, 3, *and* 4.

Date of minimum.	Min'm temp.	Wind.	Cloudiness.	Barom. N. H.	Center of high barometer.	Highest isobar.
1872. Jan. 31	4°·4	N.W.	0	30·25	Nashville.	30·50
Feb. 23	6·7	N.	3	30·07	Norfolk, Va.	30·10
March 5	—1·2	N.W.	0	29·95	Chicago.	30·30
April 18	28·1	N.	0	29·97	Near Long Island.	30·00
May 1	39·2	S.E.	3	30·26	Near New York.	30·30
June 2	48·2	N.	0	30·01	Lake Huron.	30·10
July 25	56·0	W.	5	30·08	Gulf of Mexico.	30·20
Aug. 31	54·0	W.	0	29·70	Illinois.	30·10
Sept. 4	43·3	N.	0	30·05	Pittsburg, Pa.	30·10
Oct. 30	29·3	N.E.	0	30·57	Maine.	30·60
Nov. 30	10·3	W.S.W.	0	29·75	Georgia.	30·50
Dec. 28	—20·0	N.	0	30·10	Nashville.	30·60
1873. Jan. 30	—26·0	N.W.	0	30·25	Pennsylvania.	30·30
Feb. 24	—6·0	W.	0	29·77	Nashville.	30·30
March 5	8·5	N.W.	0	30·53	Knoxville.	30·70
April 1	31·8	W.	0	30·08	Chesapeake Bay.	30·10
May 4	33·2	N.	1	29·96	Louisville.	30·10
June 1	46·3	W.	0	30·35	Chesapeake Bay.	30·40
July 19	55·0	N.W.	5	29·94	Halifax.	30·20
Aug. 24	51·0	N.	0	30·06	Lake Huron.	30·40
Sept. 15	38·0	N.	0	30·27	Washington, D. C.	30·20
Oct. 30	29·4	N.	0	30·47	New York.	30·40
Nov. 29	15·5	W.	0	30·47	Wheeling, Va.	30·60
Dec. 31	10·0	W.	0	30·33	Wilmington, N. C.	30·60
1874. Jan. 17	3·6	W.	0	30·43	Lynchburg, Va.	30·50
Feb. 2	—1·4	N.N.E.	0	30·72	Quebec.	30·80
Mar. 24	12·2	N.N.W.	0	30·32	St. Louis.	30·70
April 5	14·8	W.	0	30·44	Baltimore.	30·40
May 12	32·7	N.	0	30·52	Long Island.	30·50
June 14	46·3	N.N.W.	0	30·26	Washington, D. C.	30·30
July 4	57·0	S.	0	30·05	Charleston, S. C.	30·10
Aug. 29	50·9	N.	0	30·06	Gulf of St. Lawrence.	30·10
Sept. 23	43·5	N.W.	0	30·28	Baltimore.	30·30
Oct. 15	32·0	N.	0	30·41	Virginia.	30·50
Nov. 14	19·1	N.	0	30·67	New York.	30·70

The preceding considerations appear to me to prove that the extremely low temperatures which occur at irregular intervals in every month of the year, but particularly in the winter months, are due mainly to the descent of cold air in the neighborhood of an area of high barometer; and this descent of the air results from the outward movement which generally takes place from the center of an area of high barometer.

Long continued periods of cold weather.

Sometimes we have a period of severe cold continuing ten days or even longer. The most remarkable cases of this kind within the last three years took place between Dec. 22, 1872, and Feb. 3, 1873. The following table shows the most important particulars respecting two periods of this description as observed at New Haven.

Date.	Max'm temp.	Min'm temp.	Barom. 7 A. M. N. H.	Wind. N. H.	Cl'di-ness.	Center of high barometer.	Highest isobar.	REMARKS.
1872.								
Dec. 21	34°·2	24°·7	30·16	N.N.W.	3	Nebraska.	30·70	Pleasant day.
22	17·5	11·3	30·12	N.N.W.	0	Kentucky.	30·60	1 in. of snow last nigh
23	29·4	—0·9	30·11	S.W.	10	Georgia.	30·30	Snow ceased at 10 A. Amount, 3½ inches.
24	17·4	8·7	30·40	N.W.	10	Illinois.	30·70	Pleasant afternoon.
25	16·2	—1·5	30·53	N.	5	N.Hampshire.	30·60	Pleasant day.
26	8·8	—1·0	30·06	N.N.E.	10	Montreal.	30·20	Snowed all day till P.M. Am't, 15 in.
27	14·4	4·8	29·72	N.	10	Nebraska.	30·70	Cloudy all day.
28	28·3	—11·7	30·12	N.	0	Tennessee.	30·60	Pleasant day.
29	27·3	6·2	30·34	N.W.	10	Georgia.	30·50	Cloudy all day.
30	24·3	0·2	30·53	N.N.W.	0	N. Carolina.	30·60	Pleasant day.
31	29·3	10·4	30·28	N N.E.	10	Georgia.	30·40	Snow till 4 P.M. Am 3 inches.
1873.								
Jan. 1	30·4	8·5	30·42	N.	0	Albany.	30·40	Fine.
2	20·6	3·3	30·45	N.	0	Maine.	30·50	Cloudy afternoon.
26	27·2	7·5	30·25	N.	10	Baltimore.	30·20	Slight snow in forenoo
27	26·0	19·3	29·99	N.E.	10	Dacotah.	30·40	Snow all day. Am't, 6 i
28	33·0	15·0	30·06	S.W.	0	Nebraska.	30·80	Fine.
29	12·2	0·0	30·35	N.W.	0	Indiana.	30·50	Fine.
30	20·4	—16·8	30·25	N.W.	0	Pennsylvania.	30·30	Fine.
31	34·2	6·3	30·15	N.W.	0	Dacotah.	30·50	Fine.
Feb. 1	33·8	9·7	30·10	N.W.	0	Nebraska.	30·70	Fine.
2	17·5	2·8	30·30	N.N.W.	0	Illinois.	30·60	Fine. [evenin
3	35·5	6·2	30·25	S.W.	4	Charleston.	30·30	Snow from 2½ P. M.

Column second shows the highest temperature and column third the lowest temperature observed at New Haven on each of the days named in column first; column fourth shows the height of the barometer, column fifth the direction of the wind, and column sixth the amount of cloudiness observed at New Haven at 7 A. M.; column seventh shows the position of the nearest center of high barometer, and column eighth the highest isobar drawn on the U. S. weather map for the corresponding date; column ninth shows some miscellaneous particulars recorded at New Haven.

It appears from the preceding table and from a comparison with the U. S. weather maps, that during the period above named there was an area of high barometer of unusual extent, which covered nearly the whole of the United States east of the Rocky Mountains and generally included New Haven. The only exceptions are near Dec. 27th and Jan. 27th. But at each of these dates an unusually high barometer prevailed at the same time at a great distance west, and its influence was felt not only at places where the pressure was above 30 inches, but also at places where the barometer had not yet reached the height of 30 inches. I conclude, therefore, that the severe cold which prevailed at this period throughout the United States east of the Rocky Mountains was mainly the result of cold air descending from the upper regions of the atmosphere under the influence of a high barometer.

The only other explanation of this phenomenon which I think can be plausibly urged, is that this cold was the result of a current of air sweeping along the surface of the earth from a very high northern latitude, and bringing with it the low temperature of the region from which it came. I admit that during the period in question northerly winds were unusually prevalent, but these were winds attending high barometer in accordance with the laws which have been already established. I admit also that when there is a general drift of the atmosphere from north to south over an extensive area it must bring with it a reduction of temperature, but I am satisfied that this cause alone will not account for the suddenness and magnitude of the depression in the present case. From the 21st to the 22d of December, the mean temperature at New Haven fell 15°, and it continued at about this point or even lower for nine successive days. Was there at this time a steady flow of air from the Arctic regions sufficient to account for this effect? I am unable to appeal to my weather maps for an answer to this question, for, unfortunately, on these days, the observations from most of the stations in the extreme northwest are wanting, and if it were otherwise, it might appear that the maps did not extend far enough northward to furnish all the information which was required. If our observations covered the whole area of North America, I have little doubt we should find that the depression of the thermometer below its mean height was greater in the United States than it was in the region north of us, as I have shown was the case in the storm of Dec. 20, 1836, of which the investigation was published in vol. xi of the Smithsonian Contributions to Knowledge.

Storm of Jan. 6–8, 1874.

In the absence of adequate observations from British North America, it may be more satisfactory to take an example of a storm in the southern part of the United States, in which case the weather maps will inform us of the condition of the atmosphere on the northern margin of the storm. For this purpose, I have selected the storm of Jan. 6–8, 1874, which came up from the Gulf of Mexico and crossed the United States in a direction about N. 30° E. The following table shows the observations of the thermometer at 7^h 35^m A. M., from Jan. 4 to

Jan. 9, 1874, at all the southern stations of the Signal Servic and most of the northern stations. These observations wer copied from the weather maps and forwarded to the chief signal officer at Washington, by whose direction they were compared with the official records, the errors were corrected and th omissions in the daily weather maps supplied. From Jan. 1s to Jan. 5th a storm traveled entirely across the continent fro west to east, the center of the storm passing over Lake Superio This storm was followed by unusually cold weather, as is show in the observations of Jan. 4th at Breckenridge, St. Paul Omaha, Fort Gibson, Keokuk, &c. On the 5th of Januar

Observations of the thermometer at 7^h 35^m *A. M., Jan.* 4–9, 1874.

	Mean Temp. Jan'y.	Thermom. at 7^h 35^m A. M.						Above or below mean temp.					
		Jan. 4.	Jan. 5.	Jan. 6.	Jan. 7.	Jan. 8.	Jan. 9.	Jan. 4.	Jan. 5.	Jan. 6.	Jan. 7.	Jan. 8.	Jan. 9.
Indianola, ...	55°	49°	35°	34°	38°	41°	52°	— 6°	—20°	—21°	—17°	—14°	—
Galveston, ...	55	52	39	34	43	48	55	— 3	—16	—21	—12	— 7	
Key West, ...	68	70	66	68	68	62	57	+ 2	— 2	0	0	— 6	—1
Punta Rassa, ...	62	68	64	62	63	50	44	+ 6	+ 2	0	+ 1	—12	—1
New Orleans, ...	55	65	50	38	35	38	58	+10	— 5	—17	—20	—17	+
Mobile, ...	52	58	48	43	33	32	50	+ 6	— 4	— 9	—19	—20	—
Lake City, ...	55	59	64	64	45	30	30	+ 4	+ 9	+ 9	—10	—25	—2
Jacksonville, ...	56	57	62	65	55	38	35	+ 1	+ 6	+ 9	— 1	—18	—2
Shreveport, ...	47	42	30	39	36	40	43	— 5	—17	— 8	—11	— 7	—
Montgomery, ...	48	53	54	48	35	32	44	+ 5	+ 6	0	—13	—16	—
Savannah, ...	52	60	59	64	51	36	37	+ 8	+ 7	+12	— 1	—16	—1
Charleston, ...	50	58	61	59	55	36	37	+ 8	+11	+ 9	+ 5	—14	—1
Augusta, ...	47	60	63	64	43	33	31	+13	+16	+17	— 4	—14	—1
Wilmington, ...	47	58	61	64	58	40	39	+11	+14	+17	+11	— 7	—
Fort Gibson, ...	40	15	11	21	22	31	30	—25	—29	—19	—18	— 9	—1
Memphis, ...	40	58	24	31	29	35	52	+18	—16	— 9	—11	— 5	+1
Nashville, ...	37	61	28	35	28	26	42	+24	— 9	— 2	— 9	—11	+
Knoxville, ...	35	38	38	43	33	29	31	+ 3	+ 3	+ 8	— 2	— 6	—
Louisville, ...	33	61	25	32	29	25	38	+28	— 8	— 1	— 4	— 8	+
Cincinnati, ...	31	60	30	33	32	28	38	+29	— 1	+ 2	+ 1	— 3	+
Norfolk, ...	37	57	61	43	64	42	36	+20	+24	+ 6	+27	+ 5	—
Lynchburg, ...	33	54	56	37	59	37	32	+21	+23	+ 4	+26	+ 4	—
St. Louis, ...	32	33	12	21	22	33	38	+ 1	—20	—11	—10	+ 1	+
Washington, ...	34	60	53	35	65	43	41	+26	+19	+ 1	+31	+ 9	+
Cape May, ...	33	46	48	43	50	43	38	+13	+15	+10	+17	+10	+
Baltimore, ...	33	47	51	38	56	44	35	+14	+18	+ 5	+23	+11	+
Keokuk, ...	27	13	8	11	16	28	30	—14	—19	—16	—11	+ 1	+
Omaha, ...	20	— 2	9	16	22	27	30	—22	—11	— 4	+ 2	+ 7	+1
Pittsburgh, ...	29	65	30	35	41	33	35	+36	+ 1	+ 6	+12	+ 4	+
Philadelphia, ...	32	52	48	36	57	45	35	+20	+16	+ 4	+25	+13	+
New York, ...	30	48	47	35	53	47	33	+18	+17	+ 5	+23	+17	+
Chicago, ...	26	35	15	17	25	27	34	+ 9	—11	— 9	— 1	+ 1	+
Cleveland, ...	28	60	31	33	33	30	37	+32	+ 3	+ 5	+ 5	+ 2	+
Buffalo, ...	27	57	27	26	32	34	33	+30	0	— 1	+ 5	+ 7	+
Boston, ...	27	47	49	32	34	55	34	+20	+22	+ 5	+ 7	+28	+
St. Paul, ...	14	— 3	6	17	12	21	23	—17	— 8	+ 3	— 2	+ 7	+
Breckenridge, ...	10	—20	0	12	16	18	9	—30	—10	+ 2	+ 6	+ 8	—

another and entirely distinct storm prevailed in the Gulf of Mexico; on the 6th it passed over Georgia; on the 7th it passed over Virginia, and on the 8th it passed north of Lake Ontario. This storm was followed by an unusual reduction of temperature in the southern States. Its effects were to some extent blended with those of the preceding storm, yet the effects of the latter can be distinctly traced, especially in the neighborhood of Florida. In the second column of the preceding table, I have given the mean temperature of January for each of the stations in column first; columns 3–8 show the observations of the thermometer at $7^h\ 35^m$ A. M.; and columns 9–14 show the differences between the observed temperatures and the mean temperature of January. The observations for January 8th are graphically represented on the accompanying chart, plate I, where the curve which passes through Mobile connects all those places where the depression of the thermometer was 20° below the mean; the curve which passes between Charleston and Savannah shows a depression of 15° below the mean; the curve passing near Nashville shows a depression of 10°; while the curve passing near Cincinnati connects those places having the mean temperature of January. Everywhere south of this line the temperature was below the mean; and everywhere north of this line the temperature was above the mean. It will be noticed from the table that on the 8th, in northern Florida, the depression of the thermometer was greater than it had been on either of the preceding days at Knoxville, Nashville, Cincinnati, Louisville and Memphis. This fact, together with the form of the isabnormal curves (convex toward the north), indicates that the cold did not come from the north or northwest, but must have descended from the upper regions of the atmosphere over the southern States.

There are other considerations of a more general nature which appear to me entirely conclusive with reference to the general principle which I have been trying to establish. The phenomenon in question is not peculiar to the United States, but prevails far to the north of us, even to the coldest regions which have ever been visited by man. It was found at Melville Island, lat. 75° N.; it was found at Van Rensselaer Harbor, lat. $78\frac{1}{2}$° N.; it is found in the coldest parts of Siberia. At Melville Island, on the 26th of December, 1819, during a strong wind, the barometer fell to 29·10 inches; it soon began to rise, and in four days it rose to 30·75, the highest point attained during the year. During the same time the thermometer fell from $-5°$ to $-43°$; the lowest temperature observed during the year. At Van Reusselaer Harbor, on the 28th of December, 1853, during a severe gale, the barometer fell to 29·05, and in two days it rose to 30 50. The thermometer during the same time fell from +16° to −22°. At Jakutsk, in Siberia, lat. 62°, the mean temperature of January is −44° Fahr. But on the 21st of January, 1838, the thermometer fell to −76° F., or 32° below the mean temperature of the season. Now according to Dove's charts, there is no place on the earth's surface where the mean temperature of the coldest month is much below that of Jakutsk. What can cause such an extreme depression of the thermometer at the coldest point of the earth's surface? I think we are shut up to the conclusion that this unusual cold results from the descent of the upper atmosphere to the surface of the earth. If this is the true explanation of periods of unusual cold in Siberia, a similar phenomenon in the United States is doubtless to be explained in like manner.

The *suddenness* of these changes of temperature is sometimes more remarkable than their magnitude. On the 21st of December, 1836, the thermometer at Albany fell 18° in one hour, from 11 A. M. to noon. Allowing for the usual diurnal change of temperature, this indicates an absolute change of temperature of 20° in one hour, being the effect of a severe storm then in progress. In summer, during a thunder shower, it is not unusual for the thermometer to fall 5° or even 10° in a few minutes. Now an examination of the weather maps will show that this phenomenon is local. There is no cold stream of air flowing down from the northern regions to produce this sudden change of temperature, but the effect takes place along the track of a storm, which generally follows nearly a parallel of latitude, with, however, a slight inclination northward, that is, from a warmer to a colder region. These sudden gusts of cold air are believed to descend from the upper regions of the atmosphere.

Connection between the velocity of the wind and the distance between the isobars in the neighborhood of a storm center.

In order to determine the connection between the velocity of the wind and the distance between the isobars, I ruled a large sheet of paper with 40 vertical columns, and placed at the head of the columns the numbers 0, 1, 2, 3, etc., to denote velocity of the wind. Each weather map was then examined for observations suited to the present object. In the selection of stations the following rules were adopted: 1. All the observations within the first isobar about a storm center were omitted. 2. No observations were employed beyond the isobar 29·90, and when any station within these limits seemed to be under the influence of another storm center that observation was rejected. All the observations made on Mt. Washington were rejected. For each station which satisfied the above conditions the perpendicular distance between the two adjacent isobars was measured, and the distance recorded under the corresponding velocity found at the top of one of the vertical columns. All the weather maps for 1872 and 3' were examined in this manner, and the distance between the isobars recorded. The following table shows the average results:

Relation between the velocity of the wind and the distance between the isobars.

Veloc. miles.	No. of Obs.	Mean distance	Av. distance	Dist. in miles.	Veloc. miles.	No. of Obs.	Mean distance	Av. distance	Dist. in miles.
0	69	·79	--	---	20	76	·63	·61	95
1	55	·90	--	---	21	9	·56	·62	97
2	81	·90	·86	134	22	23	·65	·62	97
3	53	·87	·86	134	23	13	·65	·62	97
4	176	·83	·84	131	24	40	·59	·62	97
5	111	·82	·82	128	25	13	·65	·61	95
6	168	·80	·80	125	26	5	·56	·59	92
7	80	·76	·79	123	27	5	·59	·58	90
8	230	·78	·78	122	28	20	·56	·54	85
9	65	·77	·76	119	29	4	·54	·54	85
10	129	·77	·75	117	30	15	·46	·52	81
11	56	·71	·74	115	31	4	·54	·50	78
12	262	·72	·73	114	32	7	·51	·48	75
13	34	·74	·69	109	33	0	--	·46	72
14	93	·70	·68	106	34	1	·44	·44	69
15	42	·60	·67	104	35	4	·39	--	--
16	122	·64	·64	100	36	4	·39	--	--
17	26	·65	·62	97	37	0	--	--	--
18	66	·60	·62	97	38	0	--	--	--
19	10	·60	·61	95	39	0	--	--	--

Column first shows the velocity of the wind in miles per hour; column second shows the number of observations corresponding to each velocity; and column third shows for each velocity the average distance between the isobars expressed in decimals of an inch. It will be noticed that the numbers in column second are very unequal; the number of observations for velocities 4, 8, 12, 16, 20, 24 and 28 being much greater than for the intermediate velocities. This shows that the velocities are not reliable within 1 or 2 miles, and this probably explains in part the irregularity of the numbers in column third. In order to smooth down these irregularities, I have taken the average of each sucessive five numbers in column third and set down the result in column fourth. The numbers in column fourth may, therefore, be regarded as exhibiting the results of the observations freed from some of the sources of error which affect individual observations. In column fifth, these distances are expressed in miles. This last column of numbers represents, therefore, the average distance between isobars (drawn at intervals of one-tenth of an inch) corresponding to the velocities given in column first. The average distance of the stations of observation from the center of low barometer is estimated at 350 miles. The numbers in column fifth are therefore regarded as representing for the United States the average distance between the isobars corresponding to the velocities in column first, at an average distance of 350 miles from a storm's center. We thus see that the average distance between the isobars is twice as great for the lowest velocity as it is for the highest velocity, so that the distance between the isobars affords some indication of the velocity of the wind, but the individual observations present so great anomalies that it is only the average velocity of the wind which can be safely inferred from the distance between the isobars. The great irregularities noticed in the individual observations are doubtless to be ascribed in part to the obstructions to the wind's motion arising from the inequalities of the earth's surface. Over a large expanse of water the distance between the isobars must afford a much more reliable indication of the force of the wind.

In preparing the materials for the preceding article, I have been assisted by Mr. Edward S. Cowles, a graduate of Yale College of the class of 1873.

ART. II.—*On some Points in the Geology of the Blue Ridge in Virginia;* by WM. M. FONTAINE.

PROFESSOR H. D. ROGERS, in the first volume of his Pennsylvania Reports, in giving a description of the strata lying along the lower Susquehanna, was at first positively of the opinion that this great body of slates is of Silurian age. He accordingly described this system as "altered primal upper slates." Since the publication of his first volume he announced the opinion that this section contained two, and perhaps more, series of unconformable strata, one of which is older than the Silurian, being perhaps equivalent to the Huronian of Canada.

This series of slates and quartzites passes south through Maryland into Virginia, where it attains great breadth and occupies the larger part of the State. It passes entirely across the State, having the Blue Ridge Mountains on its western border and the gold belt of this section on the east.

I had an opportunity, some time since, to examine in some details a part of the western portion of this belt, and propose to give in this paper a concise account of some of the geologica features there seen. My detailed examination was confined to the Blue Ridge, and to the parallel chain running some twenty miles east of it. I also studied, with less minuteness, portion of the valley between them.

The Blue Ridge at Harper's Ferry is a single chain, but in passing southwest it expands, so as to embrace a portion of the silurian rocks on the west, and also a considerable area of the more altered rocks on the east, until in some parts it is formed of several parallel, short ranges, composed of slate of very different character and age. Thus, according to the locality examined, one would find the main chain composed of argillites of seemingly very ancient date, or of coarse syenites, or again of Silurian quartzites.

The lower chain on the east leaves the immediate vicinity of the Blue Ridge in Maryland, diverging more and more as it passes through Virginia, so as to embrace a belt of the country which widens to the south. This chain has a variety of names, according to the locality. In Maryland and Northern Virginia it is called "Catoctin," but farther southwest in Virginia, "Bull Run." It is then broken up into a number of isolated mountains for a considerable distance, reappearing as the "Southwest," and "Green Mountains," in Albemarle County. Near Lynchburg, Va., it is called "Buffalo Ridge;" and southwest of this place, it is again dispersed into isolated ridges and peaks. For the sake of convenience, I shall call this entire range by its more northerly name, "Catoctin."

The valley between these ranges presents its simplest topography to the north. It is there occupied mainly by isolated hills of considerable magnitude and by some connected chains of the same. Farther to the southwest, mountains both isolated and in short ranges appear in the central portions. Near Charlottesville, Albemarle County, these attain considerable dimensions and continue with increasing force to beyond Lynchburg, a distance of more than sixty miles. Near Lynchburg the valley obtains its maximum width of about twenty-five miles.

The most northerly portion of this belt examined by me was at "Point of Rocks," Maryland, and at "Harper's Ferry." In order to give some idea of the portions of the entire belt not examined by myself, I shall quote from Professor Wm. B. Rogers' Virginia Reports; all the more freely, as they unfortunately never have been published.

At Point of Rocks, on the Baltimore and Ohio Railroad, we have a splendid exposure of the rocks, both in the tunnel through the mountain and in the cuttings for the canal. The rocks composing the Catoctin Mountains here are, in their least altered condition, well defined argillites. This particular variety forms a very important feature in the geology of the northern part of the Blue Ridge. Its lithological features are so pronounced that it can be traced with ease as far as Rockfish Gap, west of Charlottesville, but with greatly diminished breadth there. Here in the north, it occupies almost the entire space of twelve miles between Point of Rocks and Harper's Ferry. It forms also the most of the Blue Ridge at the latter place. Throughout this entire distance this rock shows a high southeast dip (45°), and evidently lies in a series of closed folds. The following is the normal character as presented throughout the district: Texture, fine, amorphous, to sub micaceous; color, dark gray, sometimes greenish, from films of chlorite. Cleavage usually obscured by consolidation, but parallel to the bedding. It occurs in more or less massive layers, often cut by joints, and is usually harder and denser than ordinary roofing slate. At Point of Rocks the following modifications are seen:

Commencing at the east entrance of the tunnel, we have the normal slates dipping 45° southeast; more toward the center intense local metamorphism appears. Here we find both cleavage and bedding lost, and finally see a massive structureless rock, but little harder than the true slates and of sub-micaceous texture; the particles being confusedly mingled and forming a substance not unlike some igneous rocks. This central portion is of a deep green color, from the great development of chlorite. It often has nests of epidote, sparingly disseminated and associated with knots and irregular veins of quartz. The quartz is very abundant. Some triclinic feldspar occurs in seams; and specular iron in brilliant, disseminated scales. Pyrite is quite common. This central, highly altered portion, has plainly been sufficiently softened to be capable o motion like a fused mass. It has been thrust up along the plane of bedding to the northwest. This is shown by the disturbance produced in the enclosing strata next to it, and also by the crumpling and bending of the slates on the west side At the west end of the tunnel, where we have the normal slates again, these are thrown into abrupt sigmoid flexures, indicating such a motion of the central mass.

I have little doubt but that this peculiar effect is due to the crushing of the beds in the center of a synclinal fold. But the existence of a closed synclinal alone will not account for the very general occurrence of such features, throughout so much of the northern Blue Ridge, and almost everywhere along the long ranges of the Catoctin. There must have been fracture and lines of weakness of great extent in the direction of these mountains; for we find in them true igneous rocks also, especially in the Blue Ridge.

The rocks occupying a belt of two and a half miles in width on the west of the Catoctin, exhibit along one or two lines th same features of metamorphism that are to be seen at Point o Rocks, but on a smaller scale. In each case we find this increase of local change along certain belts to be marked b ranges of hills commanding the country around. The Catocti

Mountains, these hills, and the Blue Ridge at Harper's Ferry, are each the result, in part, of such local changes producing elevation, but mainly of unequal erosion. The normal slates have been eroded to a much greater extent than the more highly metamorphosed rocks, and these latter have been left standing up in connected ridges.

The entire distance from Point of Rocks to Harper's Ferry, on the Maryland side, is apparently occupied by the argillites. Professor Wm. B. Rogers states, however, that on the Virginia side a narrow tongue of gneiss is found at the eastern base of "Short Hill," near the center of the tract. This is the northern termination of a triangular area of gneiss which passes through Virginia along the eastern base of the Blue Ridge. A narrow belt of steatite and serpentine is also found in Loudon County, along with elliptical bands of magnesian limestone, which sometimes assumes the aspect of true marble. These latter rocks also pass southwest through the State in the belt in question. Professor Rogers describes the curious occurrence of the limestones. They resemble huge flattened concretions, being entirely enclosed in hydromica or talc slates, and apparently pass into them by insensible gradations.

Resuming our detailed investigations at the eastern base of the Blue Ridge on the Maryland side, at Harper's Ferry, we still find the argillites forming the principal rock, and here also inclosing a core of metamorphic matter, which seems to be entirely formed out of the slates. This rock does not correspond exactly with any described in lithology, and I am at a loss to name it in the absence of any analysis of the materials. As it appears to be largely present throughout the State along the eastern base of the Blue Ridge, preserving a pretty constant position about two and a half miles from the Silurian strata, it merits a detailed description. I think it is one of the rocks called by Professor Rogers "greenstone."

This rock is seen with its most pronounced features at the bridge-head on the Maryland side, forming the perpendicular rugged mass which gives to the pass its most striking features. It is a massive rock in structure, having all the features of an eruptive igneous material. The principal component is a greenish-gray, rather waxy-looking amorphous base, having a hardness of two and a half, and a somewhat greasy feel. Imbedded in this material occur rounded shot-like particles of quartz as large as a garden pea. They have various colors, pink, bluish, and white, the former predominating. The luster of the quartz is waxy. The nodules sometimes almost disappear and sometimes form the larger portion of the mass. The rock has evidently been in a state of partial liquidity, and been thrust up along the plane of dip to the northwest. It often con-

tains enclosed fragments of argillites. sometimes little chang
and sometimes impregnated with chlorite, or with the mine
owenite, which is one of the metamorphic associates of this ro
Width about three hundred feet. Traced from the brid
head eastward, it is seen to pass gradually into unequivo
argillite, the quartz particles diminishing, and the base assumi
more and more the laminated, slaty structure of the argillit
This portion of the argillites seems to have been involved
the upheaval of the mass, for it is cut up by joints into angu
masses, much penetrated by irregular nests and seams of quar
and is without bedding. A little farther east, just outside
this, we find a curious modification of the metamorphic ro
It here appears in the form of dykes, which thrust off the sla
in arches. This rock is almost entirely composed of quar
with a little of the amorphous matter present, which gives t
stone a green color. Both this modification and the norm
rock form an exceedingly tough and durable material, whi
by its indestructible nature has preserved the mountain fr
erosion.

The above described rock, in weathered specimens especial
might be mistaken for a species of conglomerate; and I thi
has been so described by H. D. Rogers, in his account of t
South Mountains of Adams County, Pa. From the rese
blance of its base to a variety of *Pinite*, and for lack of a bett
name, I shall call it "pinite porphyry," although it is not a tr
igneous rock. Farther east we find argillites in heavy mass
penetrated with quartz, and leaning against the ridge with a d
of about 50° southeast.

To the west, up stream, for two miles to the east limit of t
Silurian, we have normal argillites preserving a predominati
southeast dip. Close to the pinite porphyry the slates are thro
into such abrupt flexures that they are rolled into cylindri
masses, which are penetrated by numerous quartz seams. Mo
to the west several short rolls occur, all denoting a thrust to t
west on the part of the metamorphic rock. The line of ju
tion of the argillites with the Silurian is sharply marked, a
no passage of one into the other occurs, although the southe
dip is preserved.

My next investigations were made along the line of the Ch
apeake and Ohio Railroad near Charlottesville, and at Rockf
Gap, where this road crosses the Blue Ridge by a tunnel sev
eighths of a mile long, affording a splendid section of the stra
Before passing to this, a short description, derived in part fr
Rogers' Virginia Reports, must be given of the interveni
country, for in this important modifications occur. They
as follows: The argillites diminish, and are confined to the imn
diate vicinity of the Blue Ridge. Quartzites of various natu

appear along the eastern border, and form the Catoctin to the southwest. These rocks in the north are the micaceous, slaty quartzites which form the Bull Run Mountains. To the southwest they become coarser, and by the assumption of a greater or less amount of feldspar, form gneissoid quartzites, or conglomerates. A schistose, argillaceous variety also appears here, much impregnated with chlorite and epidote, from its vicinity to the metamorphic rock, similar to that at Point of Rocks. Igneous rocks (not described) occur in the Blue Ridge near Swift Run Gap. Commencing on the east of the Catoctin range, we have the following rocks: 1st, Limestones, 2d, quartzites, and 3d, epidosites. These are usually associated, and the second form the mass of the Catoctin. These mountains in this region are usually composed of quartzites, while the limestones occur to the east of them. In the valley we find that from east to west steatites, hydromica slates, mica* slates, mica schists, and gneiss appear in the order named, so as to usurp the space which more to the north was occupied by argillites. The amount of epidote produced by concentrated local metamorphism along the line of the Catoctin is so much increased, that the partially fused rock, according to Rogers, is now a true epidosite, and seems to have invaded the quartzites and impregnated them with epidote and chlorite, just like a true igneous rock. This change may be due partly to the fact that the argillites, which seem to be the source of the epidotic and chloritic products, are more deeply buried in this region. There is reason to think that the quartzites and associated rocks overlie the argillites here; hence the disappearance of the latter rocks.

We have seen that at Harper's Ferry the metamorphic products are principally quartzose. The case is different to the southwest, along the Blue Ridge. For much of the distance occupied by these mountains in this intermediate space, but especially near Swift Run Gap, and through the counties of Green and Madison, we find the argillites forming the main mass of the mountains highly impregnated with epidote, chlorite, quartz and other metamorphic products. At the same time, impregnations of various compounds of copper are found at numerous points here. As I did not visit this region, I cannot speak more definitely of it. The increased variety of metamorphic products is no doubt due to the presence of true igneous rocks in this region. Rogers mentions their existence, bu does not describe them.

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* I have in this article used the terms *mica slate* and *mica schist* to denote different rocks. The former has slaty cleavages, is thinly laminated and fragile, having an amorphous to sub-micaceous texture, with a composition almost wholly formed of micaceous matter. The *mica schists* are stronger, heavier-bedded rocks, with quartz and micaceous matter in about equal proportions, and both appearing in visible individual particles. It is noteworthy that in no case is either constituent developed into large individuals. Thus the schist here differs much from that of the region adjacent to the Richmond granite, where both quartz and mica form large particles and produce a rock much like the mica schist of New York Island.

The Chesapeake and Ohio Railroad crosses the Catoctin Mountains (here called "Southwest") about two miles east o Charlottesville, at Shadwell, which is situated on the Rivanna River. The stream has cut a passage through the range, exposing the rocks in its bed. The structure here is essentially the same with that seen at Point of Rocks. A series of closed folds give a succession of southeast dips. The material composing the mountain, however, is no longer argillite, but a more massive argillaceous quartzite, of greenish color, from the great amount of chlorite and epidote developed in it. This mass, when more highly altered, resembles very closely a true igneous rock, and is the material called by Rogers "epidotic rock." In passing out of the region of argillites of the north into the quartzites o the more southern district, we note thus a change in the structure of the Catoctin with respect to the nature of the strata. We still find a metamorphic product like that at Point of Rocks only here with much more epidote. This, however, does no form any portion of the higher ground, called mountains in this section, but lies a short distance west of it, acting as the disturber and metamorphoser of the quartzite, which usually composes the mountains. The latter rock generally dips away from the epidosites (as we may call them), or stands vertically, and by its superior hardness has resisted erosion better.

The railroad crosses the valley nearly at right angles to the strike. By following it we get a tolerably good exposure o the strata. Just west of Charlottesville the first of a series o ledges of syenite is crossed. This rock, which abounds throughout the central portion of the valley, as far as Lynchburg, distant sixty miles, will be described in the account of the strata a that place.

The succession of rocks mentioned above as met with in going from east to west, in the district to the northeast of Charlottesville, occurs here also. We find, however, more abundan quartzites, and these of gneissoid character, extending farthe west of the Catoctin range into the central parts of the valley These quartzites play an important part in the structure of th rugged mass of mountains, which, commencing to the southwest of Charlottesville, and occupying the center of the valley, extend as far as Lynchburg. They are called near Charlottesvill "The Ragged Mountains." Formed of a capping of quartzite disturbed by the syenites, they assume the character of shor ranges confusedly arranged. Toward Lynchburg they are les largely composed of stratified rocks. Various coarse granite and syenites make up most of their mass, especially in the sho ranges called "Tobacco Row" and "No Business Mountains," lying above Lynchburg on both sides of the James.

The predominant rock along the railroad is a mica slate, and schist of varying character, but is mainly slate with micaceous matter in excess. The dip is usually high to the southeast. This is, however, not rarely reversed by the influence of the syenites. Near the eastern foot of the Blue Ridge, a very coarse gneiss is found.

The railroad climbs the mountain to the height of about seven hundred feet, gradually passing inward over the east slope, so that the tunnel is cut through the center and crest of the range. The mountain proper in the vicinity of Rockfish Gap is composed of argillites, highly altered by local metamorphism. Some very interesting effects are to be seen in passing over the eastern slopes. The metamorphic agent seems to have acted more energetically in certain bands, making an acute angle with the general direction of the mountain. Along these bands the slates have lost their cleavage, and have become much impregnated with epidote, quartz and chlorite. These three minerals often form concretionary masses, four to five feet in diameter, in which they are confusedly mixed together. Such lines of increased metamorphic action are, I think, determined by crevices or breaks in the strata.

The Blue Ridge at this point is a single chain, composed of highly altered argillites, with a central mass of eruptive rocks. Commencing at the east entrance of the tunnel, which is seven-eighths of a mile long, we find the argillite in heavy beds, lying with a dip of about 50° to the southeast. These slates have a dark greenish-grey color, from the presence of films of chlorite. They continue in the tunnel for about eight hundred and fifty yards, when we come upon an eruptive igneous rock, which extends about one hundred yards. This is succeeded by argillites, which continue with the same southeast dip to the edge of the Silurian strata, which are found near the west entrance. The southeast dip continues into the Silurian beds, but its steepness increases.

A belt of argillite on each side of the eruptive rock occurs, highly altered by the influence of the latter. In these belts the slates lose their bedding and cleavage, resemble a compact mass of angular fragments, and become impregnated with various minerals. When not thus impregnated, the texture is crystalline, the color grey, and the rock becomes harder and denser. The altered belt on the west side is much wider than that on the east, being as much as one hundred and fifty yards. In these altered belts occur many interesting associations of minerals, which can be well studied in the great mass of material used in making the two approaches of the tunnel.

The eruptive rock is a species of syenite of moderately fine texture. It contains a ground mass of rather fine granular red feldspar, quartz, hornblende and magnetic iron. In this occur porphyritically larger crystalline grains of red feldspar and quartz. The slates sometimes form a peculiar rock, indicating a gradual passage from slate to syenite, owing to contact effects. These portions might sometimes be mistaken for conglomerates, since they present rounded nodules of red feldspar and quartz, which exactly resemble water-worn pebbles. They lie imbedded in slaty matter, but perfectly distinct from it. We thus see that the structure here is precisely the same with that at Harper's Ferry. The central mass, however, is true igneous syenite. In composition it resembles the coarse syenite occurring farther southwest, but it is much better crystallized.

Among the products of metamorphism to be seen in the altered slates we may mention the following: 1. Rocks so highly impregnated with epidote as to appear a mass of acicular crystals. 2. The formation of numerous concretions of the size of a buckshot, composed of a core of epidote, with an envelope of milk-white quartz. 3. Calc spar with the characteristic cleavage. 4. This is sometimes intimately associated with epidote and silky fibers of asbestos, the spar being colored green by finely-disseminated hornblendic matter. 5. Masses of carbonate of lime, having a fine saccharoid texture like alabaster, and much like enclosed and metamorphosed fragments of ordinary limestone. They are, however, impregnations. Interesting studies both of the association and paragenesis of metamorphic minerals may be obtained.

The contact of the Silurian with the argillite is beautifully exposed in the west entrance of the tunnel, and the great contrast of the two systems thus brought side by side is well shown.

[To be continued.]

ART. III.—*Projection of the Fraunhofer Lines of Diffraction and Prismatic Spectra on a Screen;* by Prof. JOHN C. DRAPER, College of the City of New York.

HAVING been engaged during the past year in making photographs of absorption spectra of organic bodies, in which a solar spectrum with Fraunhofer lines was formed by a diffraction grating, I have resorted to the following method of forming such solar spectra, a description of which may prove of interest to those who are experimenting in the same field.

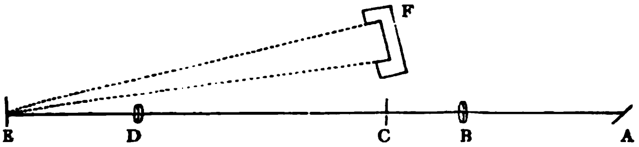
The grating generally used was made by Mr. L. M. Rutherfurd; it is ruled on speculum metal, 6,481 lines to the inch; it gives spectra by reflection. Other gratings on glass, now in my possession, give spectra by reflection and by transmission. The method answers equally well for both. It may be briefly stated as follows:

A beam of light is directed by the silvered plane mirror of a heliostat (A) into a darkened room.

It is received on an achromatic lens (B) ten centimeters in diameter; focal distance from posterior surface seventy centimeters.

A slit (C) is then placed within the focus of this lens, the distance being forty-eight centimeters from the lens (B).

After passing through the narrow slit, which is about one-tenth of a millimeter wide, the light is received upon a second achromatic lens (D), of the same diameter as the first, but with a focal distance of one hundred and fifteen centimeters. The distance of this lens from the slit is one hundred and sixty-four centimeters, and the focussing of the lines of the spectrum on a paper screen or on the ground glass of the camera is accomplished by moving the lens (D) nearer to or farther from the slit (C), or by moving the camera or screen (F) itself.



The grating (E), mounted on a suitable stand, is placed at a distance of eighty centimeters from the second lens. All parts of the apparatus being carefully adjusted, so that A, B, C, D, E are on the same horizontal axis, the grating is then arranged on its vertical axis, to throw the center of its reflected image on the opening of the slit (C).

The lines of the grating being accurately parallel to the sides of the slit, a series of beautiful spectra are produced on each side of the slit, any or all of which may be received on suitably adjusted screens, one of which is represented at (F). In all of these spectra, if the slit has been very narrow, the prominent Fraunhofer, with numerous other lines, appear sharply defined.

Of the spectra described above, only the first, second and third orders on each side of the image of the slit are available for general use on account of the overlapping of those that follow. Of those that are available, I have preferred to use the second order, since in this the dispersion is much greater than in the first, and by the apparatus described above a spectrum of a length of more than thirty centimeters is obtained.

For the projection of the prismatic spectrum a prism is substituted in place of the grating, when a very fine spectrum is produced, the focus of the violet end of which is very much closer to the prism than that of the red end.

In the diffraction spectra, also, it is necessary to vary the angle at which the screen is placed to define sharply the lines at the extremities of each spectrum. In the spectra of the first order on each side, the screen is placed very nearly at right angles to a line drawn from the grating to (*b*), in the spectrum. As each order in succession is examined, the divergence from this angle is greater and greater, and at the same time the focal distance of the lines moves nearer to the grating.

The lenses I have employed were those of a very fine photographic combination; they give with the rest of the arrangement a spectrum in which the definition of the lines is perfect, and they are present by hundreds. Though the lenses are ten centimeters in diameter, only the central portion of each is used, a diaphragm with a circular aperture of five centimeters or less being placed in front of (B).

To form the absorbent spectra of any organic substance, a suitable solution of the same is poured into a cell with parallel sides. This is placed at any convenient point between A and B, care being taken that the faces of the cell are at right angles to the course of the ray A, B. The slit may in this case be opened wider, when each spectrum will show the characteristic absorbent bands of the substance employed, the position being indicated (and if required, recorded) by their relation to the lines of the solar spectrum in which they are produced.

When the calcium or electric light is to be used for lecture room demonstration of diffraction spectra, the lens (B) should have as short a focus and as large a diameter as possible. The grating may also be so arranged on its vertical axis as to throw its image at a right angle to the line B, E, to be there received on a screen. Though by this device the spectra on one side of the image of the grating are greatly elongated and those on the other compressed, it presents the advantage of enabling the audience to see all the spectra at once, and also the optical contrivances by which they are produced.

November 25, 1874.

Art. IV.—*Abstract of results from a new discussion of the secular change of the Magnetic Declination in the United States, and some adjacent places in North and Central America*; by Charles A. Schott, Assistant U. S. Coast Survey.

(Read before the National Academy of Sciences, Nov. 4, 1874.)

[Communicated by permission of Carlile P. Patterson, Superintendent U. S. Coast Survey.]*

The present investigation incorporates the additional observations made or collected since 1859, and contains the improved results for the old stations as well as those for a number of new stations. The circular function adopted in the previous discussion to represent the secular change, continued to lead to satisfactory results, as might have been expected from its great adaptation to represent curves of a periodic character. Independent of the study of terrestrial magnetism, the necessity for the occasional reconstruction of the numerical expressions is sufficiently apparent from the demands of the Survey and the use which is made of them for the magnetic data given on our charts, viz: the magnetic declination (variation of compass) for a certain epoch, or date of publication, and its rate of change.

To briefly recapitulate the formulæ employed, let

$$D=\delta+r\sin(am+c)+r_1\sin(\alpha_1 m+c_1)+\ \ldots$$

express the magnetic declination at any time t, positive when west, negative when east of north; also, let m = number of years (and fraction of a year) from the adopted epoch t, or $m=t-1850$.

$$\alpha\quad\alpha_1\ \ .\ .\ \text{factors depending on the adopted periods,}$$

$$p\quad p_1\ \ .\ .\ \text{or}\ \alpha=\frac{360^\circ}{p}\quad \alpha_1=\frac{360^\circ}{p_1}\ \ .\ .\ .\ .$$

The quantities δ, a constant representing a mean declination, r r_1 . . . . parameters, and c c_1 . . . . epochal constants of the periodic terms, are to be determined from the observations at any one place by the application of the method of least squares, in order to satisfy the condition that the sum of the squares of the residuals of the observed and computed declinations shall be a minimum ($\Sigma\Delta^2$=a minimum). For this purpose put $\delta=\delta_1+x$, where δ_1=an assumed approximate value δ, and x a correction to it; also,

$$r\cos c=y\quad\text{and}\quad r\sin c=z,$$

then the conditional equations will take the form

$$0=\delta_1-D+x+\sin\alpha m\,.\,y+\cos\alpha m\,.\,z+\ \ldots$$

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* The paper in full will be published in Coast Survey Report for 1874. It is an extension of the article which appeared in this Journal, vol. xxix, Art. XXIX, May, 1860.

which are to be treated in the usual manner. To determine the value of α α_1 three (or more, if necessary) assumptions are made, and those values which render $\Sigma\Delta^2$ a minimum are deduced, and finally adopted. In some cases, where certain observations were evidently less trustworthy than others, and which nevertheless could not be dispensed with on account of the small number of observations, or on account of their reference to desirable epochs, special weights were assigned; generally the observations received the weight unity, a few imperfect observations the weight one-half. Of observations evidently grossly in error, no notice was taken.

The second periodic term depending on r_1 α_1 c_1 could only be established for a few places, owing to insufficiency in the number of observations and their want of the greater accuracy demanded for it.

The annual change v in the magnetic declination, positive when increasing west (or decreasing east), also the epoch of minimum west declination (or of maximum east declination), also its amount and the apparent probable error of an observation, are found as follows:

Differentiating the expression for D, we have

$$dD = r\alpha \cos(\alpha m + c)dm + r_1\alpha_1 \cos(\alpha_1 m + c_1)dm + \dots$$

hence for any time t, and for minutes of arc,

$$v = 60 \sin 1^\circ [r\alpha \cos(\alpha m + c) + r_1\alpha_1 \cos(\alpha_1 m + c_1) + \dots]$$

Maxima and minima are deduced from the equation

$$0 = r\alpha \cos(\alpha m + c) + r_1\alpha_1 \cos(\alpha_1 m + c_1) + \dots$$

from which expression we can find m.

The apparent probable error e_0 of an observation is deduced from the differences Δ of the n observed and computed values by the formula

$$e_0 = \sqrt{\frac{0{\cdot}455\Sigma\Delta^2}{n - n_1}}$$

where n_1 equals the number of unknown quantities in the expression of D, which had to be found from the observations; when weights, w, are used, substitute $w\Delta^2$ for Δ^2. The greater part of this apparent probable error is due to the fact that the observations collected at any one station were not generally made at the same spot; it represents, consequently, local irregularities in the distribution of magnetism, as well as pure observing errors.

The principal uncertainty in the investigation arises partly from the large observing errors in the older observations, made with ordinary compasses or with rude instruments generally, and partly, since the introduction of the refined instruments, the theodolite and magnetometer, from the circumstance that the various observations for the same nominal locality were taken at different spots, involving changes of local deflections of the magnet. From the extended use of iron and the growth of cities, it is difficult to select and preserve in such places a suitable locality for use at future times. Accurate investigations of the secular change can only be made at permanent observatories, or in localities not liable to disturbing influences.

In applying a periodic function to the investigation of the secular change, it is *not implied* that the phenomenon is necessarily of a periodic character, or must exhibit more than a single *complete* period; the aim is to represent by such a function the changes in the direction of the magnetic resultant, as far as observed.

The collection of the material is given first, the stations being arranged in geographical order, beginning in the northeast, passing to the south and west, and ending in the northwest.* For each locality the observed declinations are given in chronological order, together with such notes and references, respecting observer, place, publication, etc., as could be found. The stations here given are the only ones at present suitable for a discussion of the secular change, but their number is constantly increasing by the accumulation of new facts. [The collection referred to is here omitted.]

The following table contains the empirical expressions for the magnetic declinations derived from the collected observations by the process explained above, for various localities, together with their latitudes and longitudes. Total number of stations, 43, and of observations, about 417.

TABLE I.

Locality.	Latitude	Longitude	Expression of Magnetic Declination.
Halifax, N. S.	44° 39′·6	63° 35′·3	$D = +15^{\circ}{\cdot}94 + 4^{\circ}{\cdot}42 \sin(1{\cdot}0\,m + 49^{\circ}{\cdot}2)$
Quebec, Can.	46 48·4	71 14·5	$D = +12{\cdot}67 + 3{\cdot}84 \sin(1{\cdot}65m + 43{\cdot}6)$
York Factory, Hud. Bay.	57 00·	92 26·	$D = +5{\cdot}08 + 14{\cdot}12 \sin(1{\cdot}6\,m - 79{\cdot}4)$
Portland, Me.	43 38·8	70 16·6	$D = +10{\cdot}72 + 2{\cdot}68 \sin(1{\cdot}33m + 24{\cdot}1)$
Burlington, Vt.	44 28·2	73 12·3	$D = +11{\cdot}16 + 3{\cdot}76 \sin(1{\cdot}30m - 26{\cdot}3)$
			$+0{\cdot}18 \sin(7{\cdot}2m + 138)$
Rutland, Vt.	43 36·5	72 55·5	$D = +9{\cdot}76 + 3{\cdot}64 \sin(1{\cdot}6\,m - 19{\cdot}6)$
Portsmouth, N. H.	43 04·8	70 43·0	$D = +10{\cdot}29 + 2{\cdot}56 \sin(1{\cdot}37m + 5{\cdot}9)$
Newburyport, Mass.	42 48·4	70 49·0	$D = +9{\cdot}63 + 2{\cdot}63 \sin(1{\cdot}4\,m + 13{\cdot}2)$
Salem, Mass.	42 31·9	70 52·5	$D = +10{\cdot}22 + 4{\cdot}04 \sin(1{\cdot}55m - 6{\cdot}1)$
Boston, Mass.	42 21·5	71 03·8	$D = +9.46 + 2{\cdot}83 \sin(1{\cdot}3\,m + 4{\cdot}6)$
Cambridge, Mass.	42 22·9	71 07·7	$D = +9{\cdot}58 + 2{\cdot}69 \sin(1{\cdot}3\,m + 7{\cdot}0)$
			$+0{\cdot}18 \sin(3{\cdot}2m + 44)$

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* This approximates to an arrangement proceeding from the greatest western to the greatest eastern declinations.

TABLE I—*continued.*

Locality.	Latitude	Longitude	Expression of Magnetic Declination.
Nantucket, Mass.	41° 17′·0	70° 06′·0	D= + 8°·94 + 2°·45 sin (1·35m + 13°·8
Providence, R. I.	41 49·5	71 24·1	D= + 9·10 + 2·99 sin (1·45m − 3·4
			+ 0·19 sin (7·2m + 116)
Hartford, Conn.	41 46·	72 40·8	D= + 8·06 + 2·90 sin (1·25m − 26·4
New Haven, Conn.	41 18·5	72 55·7	D= + 7·83 + 3·16 sin (1·4 m − 21·6
Albany, N. Y.	42 39·2	73 45·8	D= + 8·22 + 3·05 sin (1·44m − 9·7
Oxford, N. Y.	42 26·5	75 40·5	D= + 6·19 + 3·24 sin (1·35m − 18·9
Buffalo, N. Y.	42 52·8	78 53·5	D= + 3·40 + 3·41 sin (1·4 m − 23·3
Erie, Pa.	42 07·8	80 05·4	D= + 1·27 + 2·00 sin (1·4 m − 10·5
Cleveland, O.	41 30·3	81 42·0	D= − 0·34 + 1·89 sin (1·4 m + 6·0
Detroit, Mich.	42 20·0	83 03·0	D= − 0·96 + 2·22 sin (1·5 m − 15·7
New York City.	40 42·7	74 00·4	D= + 6·43 + 2·29 sin (1·6 m − 5·5
			+ 0·14 sin (6·3m + 64
Hatboro, Pa.	40 12·	75 07·	D= + 5·23 + 3·28 sin (1·54m − 13·2
			+ 0·22 sin (4·1m + 157
Philadelphia, Pa.	39 56·9	75 09·0	D= + 5·42 + 3·35 sin (1·55m − 22·9
Washington, D. C.	38 53·3	77 00·6	D= + 1·79 + 1·90 sin (1·5 m + 5·9
Cape Henry, Va.	36 55·5	76 00·5	D= + 2·95 + 2·95 sin (1·55m − 35·3
Charleston, S. C.	32 46·6	79 55·8	D= − 2·75 + 2·38 sin (1·6 m + 15·2
Savannah, Ga.	32 04·9	81 05·5	D= − 2·54 + 2·32 sin (1·5 m − 28·6
Key West, Fla.	24 33·5	81 48·5	D= − 4·75 + 2·54 sin (1·4 m − 16·4
Havana, Cuba.	23 08·	82 22·	D= − 4·82 + 1·44 sin (1·3 m − 38·2
Kingston, Jamaica.	17 55·	76 50·	D= − 4·69 + 1·95 sin (1·2 m + 16·0
New Orleans, La.	29 57·2	90 03·9	D= − 5·68 + 2·52 sin (1·4 m − 63·8
Vera Cruz, Mex.	19 12·	96 09·	D= − 3·77 + 5·89 sin (1·1 m − 60·5
Mexico, Mex.	19 25·9	99 06·0	D= − 4·30 + 4·59 sin (1·1 m − 76·5
Acapulco, Mex.	16 50·5	99 52·3	D= − 3·97 + 4·96 sin (1·05m − 76·7
Panama, New Granada.	8 55·	79 30·	D= − 6·28 + 1·57 sin (1·2 m − 13·9
San Blas, Mex.	21 32·6	105 15·7	D= − 5·60 + 3·37 sin (1·0 m − 87·7
San Diego, Cal.	32 42·1	117 14·3	D= −12·54 + 1·64 sin (1·2 m −180·0
Monterey, Cal.	36 36·2	121 53·6	D= −12·82 + 3·54 sin (1·0 m −142·9
San Francisco, Cal.	37 47·5	122 27·2	D= −13·50 + 3·10 sin (1·0 m −132·7
C. Disappointment, W. T.	46 16·7	124 02·0	D= −20·72 + 2·81 sin (1·2 m −188·8
Sitka, Alaska.	57 02·9	135 20·	D= −29·08 − 0·010m + 0·00098m^2
Unalaska Island, Alaska.	53 52·6	166 31·5	D= −20·05 + 0·024m + 0·00080m^2

In the second table are exhibited, for each locality discussed, the observed and computed declinations (by preceding formulæ), expressed in degrees and fractions of a degree.

TABLE II.

Year.	Obsd.	Compd.	Year.	Obsd.	Compd.	Year.	Obsd.	Compd
Halifax, N. S.			Quebec, Can.			York Factory.		
1756·5	+12°·83	+12°·85	1649·5	+16°·0	+16°·34	1725·5	+19°·0	+19°·04
1775·5	13·58	14·05	1686·5	15·5	15·44	1787 5	+5·0	+4·9
1798·5	16·50	15·76	1810·5	11·0	11·25	1819·7	−6·00	−6·0
1818·0	17·47	17·25	1814·5	11·83	11·67	1843·5	−9·42	−9·0
1821·7	17·60	17·52	1831·5	13·63	13·54	1857·6	−7·62	−7·9
1852·5	18·17	19·41	1834·5	14·23	13·86			
1853·0	18·85	19·44	1842·5	14·20	14·66			
1860·5	19·92	19·76	1858·8	15·57	15·93			
1866 3	+21·09	+19·97	1859·5	16·28	15 97			
			1860·8	+16·47	+16·04			

TABLE II—*continued.*

Portland, Me.

Year.	Obsd.	Compd.
1763·5	+7°·75	+8°·05
1775·5	8·50	8·14
1845·4	11·47	11·55
1851·6	11·69	11·91
1859·5	12·33	12·33
1863·5	12·47	12·52
1864·8	12·73	12·58
1865·5	12·71	12·61
1866·1	12·72	12·64
1873·7	+12·89	+12·94

Burlington, Vt.

Year.	Obsd.	Compd.
1793·5	+7·63	+7·63
1818·5	7·50	7·51
1822·5	7·70	7·69
1826·5	7·60	7·94
1830·5	8·17	8·21
1831·5	8·25	8·29
1832·5	8·42	8·37
1834·5	8·83	8·52
1837·5	8·75	8·75
1845·5	9·37	9·33
1855·7	9·95	9·94
1873·8	+11·32	+11·32

Rutland, Vt.

Year.	Obsd.	Compd.
1789·3	+7·05	+6·51
1810·4	6·07	6·14
1811·7	6·02	6·16
1859·6	9·82	9·49
1873·8	+10·67	+10·91

Portsmouth, N. H.

Year.	Obsd.	Compd.
1771·5	+7·77	+7·78
1775·5	7·75	7·74
1850·7	10·50	10·60
1859·5	+11·25	+11·12

Newburyport, Mass.

Year.	Obsd.	Compd.
1775·5	+6·75	+7·00
1781·5	7·30	7·02
1850·7	10·09	10·28
1859·5	+10·97	+10·81

Salem, Mass.

Year.	Obsd.	Compd.
1781·6	+6·90	+6·47
1806·8	5·95	6·32
1810·5	6·09	6·49
1849·6	10·24	9·75
1855·6	+10·83	+10·40

Boston, Mass.

Year.	Obsd.	Compd.
1700·5	+10°·00	+9°·95
1708·5	9·00	9·45
1741·5	7·50	7·52
1776·0	7·67	6·62
1782·5	7·00	6·64
1793·5	6·50	6·82
1807·5	6·08	7·28
1839·5	9·10	9·03
1846·7	9·52	9·49
1855·6	10·23	10·05
1872·7	+11·25	+11·01

Cambridge, Mass.

Year.	Obsd.	Compd.
1708·5	+9·0	+9·30
1742·5	8·0	7·70
1757·5	7·33	7·28
1761·5	7·23	7·17
1763·5	7·00	7·13
1780·5	7·03	6·90
1782·5	6·75	6·89
1783·5	6·87	6·90
1788·5	6·63	6·93
1810·5	7·50	7·52
1835·5	8·85	9·02
1837·5	9·15	9·15
1840·4	9·30	9·36
1842·2	9·57	9·49
1844·5	9·65	9·65
1845·4	9·53	9·72
1850·6	9·50	10·07
1852·5	10·13	10·20
1854·5	10·21	10·33
1855·4	10·91	10·39
1856·5	10·47	10·46
1859·2	10·80	10·63
1867·5	+10·70	+11·09

Nantucket, Mass.

Year.	Obsd.	Compd.
1775·5	+6·50	+6·50
1834·5	8·45	8·64
1838·9	9·04	8·89
1842·7	9·15	9·11
1843·7	9·17	9·17
1846·6	9·23	9·33
1855·6	9·97	9·83
1867·4	+10·33	+10·42

Providence, R. I.

Year.	Obsd.	Compd.
1717·5	+9°·60	+9°·73
1720·5	9·47	9·49
1725·5	9·23	9·14
1730·5	8·90	8·85
1735·5	8·65	8·59
1740·5	8·25	8·33
1745·5	7·98	8·02
1750·5	7·67	7·66
1755·5	7·35	7·27
1760·5	6·95	6·88
1765·5	6·72	6·53
1769·5	6·50	6·29
1775·5	6·33	6·15
1780·5	6·27	6·12
1785·5	6·22	6·17
1790·5	6·17	6·25
1795·5	6·17	6·32
1800·5	6·25	6·37
1805·5	6·32	6·40
1810·5	6·40	6·45
1815·5	6·50	6·55
1819·5	6·62	6·75
1825·5	6·85	7·06
1830·5	7·17	7·45
1835·5	7·57	7·90
1840·5	8·42	8·36
1841·5	8·52	8·45
1842·5	8·65	8·53
1843·5	8·77	8·60
1855·6	+9·52	+9·42

Hartford, Conn.

Year.	Obsd.	Compd.
1786·5	+5·42	+5·28
1810·5	4·77	5·25
1824·5	5·75	5·60
1829·0	6·05	5·76
1859·6	7·29	7·34
1867·6	+7·82	+7·84

New Haven, Conn.

Year.	Obsd.	Compd.
1761·5	+5·78	+6·04
1775·5	5·42	5·27
1780·5	5·25	5·07
1811·5	5·17	4·77
1819·8	4·42	5·00
1828·5	5·28	5·35
1835·3	5·68	5·71
1836·5	5·92	5·78
1837·9	5·83	5·87
1840·5	6·17	6·02
1845·7	6·29	6·37
1848·6	6·58	6·57
1855·6	7·05	7·08
1872·5	+8·46	+8·37

TABLE II—*continued.*

Albany, N. Y.

Year.	Obsd.	Compd.
1817·8	+5°·73	+5°·71
1818·6	5·75	5·78
1825·3	6·00	6·07
1828·6	6·27	6·26
1830·5	6·30	6·37
1831·6	6·54	6·44
1834·8	6·67	6·64
1836·8	6·78	6·77
1847·9	7·58	7·57
1855·7	7·91	8·16
1856·7	8·58	8·23
1858·4	+8·28	+8·37

Oxford, N. Y.

Year.	Obsd.	Compd.
1794·0	+3·00	+2·96
1817·5	3·00	3·31
1828·5	4·50	3·79
1834·8	3·87	4·13
1836·8	4·15	4·26
1837·5	4·50	4·30
1838·5	4·45	4·36
1849·9	5·18	5·14
1857·3	5·73	5·68
1858·1	5·78	5·74
1859·0	5·83	5·81
1873·9	6 87	6·94
1874·4	+6·93	+6·97

Buffalo, N. Y.

Year.	Obsd.	Compd.
1797·5	0·00	+0·01
1837·5	+1·42	1·17
1839·5	1·25	1·30
1845·5	1·42	1·71
1859·5	2·94	2·81
1872·5	3·87	3·89
1873·5	+3·97	+3·97

Erie, Pa.

Year.	Obsd.	Compd.
1795·5	−0·72	−0·72
1841·6	+0·50	+0·51
1862·6	+1·55	+1·52
1873·4	+2·01	+2·03

Cleveland, O.

Year.	Obsd.	Compd.
1796·7	−2·00	−2·10
1830·5	−1·33	−1·03
1831·6	−1·25	−0·98
1834·1	−0·83	−0·87
1838·1	−0·58	−0·69
1841·3	−0·09	−0·54
1845·5	−0·65	−0·35
1859·5	+0·77	+0·29
1871·8	+0·54	+0 79
1872·5	+0·75	+0·81
1873·5	+0·85	+0·85

Detroit, Mich.

Year.	Obsd.	Compd.
1810·5	−2°·80	−3°·10
1822·5	−3·22	−2·82
1828·5	−2·83	−2·61
1835·5	−2·17	−2·31
1840·5	−1·97	−2·07
1859·5	−0·70	−1·01
1865·5	−0·67	−0·66
1872·4	−0·42	−0·28
1873·4	−0·29	−0·22

New York City.

Year.	Obsd.	Compd.
1686·5	+8·75	+8·78
1691·5	8·75	8·68
1723·5	7·33	7·53
1750·5	6·37	5·85
1755·5	5·00	5·46
1789·5	4·33	4·30
1824·5	4·67	4·64
1834·5	4·83	5·17
1837 5	5·67	5·37
1840·6	5·45	5·61
1841·5	6·10	5·68
1844·6	6·22	5·92
1845·7	6·42	6·01
1846·3	5·56	6·05
1847·8	5 68	6·16
1855·6	6 72	6·73
1860 7	6·73	7·03
1873·8	+7·62	+7·63

Hatboro, Pa.

Year.	Obsd.	Compd.
1680·5	+8·47	+8·49
1690·5	8·25	8·30
1700·5	7 92	7·94
1710·5	7·47	7·49
1720·5	7·00	6·95
1730·5	6·42	6·30
1740·5	5·58	5·56
1750·5	4·92	4·67
1760·5	4·00	3·75
1770·5	2·92	2·89
1780·5	2·08	2·21
1790·5	1·83	1·84
1800·5	1·92	1·79
1810·5	2·00	2·07
1820·5	2·45	2·56
1830·5	3·00	3·20
1840·5	3·83	3·89
1850·5	+4·42	+4·60

Philadelphia, Pa.

Year.	Obsd.	Comp
1701·5	+8°·50	+8°6
1710·5	8·50	8·3
1750·5	5·75	5·2
1793·5	1·50	2·2
1802·5	1·50	2·0
1804·5	2·08	2·0
1813·5	2·43	2·1
1837·5	3·87	3·1
1840·5	3 62	3·3
1841·7	3·90	3·4
1846·4	3·85	3·8
1855·7	4·53	4·6
1862·6	5·00	5·2
1872·8	+5·46	+6·1

Washington, D. C.

Year.	Obsd.	Comp
1792·5	−0·24	−0·0
1809·9	+0·87	+0·2
1841·0	1·34	1·5
1842·0	1·40	1·5
1855·5	2 40	2·2
1856·6	2·36	2·3
1857·2	2·41	2·3
1860·7	2·44	2·5
1862·7	2·66	2·5
1863·6	2 70	2·6
1866·8	2·74	2·7
1867·5	2·80	2·8
1868·5	2·85	2·8
1869·3	2·88	2·8
1870·5	2·89	2·9
1871·5	2·95	2·9
1872·5	3·00	3·0
1873·5	3·00	3·0
1874·5	+3·10	+3·0

Cape Henry, Va.

Year.	Obsd.	Comp
1732·5	+4·68	+4·7
1809·5	[0·00]	0·0
1832·5	+0·75	0·3
1856·7	+1·47	1·7
1865·8	[+2·54]	+2·4

Charleston, S. C.

Year.	Obsd.	Comp
1742·5	[−5·38]	−3·6
1775·5	−3·80	−5·0
1784·1	−5·25	−5·1
1785·8	−5·75	−5·1
1825·0	−3·75	−3·7
1837·5	−2·90	−2·9
1841·4	−2·40	−2·6
1847·8	−2·25	−2·2
1849·3	−2·28	−2·1
1874·4	−0·97	−0·8

TABLE II—*continued.*

Year.	Obsd.	Compd.
Savannah, Ga.		
1817·5	—4°·00	—4°·68
1839·0	—4·30	—4·18
1852·3	—3·67	—3·52
1857·3	—3·46	—3·24
1874·2	—1·98	—2·23
Key West, Fla.		
1829·1	—6·42	—6·57
1843·5	—6·03	—5·84
1849·6	—5·48	—5·49
1860·7	—4·78	—4·81
1861·2	—4·75	—4·78
1862·7	—4·68	—4·69
1863·5	—4·60	—4·64
1864·5	—4·56	—4·58
1865·5	—4·52	—4·52
1866·2	—4·49	—4·47
Havana, Cuba.		
1726·5	—4·4	—4·37
1732·3	—4·5	—4·54
1815·5	—7·0	—6·25
1816·6	—5·5	—6·24
1857·1	—5·25	—5·62
1858·5	—5·75	—5·48
Kingston, Jamaica.		
1732·2	—6·0	—6·28
1791·8	—6·78	—6·26
1806·0	—6·5	—5·86
1820·5	—4·8	—5·34
1822·5	—4·9	—5·26
1832·5	—5·2	—4·86
1833·5	—4·7	—4·82
1837·8	—4·3	—4·56
1847·3	—3·67	—4·24
1857·2	—3·67	—3·88
1866·5	—4·95	—3·55
New Orleans, La.		
1720·5	—2·0	—3·39
1768·5	—7·83	—5·77
1796·5	—5·10	—7·34
1806·5	—8·05	—7·75
1840·5	—8·33	—8·14
1857·0	—8·00	—7·72
1858·3	—7·86	—7·67
1870·5	—7·10	—7·13
1872·1	—6·99	—7·05

Year.	Obsd.	Compd.
Vera Cruz, Mex.		
1727·0	—2°·25	—2°·17
1769·4	—6·57	—6·79
1776·5	—7·5	—7·46
1815·5	—10·6	—9·60
1819·3	—9·3	—9·65
1856·6	—8·3	—8·49
1861·0	—8·3	—8·18
Mexico, Mex.		
1769·8	—5·45	—5·51
1804·0	—8·13	—7·94
1850·5	—8·59	—8·75
1856·9	—8·77	—8·58
1858·5	—8·37	—8·53
1860·5	—8·50	—8·46
1862·5	—8·34	—8·38
1867·0	—8·15	—8·18
1868·5	—8·17	—8·11
Acapulco, Mex.		
1744·5	—3·0	—3·32
1791·3	—7·73	—7·26
1822·5	—8·67	—8·74
1828·5	—9·12	—8·85
1838·0	—8·29	—8·91
1866·5	—8·37	—8·23
Panama, New Granada.		
1775·8	—7·8	—7·80
1790·8	—7·8	—7·83
1802·5	—8·0	—7·76
1822·5	—7·0	—7·42
1837·5	—7·03	—7·03
1849·5	—7·08	—7·67
1866·4	—5·93	—6·12
San Blas, Mex.		
1791·3	—7·47	—7·46
1822·0	—8·67	—8·64
1837·5	—8·57	—8·92
1839·5	—9·00	—8·94
1841·5	—9·20	—8·95
San Diego, Cal.		
1792·5	—11·00	—11·01
1839·5	—12·34	—12·18
1851·3	—12·48	—12·58
1853·8	—12·53	—12·67
1866·4	—13·16	—13·09
1872·9	—13·32	—13·29

Year.	Obsd.	Compd.
Monterey, Cal.		
1791·7	—10°·93	—11°·48
1795·5	—12·37	—11·76
1837·5	—14·50	—14·29
1839·5	—14·22	—14·40
1841·5	—15·00	—14·51
1843·5	—14·00	—14·62
1851·1	—14·97	—15·01
1873·7	—15·92	—15·91
San Francisco, Cal.		
1792·9	—12·80	—12·97
1827·5	—15·45	—14·80
1829·5	—15·10	—14·90
1837·5	—15·17	—15·27
1839·5	—15·33	—15·35
1841·9	—15·50	—15·46
1850·0	—15·68	—15·78
1852·3	—15·48	—15·86
1866·5	—16·42	—16·28
1872·8	—16·43	—16·41
1873·6	—16·42	—16·43
1874·0	—16·47	—16·44
C. Disappointment, W. T.		
1792·3	—18·00	—17·98
1839·5	—19·18	—19·70
1842·5	—20·00	—19·86
1851·5	—20·54	—20·38
1873·8	—21·61	—21·67
Sitka, Alaska.		
1804·5	—26·75	—26·58
1824·5	—27·50	—28·18
1827·5	—28·83	—28·36
1829·5	—28·32	—28·46
1830·0	—28·27	—28·48
1838·5	—28·62	—28·83
1839·5	—29·53	—28·86
1842·5	—28·88	—28·95
1851·0	—29·23	—29·09
1867·6	—28·82	—28·95
Unalaska Island, Alaska.		
1792·5	—19·0	—18·79
1806·5	—19·4	—19·58
1829·0	—19·9	—20·20
1848·5	—19·51	—20·08
1849·5	—20·00	—20·06
1867·7	—19·79	—19·38
1870.5	—19·75	—19·22
1873·4	—19·00	—19·05

The third table shows the number of observations at each place; the apparent probable error of one observation (including errors arising from want of identity of stations and from instrumental defects) expressed in minutes of arc; the computed epochs of greatest easterly deflection in the secular motion, together with the amount and direction of the declination at that epoch, and the computed annual changes at the epochs 1870 and 1880, expressed in minutes.

TABLE III.

Locality.	Number of Observations.	Apparent probable error of an observation.	Epoch of *easterly* secular digression (needle stationary).	Amount when at easterly digression.	Annual change in 1870.	Annual change in 1880.
Halifax, N. S.	9	±33′	1711	+11°·5	+1′·6	+0′·9
Quebec, Canada.	10	15	1769	+8·8	+1·7	---
York Factory, Hudson Bay.	5	14	----	---	---	---
Portland, Me.	10	9	1764	+8·0	+2·4	+1·6
Burlington, Vt.	12	6	1810	+7·4	+5·7	+6·0
Rutland, Vt.	5	15	1807	+6·1	+5 9	+5·4
Portsmouth, N. H.	4	5	1780	+7·7	+3·1	+2·5
Newburyport, Mass.	4	13	1776	+7·0	+2·9	+2 2
Salem, Mass.	5	26	1796	+6·2	+5·9(?)	+5·0(?)
Boston, Mass.	11	25	1777	+6·6	+3 3	+2·8
Cambridge, Mass.	23	11	1783	+6·9	+2·9	+2·1
Nantucket, Mass.	8	6	1773	+6·5	+2·6	+2·3
Providence, R. I.	30(?)	7	1780	+6·1	+3·8	---
Hartford, Conn.	6	14	1799	+5·2	+3·4	+3·0
New Haven, Conn.	14	9	1801	+4·7	+4·6	+4·3
Albany, N. Y.	12	6	1794	+5·2	+4·3	+3·8
Oxford, N. Y.	13	9	1797	+3·0	+4·5	+4·3
Buffalo, N. Y.	7	8	1802	0·0	+4·9	+4·7
Erie, Pa.	4	--	1793	−0·7	+2·8	+2·5
Cleveland, O.	11	12	1781	−2·2	+2·3	+1·9
Detroit, Mich.	9	11	1801	−3·2	+3·4	+3·0
New York City.	18	15	1797	+4·0	+2·4	+2·5
Hatboro, Pa.	18(?)	6	1797	+1·8	+4·6	+4·5(?)
Philadelphia, Pa.	14	20	1807	+2·1	+4·7	+3·7
Washington, D. C.	13	6	1786	−0·1	+2·4	+1·9
Cape Henry, Va.	5	14	1815	0·0	+4·8(?)	+4·7(?)
Charleston, S. C.	9	19	1784	−5·1	+2·7	+1·8
Savannah, Ga.	5	15	1809	−4·9	+3·6	+3·5
Key West, Fla.	10	4	1797	−7·3	+3·6	+3·4
Havana, Cuba.	6	26	1810	−6·3	+1·9	---
Kingston, Jamaica.	11	25	----	−6·6	+1·9	---
New Orleans, La.	9	30	1831	−8·2	+3·0	+3·4
Vera Cruz, Mex.	7	17	1823	−9·7	+5·3	---
Mexico, Mex.	9	5	1838	−8·9	+3·1	+3·8
Acapulco, Mex.	6	21	1837	−8·9	+3·1	+3·8
Panama, New Granada.	7	13	1787	−7·9	+1·9	+1·8
San Blas, Mex.	5	10	1848	−9·0	+1·3	---
San Diego, Cal.	6	6	1925	−14·2	−1·9	−1·7
Monterey, Cal.	8	21	1903	−16·4	−2·0	−1·5
San Francisco, Cal.	12	9	1893	−16·6	−1·3	−0·7
Cape Disappointment, W. T.	5	12	1932	−23·5(?)	−3·4	−3·1
Sitka, Alaska.	10	18	1855	−29·1	+1·8	+2·9
Unalaska Island, Alaska.	8	17	1835	−20·2	+3·4	+4·4

The probable errors given above will serve to convey some rude idea of the relative value of *each series* of observations; the imperfections in the instrumental means of the older observations in many cases react unfavorably on the modern observations made with more precise instruments; the observations, for instance, taken by Hudson in 1609, in the vicinity of New York Bay, are fairly chargeable with a probable error of ±3° (a single result); those taken by Vancouver on our western coast, between 1792 and 1794, are subject to a probable uncertainty of ±1° (each). Increased precision was obtained with the improvements of the azimuth compass and the allowance for disturbing effect of the ship's iron, and, for shore stations, with the introduction of the theodolite for determining the astronomical meridian. With a portable magnetometer (collimator) the *instrumental* means need not leave a greater uncertainty than about one minute, but the *actual probable error* of any determination is limited by the accidental variations in the mean directions of the magnetic force from day to day, making it desirable to continue the observations for three or more days and to correct them for daily variation. It is principally dependent on the magnitude of the horizontal force.

A cursory examination of the column containing the epochs of greatest *easterly* excursion, the *deflecting* force producing the secular change attaining then an easterly maximum, shows that the needle became stationary in direction and then reversed its secular motion in the New England States toward the end of the past century, in the Atlantic Coast States, to the west and south, early in the present, and in Mexico about the close of the first third of the present century. In California, Oregon and Washington Territory it has not yet reached this condition. We thus have the following epochs for comparison: Halifax about 1711; Portland, Portsmouth, Newburyport, Salem, Boston, Cambridge, Nantucket and Providence about 1799; Hartford, New Haven, New York, Hatboro, Philadelphia, Washington and Cape Henry about 1800; Charleston, Savannah, Key West and Havana about 1800; New Orleans about 1831; Vera Cruz. Mexico, Acapulco and San Blas about 1837; San Diego, Monterey and San Francisco, expected about 1907 (yet very uncertain). We are thus directed to the extreme northeastern States for probable indications of what may be expected to follow on the seaboard in more southern and western States. Respecting the secular movement of the needle, apparently a little more than a century passed before the influence which produced the turning of the north end of the needle westward in Maine (increasing there the western declination) was felt in Lower California (diminishing there the eastern declination); in California, Oregon and

Washington Territory the eastern declination is at present still increasing, but with a losing rate. By the time the western elongation of the secular change is reached in Maine we may expect to see the needle in the opposite phase, or at its eastern elongation, in California. We cannot as yet follow this influence directly over the interior of the United States for want of early observations; the westernmost interior stations for which an epoch could be made out were Buffalo. Erie, Cleveland and Detroit; these give the average turning epoch 1794. It may be quite practicable hereafter to trace out curves uniting all stations where the needle was stationary at a given epoch and again at other epochs for regular intervals of time, say of 10 or 25 years.

Returning to the first table, the constant in each formula would represent the normal direction of the needle, about which the secular change would be performed in an average cycle of about 270 years, and with extreme deflections on either side of it, equal to the co-efficient of the periodic term—all under the *supposition* that the law of the secular movement was truly expressed. It is no doubt much more complex, and besides, may fail at any time; yet as far as our present experience reaches, and within the interval when the first reliable observations were made to the present time, it is found trustworthy.

Table of Decennial values of the Magnetic Declination computed from preceding equations.

These tables have been constructed to facilitate the reduction of observed declinations from one epoch to another; they will be found specially useful, when old lines run by compass have to be retraced and for the construction of isogonic charts for a given epoch.

Blanks occurring in the table indicate either no or doubtful values for the corresponding times; values given to the nearest tenth of a degree are less reliable than those given to hundredths. A + sign indicates west, a – sign east declination.

TABLE IV.

Year (Jan. 1st).	Halifax, N. S.	Quebec, Can.	York Factory.	Portland, Me.	Burlington, Vt.	Rutland, Vt.	Portsmouth, N. H.	Newburyport, Mass.	Salem, Mass.	Boston, Mass.	Cambridge, Mass.	Nantucket, Mass.	Providence, R. I.	Hartford, Conn.	New Haven, Conn.	Albany, N. Y.	Oxford, N. Y.	Buffalo, N. Y.	Erie, Pa.	Cleveland, O.	Detroit, Mich.
1640	...	+15·9	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
1650	...	16·4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
1660	...	16·5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
1670	...	16·4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
1680	...	15·9	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
1690	...	+15·2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
1700	...	...	...	...	...	...	...	...	...	+10·0	+9·8	...	...	...	...	...	...	...	...	...	...
1710	...	...	...	...	...	...	...	...	...	9·3	9·2	...	+10·4	...	...	...	...	...	...	...	...
1720	...	...	+18·6	...	...	...	...	...	...	8·7	8·7	...	9·5	...	...	...	...	...	...	...	...
1730	...	...	19·2	...	...	...	...	...	...	8·1	8·3	...	8·9	...	...	...	...	...	...	...	...
1740	...	...	18·8	...	...	...	...	...	...	7·6	7·9	...	8·4	...	...	...	...	...	...	...	...
1750	+12·5	...	17·2	...	...	...	...	...	...	7·2	7·5	...	7·7	...	...	...	...	...	...	...	...
1760	13·0	...	14·8	+8·1	...	...	...	...	...	6·8	7·2	...	6·9	...	+6·1	...	...	...	...	...	...
1770	13·7	...	11·6	8·1	...	...	+7·8	+7·0	...	6·7	7·0	+6·5	6·3	...	5·5	...	...	...	...	...	...
1780	14·4	...	7·9	8·2	...	+7·0	7·7	7·0	+6·4	6·6	6·9	6·5	6·12	+5·4	5·1	...	...	...	...	...	...
1790	15·1	...	4·0	8·5	+7·7	6·5	7·8	7·2	6·2	6·7	6·9	6·7	6·24	5·2	4·8	...	+3·01	+0·14	−0·7	−2·2	...
1800	15·9	...	+0·1	8·9	7·52	6·2	8·0	7·4	6·2	7·0	7·1	7·0	6·37	5·16	4·7	...	2·96	−0·01	−0·7	−2·0	−3·18
1810	16·7	+11·2	−3·3	9·4	7·39	6·14	8·4	7·8	6·5	7·4	7·5	7·4	6·45	5·24	4·8	+5·4	3·10	+0·05	−0·6	−1·8	−3·11
1820	17·4	12·3	−6·1	10·0	7·58	6·39	8·8	8·4	7·0	7·9	8·0	7·84	6·73	5·46	5·0	5·79	3·40	0·30	−0·3	−1·5	−2·90
1830	18·1	13·4	−8·1	10·6	8·17	6·90	9·35	9·0	7·8	8·43	8·64	8·38	7·43	5·80	5·43	6·32	3·87	0·74	+0·03	−1·05	−2·55
1840	18·7	14·4	−9·0	11·23	8·94	7.64	9·94	9·6	8·7	9·05	9·33	8·95	8·31	6·24	5·99	6·97	4·46	1·33	0·44	−0·60	−2·09
1850	19·3	15·3	−8·8	11·82	9·62	8·53	10·55	10·23	9·8	9·69	10·03	9·53	9·09	6·77	6·67	7·70	5·14	2·05	0·91	−0·14	1·56
1860	19·8	16·0	−7·5	12·35	10·21	9·53	11·15	10·83	10·9	10·32	10·67	10·06	9·65	7·36	7·41	8·47	5·89	2·85	1·39	+0·31	−0·99
1870	20·1	+16·4	−5·3	12·80	10·97	10·54	11·7	11·4	11·9	10·90	11·21	10·54	10·21	7·99	8·18	9·2	6·65	3·68	1·87	+0·72	−0·41
1880	+20·3	...	...	+13·13	+11·97	+11·49	+12·2	+11·8	+12·8	+11·41	+11·63	+10·93	+10·94	+8·62	+8·9	+9·9	+7·38	+4·49	+2·31	+1·07	+0·13

TABLE IV—*continued.*

Year (Jan. 1st).	New York City.	Hatboro, Pa.	Philadelphia, Pa.	Washington, D. C.	Cape Henry, Va.	Charleston, S. C.	Savannah, Ga.	Key West, Fla.	Havana, Cuba.	Kingston, Jamaica.	New Orleans, La.	Vera Cruz, Mex.	Mexico, Mex.	Acapulco, Mex.	Panama, New Gran.	San Blas, Mex.	San Diego, Cal.	Monterey, Cal.	San Francisco, Cal.	Cape Disappointment, W. T.	Sitka, Alaska.	Unalaska Isl'd, Alas.
1640	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	----	----	----	----	----	---
1650	---	--	---	---	---	---	---	---	---	---	---	---	---	---	---	---	----	----	----	----	----	---
1660	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	----	----	----	----	----	---
1670	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	----	----	----	----	----	---
1680	+8·8	+8·5	---	---	---	---	---	---	---	---	---	---	---	---	---	---	----	----	----	----	----	---
1690	8·8	8·3	---	---	---	---	---	---	---	---	---	---	---	---	---	---	----	----	----	----	----	---
1700	8·5	7·9	+8·7	---	---	---	---	---	---	---	---	---	---	---	---	---	----	----	----	----	----	---
1710	8·0	7·5	8·3	---	---	---	---	---	---	---	---	---	---	---	---	---	----	----	----	----	----	---
1720	7·6	7·0	7·8	---	---	---	---	---	—4·2	...	—3·4	---	---	---	---	---	----	----	----	----	----	---
1730	7·2	6·3	7·0	---	+4·9	---	---	---	—4·5	—6·2	—3·7	—2·5	---	---	---	---	----	----	----	----	----	---
1740	6·6	5·6	6·2	---	+4·2	—3·1	---	---	—4·8	—6·4	—4·1	—3·6	---	—2·9	---	---	----	----	.	---	----	---
1750	5·9	4·7	5·3	---	---	—4·1	---	---	---	---	—4·7	—4·7	---	—3·8	---	---	----	----	----	----	----	---
1760	5·2	3·8	4·4	---	---	—4·6	---	---	---	---	—5·3	—5·8	—4·7	—4·7	--	---	----	----	----	----	----	---
1770	4·6	2·9	3·6	---	---	—4·9	---	---	---	---	—5·9	—6·8	—5·5	—5·6	—7·7	---	----	----	----	----	----	---
1780	4·4	2·2	2·9	---	---	—5·1	---	---	---	---	—6·5	—7·7	—6·4	—6·4	—7·8	---	----	----	----	----	----	---
1790	4·29	1·8	2·4	—0·1	---	—5·1	---	---	---	—6·3	—7·0	—8·5	—7·1	—7·2	—7·8	—7·4	—11·0	—11·4	—12·8	—18·0	----	—18·6
1800	4·28	1·8	2·1	0·0	+0·2	—4·9	---	---	—6·2	—6·0	—7·5	—9·1	—7·7	—7·8	—7·8	—7·9	—11·1	—12·0	—13·4	—18·1	—26·12	—19·3
1810	4·30	2·03	2·1	+0·3	0·0	—4·5	—4·9	---	—6·26	—5·7	—7·9	—9·5	—8·3	—8·3	—7·7	—8·3	—11·3	—12·6	—13·9	—18·4	—27·11	—19·7
1820	4·47	2·53	2·28	0·6	0·0	—4·04	—4·8	—6·9	—6·22	—5·4	—8·1	—9·6	—8·6	—8·68	—7·5	—8·6	—11·6	—13·3	—14·42	—18·7	—27·89	—20·1
1830	4·91	3·17	2·71	1·0	+0·25	—3·44	—4·5	—6·52	—6·12	—5·0	—8·2	—9·6	—8·8	—8·88	—7·2	—8·8	—11·9	—13·9	—14·92	—19·2	—28·48	—20·2
1840	5·59	3·86	3·33	1·49	0·66	—2·78	—4·14	—6·03	—5·94	—4·6	—8·14	—9.4	—8·9	—8·91	—6·96	—8·94	—12·2	—14·44	—15·38	—19·72	—28·88	—20·2
1850	6·34	4·57	4·11	1·99	1·24	—2·12	—3·65	—5·47	—5·71	—4·2	—7·94	—8·9	—8·76	—8·79	—6·66	—8·97	—12·54	—14·95	—15·78	—20·29	—29·08	—20·1
1860	6·96	5·29	4·99	2·47	1·95	—1·52	—3·08	—4·86	—5·44	—3·8	—7·61	—8·25	—8·48	—8·50	—6·33	—8·89	—12·88	—15·42	—16·11	—20·88	—29·08	—19·7
1870	7·43	6·0	5·89	2·90	2·73	—1·00	—2·48	—4·24	—5·2	-3·4	—7·15	—7·4	—8·04	—8·06	—6·0	—8·7	—13·20	—15·79	—16·36	—21·46	—28·88	—19·3
1880	+7·84	+6·8	+6·76	+3·26	+3·5	—0·62	—1·89	—3·65	...	...	—6·62	...	—7·46	—7·5	...	...	—13·50	—16·08	—16·52	—22·00	—28·5	—18·6

ART. V.—*On the Tails of Comets*; by HENRY M. PARKHURST.

[Read before the Amer. Assoc. for the Adv. of Science, Aug. 18, 1874.]

IN predicting the form and position of a comet's tail, I adopt the theory of Professor Pierce:

"Each particle of the matter which composes the tail is supposed to move in a hyperbolic orbit, with the sun in the focus of the opposite branch, under the influence of a repulsive force emanating from the sun, and decreasing by the law of the inverse square of the distance." (Gould's Astronomical Journal, vol. v, page 186.)

The application of the formulæ gives for any required time one line only, commencing at the comet and extending indefinitely. Professor Pierce adopted, in his computations upon the tail of Donati's comet (Gould's Astr. J., vol. vi, page 51), an excess of repulsive force $1\frac{1}{2}$ times that of gravitation, making his comparisons with the front edge of the tail, and arbitrarily adopting such a repulsive force as best to explain the observations. For the purpose of comparing with the center of the tail, I adopted in my computations a repulsive force exactly equal to and replacing gravitation as the more probable law. On comparison with the observations, however, I find that the computed line closely agrees in nearly every instance with the observed right hand edge of the tail; and I shall, therefore, adopt that as the standard of comparison. Although the comet apparently moved in the other direction, yet in fact that was the front edge of the tail.

Most of the earlier observations were furnished to me by W. S. Gilman, Jr., of New York City, and all the later ones by Lewis Swift of Rochester, whose observations continued several days after the comet had ceased to be visible in New York City.

From June 12 to June 30, I have only the record of the direction of the tail, without reference to stars (G), agreeing with computation. On July 1, the tail "pointed to and reached 55 of Camelus." (P.) The computed line passes nearly through that star. On July 7, the front edge was seen a little to the left of 63 Arg. 749. (Bonn Catalogue.) (P.) The computed line passes a small fraction of a degree to the left of the star. On July 13, the last day when the head of the comet was favorably seen, "the right edge just touched *o* Ursa Major" as seen by Mr Swift, and passed $\frac{1}{2}$° to the left of it according to my own record. The computed line passes $\frac{3}{4}$° to the left of it The curvature on this evening was commonly noticed. Computation indicates an arc of about 15°. On July 14, "*h* U. Maj. was in the middle of the tail." (S.) Allowing for the recorded width, it should be in the middle of the tail. On July 17, "v was exactly in the center," and "its left edge just touched α." (S). These are each within ½°. On July 18, "it passed over α," (S.) and α U. Maj. was within the range of the computed tail. "θ and F (φ) were not only in the center of the tail but in line with its axis." (S.) They were within ¼° of the computed center, and very nearly in the computed line of its axis at that point. On July 19, "the tail passed midway between α and β, but touched neither." (S.) The computed line passes nearly centrally between them. "Its left edge just touched δ." (S.) Here is an inaccuracy; for it could not have touched δ without passing over β. On July 20, it passed "midway between β and γ U. Maj., and centrally over δ." (S.) This agrees with computation. On July 21, the last day of its visibility, γ U. Maj. was seen "at its right edge, perhaps 1° within it." (S.) The computed line is 1° to the right of γ. "λ and μ U. Maj. were seen in the center of luminosity." (S.) This agrees exactly with computation. "No curvature perceptible." Computation indicates that the tail increased its curvature until about the 13th, a few days earlier the tail being too short for it to be perceptible, and a few days later the curvature itself becoming too small to be perceptible.

In all these cases the accordance of the right hand edge of the tail with the computed line was as close as the nature of the observation would admit. But among the observations kindly furnished me by Mr. Gilman, the accuracy and faithfulness of which I cannot doubt, are two which I cannot in fairness omit. On July 3, he recorded the place of the tail as wholly to the right of the two stars 65 Arg. 606, 607. The computed line passes to the left of those stars. As he did not use a diagonal prism, and inverted his chart in making the comparison, it seems impossible that he should have put it upon the wrong side, especially as his attention was also directed to the star 65 Arg. 599, near the other edge. On July 6, he recorded the place of the tail as passing through the star 63 Arg. 749, the chief star of an unmistakable group. The computed line passes nearly a degree further east. On this diagram you will see that the observed lines, before and after, are nearly parallel, and that these two widely deviate. You will also see, that in each case, connecting the observed point in the tail with the position of the head on the previous evening produces a line parallel to the rest; and although Mr. Gilman himself does not conceive it to be possible that he connected the position of the tail on one day with that of the head upon another, or that he could have even seen the comet on July 5, which was Sunday, I can find no other satisfactory solution of the discrepancy; and that explanation would substitute two observations accurately agreeing with the computations, for two which are entirely inconsistent with the remainder of the series. Perhaps I should add that these observations, and indeed all the observations but one, were made without any expectation that they would ever be of value.

In predicting the width of a comet's tail, it may be assumed to bear a certain ratio to its length. Up to the time of the disappearance of the nucleus, the ratio of one-sixth was sufficiently exact. The tail was then approaching us, and on this account would be expected to appear to grow wider as well as longer. Assuming that it remained unchanged in form, it should have been about six times as wide on July 21 as on July 13. But instead of being 18° wide, it was seen to be only "4½°, possibly 5°."

I have refrained from alluding to an important point with regard to the front edge. When the tail is coming directly toward us, the front edge becomes the medial line. When the tail is 4° wide, assuming the right hand edge to be the front edge would involve an error of 2°; or, if the tail were 18° wide, as computed, it would involve an error of 9°. There is no such error; therefore, there is no visible portion of the tail to the right hand of the true front edge. If the tail is flat, lying in the plane of its orbit, we may readily understand why it should be so foreshortened when directed toward us. Indeed, this proves too much; for if it were as thin as Saturn's ring, it should have appeared as a mere line instead of being 5° wide. If the form of the section were elliptical, it would account for the observed width on the 21st of July; but when the tail was first seen, the earth was fully 11° above its plane, as seen from the comet, and the tail would have appeared much narrower in all the observations before July 13. The dark line behind the nucleus cannot indicate a hollow conical tail, for a diminution of the central light by $\frac{1}{18}$th part would hardly be perceptible; but that dark line, and the dark lines separating the different envelopes, are perfectly consistent with the theory of a thin flat tail.

There is another important fact. The front edge was toward the right until July 20; but at noon of July 21 the earth passed through the plane of the orbit. On the evening of July 21, therefore, when the tail was 5° wide, the front edge should have been either at the left hand, or at any rate at least 2° from the right hand edge; and yet the latter agreed with the line computed from the same formulæ with those of all the preceding days; and although the tail was seen for five days in succession, during which the earth passed through the plane of the orbit, there is no apparent discrepancy between the computed line and the observed position of the *right hand* edge of the tail.

A diagram I have constructed illustrates several results necessarily depending upon the hyperbolic theory. The black line represents the orbit of the comet of 1843, so far as included within the radius of the earth's orbit. The red lines, starting from the orbit, and after passing their perihelion points radiating in nearly straight lines from the sun, represent the hyperbolic orbits of particles of matter, leaving the head of the comet at the given times.

In these computations, also, I have adopted 1 as the ratio of repulsion. Had Coggia's comet possessed sufficient luminosity to be visible on July 24, it would have afforded a test of the true amount of repulsive force; but as its plane was still turned almost directly toward us when the tail was last seen, a variation in the assumed amount of the repulsive force makes no appreciable difference in its position; and the streamers seen extending from the convex side of the tail of Donati's comet suggest the theory that upon every formation of a new envelope, a small portion of the matter was endowed with a repulsion at least ten times that of the rest of the matter forming the tail.

The blue lines circling around the sun connect the positions of the particles in their hyperbolic orbits at given times, and therefore represent the visible and the invisible tail of the comet. The particles leaving the head of the comet thirty days before it reaches its perihelion, follow nearly behind it, with continually retarded motion, and when the head of the comet reaches the sun, those particles have only passed over half that distance, and are just about to commence their outward course. In receding from the sun the motion is gradually accelerated until the particles reach the same distance from the sun at which they left the head. The moment they pass outside of that point they are irrevocably lost. A reconversion of the repulsive force into attraction would not avail, but they must fly, unless intercepted, beyond the confines of our solar system.

Particles leaving the head of the comet at a later date pursue a somewhat similar course, coming nearer to the sun, but all at about the same time turning and flying off into space. The nearer they come to the sun before commencing their hyperbolic course, the more violent is the repulsion, and the greater their outward velocity. The particles which leave the head of the comet exactly at the perihelion, pursue a line almost directly from the sun, but about 66° from the axis of the comet's orbit. These particles commence their outward course with a velocity sufficient to carry them outside the earth's orbit in a little over two days; and that velocity remains nearly uniform in consequence of the sudden removal of the matter beyond the sphere of the sun's forcible action. Consequently, when we undertake, for the 30th day after the perihelion passage, to trace the entire tail which has left the head during the preceding 60 days, we must pursue a curve commencing at the position of the head of the comet, passing to the right and downward, crossing the perihelion line at a distance from the sun one-third greater than that of the planet Neptune, and re-entering the earth's orbit above.

How much of this will be visible? As we follow the tail from the head it becomes fainter, first, because it is more distant from the sun, and therefore less illuminated. Another reason is that it is more diffused from its rapid expansion like the fire from a catharine wheel, the portion of the tail forming a certain arc, continually forming nearly the same arc of a larger circle. But is that all? Here is a portion of the tail above, more illuminated than the head of the comet itself, and which occupies less than one-fourth the space traveled over by the comet while it was issuing.

While the dark line in the center of a comet's tail seems to disprove the theory naturally suggested by the fact of the front edge being the line of computation, that the resistance of the ether is the cause of the expansion of the tail, the ether may have a perceptible effect upon its form and position. Had the sun an ether of its own, carried with it through that still more rare ether which brings to us the pulsations of light, the effect of the ether upon the tail would be only that resulting from the motion of the comet itself; but leaving the ether behind it in its course toward the constellation Hercules, it will tend to sweep the tail back from that point: and here we may find a possible explanation of the apparent wafting of the tail out of the plane of its orbit, leaving the front edge still on the right, and causing a thin, flat tail to have a visible width tenfold greater than it would have had if strictly in the plane of the orbit.

Note.—Observations in England on July 9, 14, 17, 18, 19 and 21, confirm the positions above given, but are not sufficiently definite for exact comparison. The theory of Prof. Norton (Am. Jour. Sci., 1861), that the width of the tail arises from the variation of the repulsive force exercised upon different particles of the cometary matter, and its thickness from the repulsion of the nucleus itself at the time of emission, I had not seen when the above was written. I have computed a new line consisting of particles equally repelled and attracted, and I find that it nearly corresponds with the former computed line, or with the observed right hand edge of the tail, apparently indicating that the tail was, from some cause, chiefly south of the plane of the orbit. The theory of Prof. Norton affords a plausible explanation of that cause.

New York, Oct., 1874.

ART. VI.—*The very much extended Nebulæ of Sir John Herschel General Catalogue;* by CLEVELAND ABBE.

[Read before the Philosophical Society of Washington, June 4, 1874.]

In 1865 and 1867, I considered carefully the question of t apparent distribution of the nebulæ in space, basing my studi upon the General Catalogue of Sir John Herschel. Among oth points that occupied my attention, I attempted, by separatin the several general classes of nebulæ and clusters, to render mo definite our ideas as to the different laws that obtain in referenc to the distribution of resolvable and unresolved nebulæ.

Some of the conclusions then arrived at were published i the Monthly Notices of the Royal Astronomical Society fo 1867, and have been in part confirmed by the subsequen writings of others. Mr. Proctor has rendered good service t this branch of Astronomy by reproducing on equal surfac charts the table given by me showing the distribution of th unresolved nebulæ; which charts, published first in the Monthl Notices, and republished in his "Essays on Astronomy" an his "Universe," as well as the more elaborate and beautif charts of Messrs. Waters and Proctor, are well adapted to co vey such information as can be gained from this study. Bu whether we gauge the stars or the nebulæ, at best we can onl obtain results that are exceedingly unsatisfactory, and perhap no two persons would agree in adopting the same conclusion on this subject. In hopes of adding something more exact t our knowledge of the construction of the universe, I sough two years ago, to resolve several more definite problems, suc as involve fewer assumptions, and one of which I will no present, premising that the subject is brought to your notic not because I think myself to have obtained anything conclusive, but because the problem seems to direct attention to a poin worthy of special observation. In fact, the material I have ha to work with has been so unsatisfactory that it will be a sufficien reward if I am able to so attract the attention of those possessing powerful telescopes as to induce them to revise the roug measurements that I have been obliged to content myself wit

The general problem, as it has presented itself to me, ma be thus stated: We have thus far studied the distributio in space of the centers of the nebulæ,—are there not plane that have a definite relation to these bodies? Among the star we have the Milky Way and the orbits of binary stars; amon the planets we have their orbits and equators, and the orbits their satellites; among the nebulæ we may expect to find analogous planes, whose relations to each other and to those alread known cannot but be highly instructive. As yet we have bu very few double nebulæ, nor can we for a long time hope to determine the planes of the orbits of any binary nebulæ, if such exist; on the other hand, in regard to the axes of rotation of nebulæ or the planes of their equators, there is more room for study. It is a plausible hypothesis that some nebulæ are in rotation about their respective axes, and only in the case of an irregular nebula do we find presumptive evidence of numerous centers of aggregation and rotation; this latter class will not now further claim our attention. Those nebulæ whose whole mass is rotating about a single axis must appear to us either circular or elliptical, according to our position in relation to that axis; we might then at once assume that every well defined circular nebula has its axis of rotation directed toward us, and might thus determine the position of the plane of its equator; this, however, would be hazardous, since not only do we thus assume the fact of a rotation, but also assume that the average rotation of all nebulæ is so rapid that when viewed from any other direction they would present a sensibly elliptical outline; moreover, often globular clusters of stars are recorded as circular or globular nebulæ. I have therefore, for the present, passed by the circular and the ordinary elliptical nebulæ, and have confined myself to those described in Herschel's Catalogue as exceedingly or very much extended; the list of these, amounting in all to fifty-nine, is given in the accompanying table. In regard to them it may be remarked that if these nebulæ are gaseous and without rotation, we can only explain their apparent shape by supposing them to be endowed with a motion of translation, to be in fact wisps, like comet's tails; if, on the other hand, they be in a state of rotation, they must be flat rings or discs or extended flattened ellipsoids, and we are authorized to consider that the planes of their equators do sensibly pass through the position of the observer; similarly, if the nebulous appearance be due to the presence of lenticular or ring-shaped cloud of asteroids, or of meteoric dust, we shall be able to make a determination of the plane of the orbits of these bodies.

The trigonometrical formulæ by which we may compute the right ascension and declination of the poles of a very much extended nebula are given in the following note.

It is evident that the principal elements of uncertainty are, first, the inaccuracy of the adopted angles of position (π), and second, the error of the assumption that the planes of the nebulæ in question are not inclined to the line of sight. The latter error may, on the average, amount to 5° or 10°: the errors of the π may be averaged as about 5°, 15°, or 30°, according as the values are given in Herschel's Catalogue as exact, approximate or rough: the influence of these errors on our results can be easily ascertained by means of the differential formulæ.

Having computed the right ascensions and declinations of the south poles of the fifty-nine nebulæ in question, as given in columns six and seven of the accompanying table, I have also plotted them upon equal surface charts similar to those used by Messrs. Proctor and Waters, on which also have been drawn the limits of the Milky Way as given by those same gentlemen, according to Heis and Herschel. Owing to the fact that the unresolved nebulæ are, as a rule, far more numerous near the poles of the Milky Way than elsewhere, it follows at once that the greater number of the nebulæ now under consideration are near these poles, and therefore our poles of rotation, if we may presume to use that term, lie near the Milky Way itself: but a careful enumeration shows us that in the northern hemisphere these poles lie to the southward of the central portion of the Milky Way, while in the southern hemisphere the reverse holds good; in fact, there exists a medial plane about which the poles of these nebulæ cluster, and which is itself inclined to the plane of the Milky Way at an angle of about 30°, so that if the north pole of the Milky Way be in right ascension 12 h. 45 m. and declination 30°, the pole of the plane near which the rotation axes of the nebulæ lie will have about the same right ascension, but a declination of about 60°. Numerically expressed, this latter plane is so situated that of fifty-nine nebulæ twenty-nine have their axes inclined to it by less than 10°, and forty-two have their axes inclined by less than 20°.

It is, I conceive, quite desirable that we should, on the one hand, have more accurate determinations of the position angles of these extended and ray-like nebulæ, and that, on the other hand, the reversion spectroscope of Zöllner should be applied to determine whether or no they be really in a state of rotation.

By adopting some average value for the oblateness of the spheroids, that appear to us as elliptical or oval nebulæ, it will be possible to obtain a certain approximate estimate of the probability that the plane here determined has some bearing upon the general question of the arrangement of the three or four thousand known nebulæ (the clusters being included); but such a computation involves too much of hypothesis to be of interest at present.

It may then in general be stated, that so far as we are able to determine the positions of planes of rotation among the nebulæ, they do not show any such tendency to agree with each other as is shown in the orbits and equators of the major planets of the solar system; that, on the contrary, they are inclined at all possible angles to each other, but have this remarkable feature, that their mutual nodes cluster about a point in right ascension 12 h. 45 m. and north declination 60°.

The Positions of the Poles of the "very much" and "exceedingly" "extended nebulæ" of Sir John Herschel's "General Catalogue," referred to the mean Equinox of 1860.

The Nebula.						Description.		Its South Pole.		
No.	R. A. h.	m.	N. P. D. °	′	π			R. A. h.	m.	N.P.D. °
67	0	23·5	124	1·8	47·5	vB: L; vmE; psbM; f of 2	2†	8	29	127·7
132	0	40·1	111	30·9	172	F: vL; vmE.	2*	18	28	97·3
138	0	40·6	116	3·7	54·5	!!; vvB; vvL; vmE; gbM.	9*†	8	46	136·9
191	0	57·4	156	21·5	145·4	eF: vmE; vlbM.	1	16	48	103·2
361	1	27·8	120	7·6	118·3	vB; vL; vmE; sbM.	5	16	35	139·7
400	1	41·6	84	47·2	165 ±	vF; vmE; sbM.	3†	19	47	104·9
527	2	13·8	48	16·8	22·3	!; B; vL; vmE.	5†	7	12	106·4
610	2	40·3	120	51·5	151·1	vB; L; vmE; vbMN.	3	19	37	114·5
662	3	5·9	143	52·3	80	B; L; vmE; vgbM.	2	14	16	125·5
776	3	39·1	135	5·3	221·6 =41·6	pB; L, vmE.	1	11	47	117·9
779	3	39·9	135	5·4	42·3	pF; pL; eE; vgpmbM.	3	11	52	118·3
811	4	0·6	144	29.4	10	B; L, vmE; bM.	2	10	33	95·8
823	4	6·6	123	14·1	32·2	B; vL; vmE; psmbM.	3†	11	22	116·4
1035	5	4·9	149	54·2	162	vF; pL; vmE.	2	22	2	98·9
1437	6	31·5	81	8·3	330 =150	B; vmE; Noom=*11.	7†	0	51	119·6
1472	6	49·4	130	41·5	44·8	pB; pL; vmE, pslbM.	2	15	1	122·3
1674	8	27·2	112	29·8	110·3	cB; L; vmE;	5	23	23	150
1713	8	44	56	3·6	40·9	vB; vL; vmE; gmbM.	5	13	1	122·9
1745	8	55·5	135	20·9	19	!; eeF; vL; vvmE.	1†	15	50	103·3
1823	9	12·3	38	25·6	150·8	vB; L; vmE; vsmbM=*10.	3	4	46	107·7
1931	9	40·3	55	56·2	90	!; cB; L; vimE.	3†	9	40	145·9
1958	9	46·4	87	56	111·5	vF; vL; vmE.	3	4	7	158·5
2003	9	56·7	115	29·4	82·3	cF; vL; vmE; lbM.	2†	20	46	153·5
2008	9	58·2	97	2·5	45	vB; L; vmE; vg, vsmbMEN	3	16	26	134·6
2238	10	44·6	52	38·5	42·5	pB; pL; vmE; ⁑ inv?	3*	14	48	122·5
2318	11	3	33	34·7	79	cB;vL; vmE; pbM; r.	3	11	55	122·9
2378	11	12·9	75	38·5	102	pB; vL; vmE.	8†	8	31	161
2413	11	18·4	45	38·5	0 ±	vB; cL; vmE; vsmbMN.	4	17 or 5	18	90
2595	11	45·3	116	7·4	59·3	vF; cL; vmE.	1	20	11	140·4
2715	11	59	39	38·8	166·5	pB; vL; vmE; vgvlbM.	3	6	41	98·6
2749	12	2·9	42	46·3	109	pF; cL; vmE; vgbM.	3	10	23	130·0
2761	12	4	38	43·9	60 ±	pF; cL; vmE.	4	14	30	122·8
2786	12	6·7	74	19	152·1	B; vL; vmE; vsvmbM.	7	6	39	116·8
2806	12	8·8	76	4	17 ±	vB; vL; vmE; sbMN.	5†	17	52	106·5
2831	12	10·5	51	24·7	43·2	pB; vL; eE; vgbM.	3	16	8	122·4
2877	12	14·6	74	36·9	0 ±	L; vmE; f of 2.	4	18 or 6	14	90
3066	12	26	89	8·9	95 ±	cB; vL; vmE; B* in cont.	3	7	4	175
3132	12	32·7	100	50·2	92	!; vB; vL; eE; vsmbMN.	3†	1	15	169·2
3142	12	33·7	27	36·8	118·6	B; L; vmE; glbM.	2	10	27	114
3189	12	37·1	57	3·8	34.3	!; pB; L; vmE; sp of 2.	6*†	17	15	118·2
3240	12	42·6	74	4·3	28.5	cB; pL; vmE; sbMN.	4†	18	8	117·3

The Nebula.				Description.		Its South Pole.		
No.	R. A. h. m.	N. P. D. ° ′	π			R. A. h.	m.	N P.D. °
3278	12 45·9	78 0·6	34	pB; vmE; 3 B st s; f of 2.	6†	18	14	123·2
3321	12 49·9	67 33·3	120 ±	!; vB; vL; vmE; bMBRN=* ?	10†	9	3	143·3
3340	12 52·4	54 23	30 ±	vF, pL; vmE; bet 2 st.	5†	17	38	114
3386	12 57·2	138 32·1	38.7	B; vL; vmE.	1	21	1	114·5
3437	13 4·5	52 11·2	25	vB, vL; vmE; vsbMN.	4	18	0	109·5
3525	13 17·3	132 17·2	122·5	!!; vB; vL; vmE; bifid.	4†	4	11	128·6
3696	13 47·9	83 58·5	15	F; pL; vmE; r.	7	19	40	104·9
3761	13 57·5	40 9·3	90 ±	pB; cL; vmE; smbMN.	2	13	57	130·2
3822	14 9·7	53 7·6	110·3	cF; pL; vmE; vgvmbM.	2	11	58	138·5
4004	14 47	85 53·1	148·4	F; pS; vmE; gvlbM.	4	8	57	121·5
4086	15 12·–	32 10·–	155	A ray; vmE, par. to 4087.	0	10	38	103
4087	15 12·2	33 10·1	155	cB; vL; vmE; vg, psbMN.	3†	10	38	103·4
4614	20 49·5	146 6·4	0	eeF; vS; vmE; *13 att, n.	2	2 or 14	49	90
4663	21 19·2	143 23·3	90·8	eF; pL; vmE.	1	9	23	126·7
4679	21 26·6	145 10·8	127·1	pB; pL; vmE: g, palbM.	1	12	19	117·1
4830	22 36·5	120 47·1	0	F; pL; vmE; vgvlbM.	1	4 or 16	36	90
4860	22 47	130 24·6	43·3	cB; L: vmE; mbM.	2	6	53	121·5
4885	22 54·9	131 34·9	5 ±	cF: ps; vmE.	2	5	7	89·3

Columns 1–6 are copied from Herschel's General Catalogue. The position angles in column four have been micrometically measured when given to the tenth of a degree; have been carefully estimated when given with an appended ±; have been roughly estimated when given to the nearest 10°.

NOTE.—A nebula which seems a mere ray or line being situated at S′, prolong this ray into a great circle, S′S″, whose south pole is p, and join S′ and p with the north pole of the heavens, P. We now have,

$$\cos(d) = -\sin(\delta)\sin\pi$$
$$\tan(a-a) = +\sec(\delta)\operatorname{cotg}\pi$$

where the sign of $\sin(d)$ is positive,
" $\cos(a-a)$ is the same as the sign of $\cos(\delta)$.
" $\sin(a-a)$ " " $\cos\pi$.
π=PS′S″ or the positive angle of the ray measured *n. f. s. p.*
(δ)=PS′.
a=right ascension of south pole of nebula.
(d)=north polar distance of "

For a list of explanations of the meanings of the abbreviations used in the 5th and 6th columns of the accompanying tables, see pp. 11–13 of the original memoir by Herschel, in the Philosophical Transactions for 1874.

Washington, August 1st, 1874.

ART. VII.—*On Venus as a Luminous Ring;* by Prof. C. S. LYMAN.

IN this Journal,* eight years ago, a brief notice was published of some observations made by the writer on Venus when near her inferior conjunction in 1866. The planet was then (for the first time, so far as appears) seen as a very delicate luminous ring. The cusps of the crescent, as the planet approached the sun, had extended gradually beyond a semicircle, until they at length coalesced, and formed a perfect ring of light.

No opportunity has since occurred of repeating these observations until the day of the recent transit. On Tuesday, Dec. 8th, Venus was again in close proximity to the sun, and the writer had the satisfaction of watching the delicate, silvery ring enclosing her disc, even when the planet was only the sun's semi-diameter from his limb. This was at 4 P. M., or less than five hours before the beginning of the transit. The ring was brightest on the side toward the sun—the crescent proper. On the opposite side the thread of light was duller and of a slightly yellowish tinge. On the northern limb of the planet, some 60 or 80 degrees from the point opposite the sun, the ring for a small space was fainter, and apparently narrower, than elsewhere. A similar appearance, but more marked, was observed on the same limb, in 1866.

These observations were made with a five-foot Clark telescope of 4⅔ inches aperture, by so placing the instrument as to have the sun cut off by a distant building while the planet was still visible. The ring was distinctly seen when the aperture was reduced to one and a half inches. The 9-inch equatorial could not be used, as there were no means of excluding the direct sunlight.

The morning after the transit the sky was slightly hazy and the planet could not be found, though probably it might have been if the small telescope had been mounted equatorially.

On the day following (the 10th), the crescent, extending to more than three-fourths of a circle, was seen with beautiful distinctness in the equatorial, and on this and two subsequent days, measurements were taken with the filar micrometer for the purpose of determining the extent of the cusps, and consequently the horizontal refraction of the atmosphere of the planet, on the assumption that the extension of the crescent and formation of the ring are due to this refraction.

The results of these observations are given below, each result being the mean of the number of separate measurements indicated in the last column. On the 10th, the chord of the arc between the cusps was measured; on the other days the distance between lines tangent to the cusps and to the opposite limb.

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* Vol. xliii, p. 129.

Mean dates. Dec.				Dist. of centers of ⊙ and ♀.		Extent of Crescent.		Hor. refr. of ♀'s atmos.	Num. of ob. of cusps.
8	3^h	0^m	P. M.	0°	36′·6	360°	00′		
10	11	36	A. M.	2	31·7	279	28	46′·6	4
11	10	16	A. M.	4	2·5	233	15	43·0	6
11	2	40	P. M.	4	20·4	231	46	45·5	15
12	2	45	P. M.	5	58·3	215	21	42·9	22
						Mean		44·5	

These observations give a mean of 44′·5 as the horizontal refraction of Venus's atmosphere, or about one-quarter greater than that of the earth's. The writer's observations, in 1866, gave 45′·3. Mädler, from observations of the cusps in 1849, when the nearest approach of the planet to the sun was 3° 26′, made the refraction 43′·7.

The formula for the refraction is this:

$$\sin y = \sin d \sin \frac{c-180}{2}. \qquad 2x = y - \frac{r}{\rho}.$$

in which d = distance of centers of ⊙ and ♀.
c = arc of crescent.
r = sun's semi-diameter.
ρ = radius vector of Venus.
x = horizontal refraction of Venus's atmosphere.

Six measurements of the diameter of the planet on the 10th give 63″·1. Twenty-four, on the 11th, give 63″·75. The English and American Almanacs give 62″·4 and 64″·5 respectively.

ART. VIII.—*Discovery of a New Planet;* by Professor JAMES C. WATSON. From a letter to one of the editors, dated Peking, China, October 17, 1874.

I have the pleasure to send you the following places of a new planet which I discovered at 8^h 30^m on the evening of Oct. 10th.

	Peking M. T.			α			δ		
1874, Oct. 10,	13^h	30^m	0^s	0^h	58^m	$1·68^s$	+10°	42′	52·0″
" 11,	9	58	15	0	57	15·48	10	40	33·9
" 11,	10	43	44	0	57	13·70	10	40	22·7
" 11,	11	31	0	0	57	11·75	10	40	24·8
" 13,	8	33	39	0	55	29·90	10	34	56·9
" 13,	9	43	44	0	55	27·67	10	34	38·5
" 13,	9	56	45	0	55	27·22	10	33	47·8
" 14,	9	35	11	0	54	33·71	10	31	57·3
" 15,	7	49	29	0	53	43·70	10	29	10·6
" 16,	8	58	34	0	52	46·46	10	26	15·0

The planet resembles a star of bright 11th magnitude.

ART. IX.—*Ancient Lake basins of the Rocky Mountain Region*; by Prof. O. C. MARSH.

THE existence of several large fresh water lakes in the Rocky Mountain region, during Tertiary time, is now well established, mainly through the researches of explorers whom the striking scenery of the "Bad Lands," or the extinct animals entombed in them, have attracted thither. The geological age of some of these lakes, however, seems to be still in doubt, at least widely different opinions on this point are freely expressed. The extent of these various lake-basins, and their relations to each other, are likewise a subject on which information is especially desirable, and hence the results of some recent observations are here presented.

The deposits left in these old lakes show them to be of Eocene, Miocene, or Pliocene age, the fauna of each formation being entirely distinct, as well as quite different from existing species.

I. *Eocene Lake-basins.*

The oldest of the great Tertiary lake basins of the West are of Eocene age. The one first discovered and best known, which has been called the Green River basin, lies between the Rocky Mountains and the Wasatch range, in the depression now drained by the Green River. It has the Uintah Mountains for its southern border, and extends north at least as far as the Wind River range. This basin was visited by the writer in 1868, but first explored in 1870, when he traced its deposits for several hundred miles, and from the rich vertebrate fauna fully determined its Eocene age.* These same beds have since been pronounced Miocene by Prof. Hayden and others, but the 150 species of extinct vertebrates now known from them prove them Eocene as conclusively as any single formation has yet been determined in this country. A comparison of almost any group of these fossils with the corresponding one from the Paris basin will afford sufficient evidence on this point. Some of the extinct mammals, indeed, indicate a still lower horizon, or one nearly equivalent to the Lignite beds of France.

The Tertiary deposits in the Green River Basin are all of fresh-water origin, and of enormous thickness,—6000 feet at least. They are nearly or quite horizontal, and as a rule rest unconformably on the Cretaceous coal-bearing rocks below. The latter are in part brackish water beds, containing, with some characteristic Cretaceous fossils, many remains of plants, which have led Hayden, Lesquereux, and others to regard them as Tertiary. The evidence from the fossil plants is far from conclusive, while the Cretaceous age of essentially the same beds has been clearly made out at several different localities. At one of these, in 1870, the writer found above a seam of coal *Ostrea congesta* Conrad, a typical Cretaceous fossil, and a crinoid allied to *Marsupites* of the English Chalk. Below the coal, but in the same series, were remains of fishes and turtles, both of Cretaceous types, and teeth of a Dinosaur.* Nearly as conclusive evidence has since been found at other localities. In considering a question of this kind, where the evidence from fossils appears conflicting, it should especially be borne in mind that vertebrates afford a much more accurate guide to climatic and other geological changes than invertebrates, and vastly more so than plants.

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* This Journal, vol. i, p. 191, March, 1871.

This Eocene lake basin remained dry land during all of Miocene time, and perhaps much longer. It was then again submerged for a short period, and its eroded surface covered with water-worn debris from the surrounding mountains. The evidence of this is seen in the coarse conglomerate crowning the highest buttes, which have thus escaped in part the enormous denudation most of the deposits in this basin have suffered.

South of the Uintah Mountains, a second and larger lake existed in the Eocene. It was 2,000 feet or more lower than the northern lake, and received part of its waters from that source. It had the Rocky Mountains for its eastern border, the Wasatch range on the west, and extended from the Uintahs far to the southward, doubtless quite to the present territory of New Mexico. This basin was first explored, and its Eocene age established, in 1870, by the writer, who finding it distinct from the Green River basin on the north, named it the Uintah basin.† These two lakes were contemporaneous, for a long period at least, and undoubtedly were connected together east of the present Green River cañon. There is evidence that the southern lake continued for some time after the northern one dried up. The deposits of the Uintah basin are of very great thickness, and are underlaid by Cretaceous beds, in some places much inclined.

The fauna entombed in both these Eocene lakes is essentially the same, and indicates a tropical climate. This is seen especially in the great number of Tapiroid mammals, monkeys, crocodiles, lizards and serpents. Remains of *Dinocerata*, the largest of Eocene mammals, have as yet been found only in the northern basin, and there are some other important differences at present, which future discoveries may remove.

There are indications of another Eocene lake in Eastern Oregon, west of the Blue Mountains, but as only a few plants have hitherto been found in its deposits, its relations in time to the great central Eocene lakes cannot as yet be determined.

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* This Journal, vol. i, p. 195, March, 1871.

† Loc. cit., p. 196.

II. *Miocene Lake-basins.*

The Tertiary lake-basin first made known in the Rocky Mountain region is that south of the Black Hills, so long famous for its "*Mauvaises Terres*" or "Bad Lands," and for its wonderful vertebrate fauna, which Prof. Hayden has done so much to bring to light. This ancient lake, which was comparatively shallow, appears to have extended south from the Black Hills to near where the Republican River now is, or from about the 44th to the 40th parallel of latitude. Its western border was the Rocky Mountains, and its eastern margin probably not far from the 99th meridian. The strata in this basin are all nearly horizontal, and indicate quiet waters. They are lighter in color, and much less arenaceous than those of the Eocene lakes. The best exposures of these Miocene beds are seen near the White River, and this name has very appropriately been used by Prof. Hayden to distinguish the lake basin in which they were deposited. In Northeast Colorado the same formation is well developed. The "Bad Lands" there were discovered and first explored by the writer in 1870.* These Miocene strata rest, with more or less unconformity, on an extensive series of lignite-bearing beds, which in many respects resemble those beneath the Eocene basins. The age of these beds, also, is in dispute, but the remains of Dinosaurs and some other typical Mesozoic vertebrates, which have now been found at many widely separated localities, leaves little doubt that they should be placed in the Cretaceous, the great coal-bearing formation of the Rocky Mountains.

After this Miocene lake was filled up, and its surface more or less eroded, a great subsidence took place, and a second larger lake was formed, in which extensive beds of clay and sand were deposited over the same area. As both series of strata are essentially horizontal, and of nearly the same color, the dividing line in many places can be made out only by means of the vertebrate fossils they contain. In this way, the writer has recently ascertained, by personal observation, that most of the upper beds (D and E), 500 feet at least in thickness, which were called Miocene by Prof. Hayden,† are deposits of the more recent Pliocene lake. This would leave for the true Miocene beds a thickness of about 300 feet. The upper strata will be again referred to in considering the Pliocene lake.

The fauna of the White River lake-basin is now well known to naturalists. It indicates a climate much less tropical than that of the Eocene lakes, as is seen in the absence of monkeys and scarcity of reptilian life. The *Brontotheridæ*, the largest known Miocene mammals, are peculiar to the lower strata of this basin. They fully equalled the Eocene *Dinocerata* in size.

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* This Journal, vol. 1, p. 292, Sept., 1870.

† Transactions American Phil. Soc. vol. xii, p. 105, 1862.

A still more ancient Miocene lake existed, in about the same latitude, on the Pacific slope, near the central part of the present State of Oregon. The Blue Mountains formed the eastern and southern shores of this lake, but its other limits are difficult to ascertain, as this whole country has since been deeply buried by successive outflows of volcanic rocks. It is only where the latter have been washed away that the lake deposits can be examined. The discovery and first explorations in this basin were made by Rev. Thomas Condon, the present State geologist of Oregon. The typical localities of this Miocene basin are along the John Day River, and this name may very properly be used to designate the lake-basin. The strata in this basin are more or less inclined, and of great thickness. One section, near the John Day River, examined by the writer in 1871, and again in 1873, seems to indicate a thickness of not less than 5,000 feet. The upper beds alone of this series correspond to the deposits in the White River basin. The lower portion also is clearly Miocene, as shown by its vertebrate fauna, which differs in many respects from that above. Beneath these strata are seen, at a few localities, the Eocene beds containing fossil plants, mentioned above. They are more highly inclined than the Miocene beds, and some of them show that they have been subjected to heat. The inferior strata elsewhere are Mesozoic, and apparently Cretaceous. Above the Miocene strata, Pliocene beds are seen in a few places, but basalt covers nearly all.

III. *Pliocene Lake-basins.*

At the close of the Miocene, a subsidence took place east of the Rocky Mountains. A great Pliocene lake was thus formed directly over the eastern Miocene basin, having nearly the same boundaries on the north and west, but extending much farther east, and stretching south nearly to the Gulf of Mexico. It covered an area at least five times as great as the older lake, while its deposits attained a thickness of nearly or quite 1,500 feet. This lake basin may with great propriety be called the Niobrara basin, since the Niobrara River cuts through its typical strata for more than 200 miles of its course.

The beds in this basin lie nearly horizontal. They are light in color, and much more arenaceous than the Miocene below. The upper strata consist of hard sandstones or calcareous grits, which weather but slowly, and hence still form the great table-lands over much of the area of the basin. The writer has traced these high plateaux and the intervening isolated buttes from near the Black Hills south to the Arkansas River, and found them all of Pliocene age. South of the Smoky Hill River these strata rest directly on the Cretaceous.

The fauna of this lake-basin indicates a warm temperate climate. The more common mammals are a mastodon, rhinoceroses, camels and horses, the latter being especially abundant.

[To be continued.]

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *Theory of Electricity.*—Mr. E. EDLUND has published in full his theory that electricity is identical with the luminiferous ether. He first shows that the velocity *h*, with which the ether particles move, differs from *v*, the velocity with which this motion is propagated; *h* depends on the current and increases with it, while *v* depends on the ratio of the electricity to the density, but is independent of the current. It is the velocity *v* which Wheatstone, Fizeau and others have attempted to measure. The objection to this theory raised by MM. Roiti and Herwig are due to their overlooking the differences in these velocities.

The memoir deduces theoretically Ampère's general formula and the law of the division of the current between several conductors, when these contain electro-motive forces. Kirchhoff's laws are therefore an immediate consequence of the proposed theory. The heating produced by a galvanic current and by an electric battery are also given. The law for the duration of the discharge has also been deduced, and agreed with that found experimentally by M. Reiss. The oscillatory discharge of a battery and the phenomena of Peltier again give the same result as that deduced experimentally; and the same may be said of the laws experimentally established by Wiedemann for the transport of liquid in the direction of the galvanic current and for currents through a diaphragm, discovered by M. Quincke.

As to the rotation of the plane of polarization of light by a current, M. C. Neumann has shown that it can be explained satisfactorily if we admit that the molecular currents of Ampère act upon the molecules of ether in vibration precisely as if these last were electric molecules. The demonstration is based on the formula given by Weber for the action of an element of a current upon an electric molecule in motion. But as, according to Edlund, the electric fluid is nothing but the ether itself, evidently Neumann's hypothesis is confirmed, since according to the views of the author the molecular currents of Ampère are merely currents of ether.

The author says, in conclusion, that the theory proposed explains satisfactorily all the electric phenomena for which one can demand an explanation, based simply on such a theory. The explanation of many phenomena evidently demands the knowledge of the laws of other forces than electricity, since they unite with it in producing the phenomena in question. For instance, Faraday's law, that when a current traverses several electrolytes, the decompositions are proportional to their chemical equivalents, requires for its theoretical proof a much more exact knowledge of chemical forces than we at present possess. It is not then in the nature of things that such a law can be deduced exclusively from a theory of electrical phenomena.—*Mem. Swedish Acad. of Sci.*, XII, No. 8; *Bib. Univ.*, ccii, 174. E. C. P.

2. *Electrostatic Induction.*—M. NEYRENEUF has repeated with a Holtz the experiments of MM. Verdet and Masson, to determine the direction of the induced current with the electric egg. Two Matteucci disks contain, one the inducing, the other the induced spiral. This latter is put in connection with the two extremities of a cylindrical Geissler tube 50 cms. long. The first spiral is connected by one end with the negative armature of the machine, while the other is attached to one plate of a condenser with air film, whose collector communicated with the positive armature. The spark leaps between the two armatures of the machine from which the condensers have been removed. Two currents in opposite directions traverse the inducing spiral under these conditions, the one to charge, the other to discharge the condenser. Those produced in the second spiral illuminate the Geissler tube very brilliantly.

With an explosive distance of 5 cms. we observe clearly the difference in appearance of the two poles; but we can, by diminishing it continuously, produce three separate inversions at the instants when the appearance of the two poles is identical. When the striking distance is 3 cms. large and widely separated stratifications are formed, but presenting before the last inversion all the characters produced by a Ruhmkorff coil. The same effects are produced by altering the distance of the spirals or of the plates of the condenser. These condensers show that the phenomenon in question bears no relation to the lateral discharge, characterized especially by the constancy of the direction of the current it produces.—*Comptes Rendus*, lxxix, 1071. E. C. P.

3. *Effect of Flame on an Electric Spark.*—Mr. S. J. MIXTER notices a curious effect of a gas flame on the current of a Holtz machine. The jet consisted of a glass tube drawn out to a point, and the flame had a length of about an inch and a diameter of only an eighth of an inch. Inserting this between the two terminals of the machine, the length of spark obtainable was at once increased from less than ten inches to over twelve, the full distance to which the balls could be separated. The same increase was not obtained by simply inserting a conductor between the two terminals, a ball an inch in diameter only lengthening the spark about an inch.

E. C. P.

4. *Laws of Tuning Forks.*—M. MERCADIER has determined experimentally the effect of a change in the dimensions of a tuning fork on its number of vibrations. A style is attached to the end of the prong and draws a sinuous line on a revolving cylinder covered with lampblack. Another style attached to an electromagnet registers the beats of a clock giving seconds. The vibration of the fork is maintained electrically. By the thickness of prong is meant its dimension parallel to the vibrations, by its breadth, the perpendicular direction. A fork having a breadth of 35·3 mms. gave 144·7 vibrations. Reducing its breadth to 30·9 and 24·8 it still gave 144·7 and 144·9 vibrations. Hence the number of vibrations is independent of the breadth. A similar meas-

urement was made with different thicknesses from 20·25 mms. to 4·37 mms. and showed that the number of vibrations was nearly proportional to the thickness. A fork three decimeters long was then shortened 20 mms. at a time and the vibrations measured in each case. The length of the prong being altered from 295 to 57 mms., the number of vibrations increased from 40 to 974. Hence the vibrations are inversely proportional to the square of the lengths, calling the latter the projection of the line bisecting the prong, or the axis plus a small quantity y, which in this case equalled 3·8 mms. These laws may be written in the form of the equation $n=\frac{Ke}{(l+y)^2}$, in which n is the number of vibrations, e the thickness, l the length, y a small constant, and K a constant depending on the material. For steel, $K=818270$. To compare this constant with theory, K was computed from the formula for the number of vibrations of an elastic bar in terms of v, the velocity of sound in it; taking $v=4985$ m., according to the experiments of Wertheim; K in this case became 820131, a result differing from that previously obtained by only a fifth of one per cent. The number of vibrations of three forks were found by measurement to be 144·7, 77·7, 29·7, and by the formula 146·4, 79·0, 29·0. Hence forks may be made by calculation to give any desired number of vibrations within one or two per cent.—*Comptes Rendus*, lxxix, 1001, 1069. E. C. P.

5. *Polarization of Light;* by WILLIAM SPOTTISWOODE, M.A., LL.D., F.R.S. 130 pp. 12mo. London: 1874. Nature Series. (Macmillan & Co.)—This small treatise on the Polarization of Light, as the Preface states, contains the substance of lectures delivered by the author at various times to his work-people, and "constitutes a talk rather than a treatise on Polarized Light." The fundamental principles of the subject are explained in a simple manner, and are well illustrated by means of two beautifully colored plates, besides a number of wood-cuts.

II. GEOLOGY AND NATURAL HISTORY.

1. *Earthquakes of North Carolina.*—An excellent article on earthquakes in the mountain region of North Carolina has been published by Hon. T. L. Clingman in the *Western Expositor* of Asheville, N. C. He states that more than thirty years since his attention was called to statements that a mountain in the northern part of Haywood County was shaken at intervals of two or three years; and in 1848 he visited the region and published a paper on it. The principal facts stated were these:

Between the Blue Ridge, which in North Carolina separates the waters falling into the Atlantic from those discharged into the Missisippi, and the great chain on the Tennessee border designated in its course by such names as Iron, Unaka, and Smoky, there is an elevated plateau of over two hundred miles in length, with an average breadth of fifty miles. The beds of the larger streams are two thousand feet above the sea, and the general level of the country, exclusive of the mountain ranges, may be estimated at twenty-five hundred feet above tide-water. Haywood County joins the State of Tennessee on its northern border, and the seat of the disturbance is within less than twenty miles of the line of that State. A considerable range of mountains extends north and south along the line which separates the counties of Buncombe and Haywood. From the west side of this extends a ridge, which terminates near the head of Fines Creek. A quarter of a mile from its western end, as one moves up it toward the east, is the locality referred to. The effect of the disturbance is visible near the crest of the ridge and extends in a direction nearly south, down the side of the little mountain, four or five hundred yards, to the level ground, and across it for some distance and along the elevations beyond. The whole extent may be a mile in length, with a breadth of not more than a couple of hundred yards at any point. The top of the ridge, where evidences of violence are seen, is perhaps three or four hundred feet higher than the ground below. There are cracks in the solid granite of which the ridge appears to be composed, but the chief evidences of violence were observable a little south of the crest. From thence along the side of the mountain, as one descends, there were chasms, none of them above four feet in width, generally extending north and south, but also occasionally seen in all directions. All the large trees had been thrown down. There were a number of little hillocks, the largest eight or ten feet high and fifty or sixty feet in diameter. They were usually surrounded by what appeared to have been a narrow crevice. On their sides the saplings grew perpendicularly to the surface of the ground, but obliquely to the horizon, making it manifest that they had attained some size before the hillocks had been elevated. I observed a large poplar or tulip tree which had been split through its center, so as to leave one-half of it standing thirty or forty feet high. The crack or opening under it was not an inch wide, but could be traced for a hundred yards, making it evident that there had been an opening of sufficient width to split the tree, and that then the sides of the chasm had returned to their original position without having slipped, so as to prevent the contact of the broken roots.

When I was there I was told that three years had elapsed since the last previous shock. They were first noticed about the year 1812, and usually repeated at intervals of two or three years. In 1851 I visited the locality again, having been informed that a feeble jar had occurred. As soon as I arrived at the locality, I was struck with the truthfulness of what many persons had told me, that after each shock the appearance of the place was so much changed that it did not at all resemble itself. On this occasion, though the shock had been a feeble one, I found the appearances very different. The greatest evidences of violence were near the foot of the ridge, the branch having been somewhat turned out of its course. Near this place a rock of considerable size had been thrown up and had only partially settled back, owing to the closing of the opening under it, so that the former earth marks were seen several feet above the ground on its sides.

In the year 1867 I saw the locality again. A number of shocks had in the meantime occurred, and the appearances were very different from what they had been. From the top of the ridge to the base it seemed a mass of rocks, most of the earth having been carried away. The depression at the top was greater, while the successive jars had, under the action of the force of gravity, moved the mass downward, and had forced the stream still further away from the hill. The violence had at one point extended itself a little further to the east. A large oak tree of great age and four or five feet in diameter had been entirely split open from root to top, and thrown down so that the two halves lay several feet apart.

In my former publication, it was suggested that if the phenomena at this point were due to volcanic action, similar disturbances would be noticed at other localities in the Alleghany range. I was soon informed that three or four years previously, in the southeastern part of Macon County, between the Tuckasegee River and the Cowee Mountain, the ground was shaken violently for several minutes. A few days afterward some persons discovered a fresh chasm, two or three feet wide, which extended more than a mile. This was in the month of June, and they said the leaves and branches of timber immediately above the chasm, in places, presented the appearance of having been scorched. Though I was not able to visit the place, yet from the character of my informants I do not doubt but that the facts were as above stated.

I have also been informed that in the county of Cherokee, in the year 1829, or thereabouts, the Valley River Mountain was cleft open for a considerable distance, during a violent shaking of the earth in that vicinity. The chasm, though partially filled up, is represented as still visible.

Mr. Silas McDowell, of Macon County, a highly respectable and intelligent gentleman, accustomed to observe and write on such subjects, has stated recently in a paper published at Asheville, that many years since there was a violent shock in the neighborhood where he resides, during which a chasm was opened on the north side of the mountain which separates the Ellejay waters from those of the Sugar Fork River. He states that the opening is still visible. This locality is eight or ten miles to the southeast of Franklin, in Macon County.

About three years since I heard from many persons, that for several weeks smoke continued to issue from a small crevice in the rock, in Madison County. Not long afterward I went to the place, and though the smoke had previously ceased to issue, yet there was evidence that the locality had at some time, probably during the present century, been subjected to violence, that had changed the outlines of the ground and surface rocks. This spot is about fifteen miles east of the Haywood Mountain, and about as far from the Warm Spring to the northwest of it.

Lastly, we have to notice the disturbance of the Bald and Stone Mountains. They are situated six or eight miles to the east of the Blue Ridge. Between the headwaters of the Catawba and those of Broad River, there extends many miles eastward a range of mountains attaining the height in places of four thousand feet. The Bald and Stone Mountains, from their appearance, are probably the highest part of this ridge, and nearly equidistant from the Catawba and Broad Rivers. My information in reference to them is derived entirely from conversations with a number of gentlemen, and from the accounts published in the newspapers. The first shocks were perceived on the 10th of February last, and they were for the first month or two more frequent than they have since been. During the last two months they have occurred at intervals of a week or two, but have been rather more violent than the average. Within the last five months probably a hundred shocks, accompanied with noises, have occurred.

The distance from this point to the Valley River Mountain, in Cherokee, nearly due west, is more than one hundred miles in a direct line. From the mountain in Haywood, to reach the parallel of latitude passing through the mountain near Ellejay, in Macon, one must travel more than thirty miles south. It is thus manifested that there is a belt of country more than a hundred miles in extent from east to west by thirty in breadth in which such disturbances have been observed. In the present state of scientific knowledge, it may not be an easy task to offer an explanation of the causes which will be generally accepted as satisfactory.

When we take into account these indications at different points in the North Carolina mountains, it seems evident that there is beneath the surface a condition of things that extends over a considerable area. A portion of the globe which, from its geological structure, ought to be regarded as being as stable as any part of our planet, is nevertheless not free from change. Whether this is to be regarded as due to the diminishing force, which at one time was sufficient to heave up this tract of country, with all its mountain chains, or whether it is to be considered as evidence of a gradual return of that volcanic action which manifests itself still elsewhere, to so great an extent, it is perhaps difficult to decide until further observations have been made. Is it not of sufficient interest to justify the managers of the Coast Survey, or some other competent agency, to make such careful measurements of the height of certain points, as to ascertain, within the next twenty-five or fifty years, whether any, and to what extent, changes may be occurring in this region?

2. *Porphyry of the Island of Lambay, a few miles north of Dublin Bay.*—Professor Edward Hull finds this green porphyry (Geol. Mag. for Oct., 1874) to consist of a felsitic base, which is penetrated throughout with grains of a chloritic mineral and magnetite, and crystals of orthoclase. Minute cavities have sometimes clusters of crystals of magnetite on the inner surface, and chlorite, or calcite, or both, in the interior. The

chlorite is regarded by the author as of secondary origin, and as "introduced by the agency of water, which has permeated the whole mass of the rock through channels inconceivably narrow." The calcite is diffused throughout the rock and also in its minute rifts and cavities, and the author remarks also that "it is unquestionably due to infiltration;" adding that "it is easy to conceive that water percolating downward through to the limestone and the thin coating of Old Red Sandstone beneath, would become thoroughly impregnated with carbonate of lime, which it would deposit amongst the fissures and cells of the older rocks beneath."

As urged in this Journal, vol. vi, p. 107, with reference to the Mesozoic trap of the vicinity of New Haven, Connecticut (and applied also to other traps or dolerytes), it seems to be quite certain that the chlorite in the above mentioned porphyry, and in igneous rock generally, was not only derived from ingredients in the rock, but was made through the agency of water that gained admission from some subterranean source *when the melted rock was ascending to the surface;** that the same is true for other hydrous minerals disseminated through the mass of any igneous rock, for example, the zeolitic, in phonolytes. The same kind of evidence sustains, as stated in the same place, the conclusion that all amygdaloidal rocks owe the cavities which they contain (the cavities occupied by the amygdules), and a large part of the zeolitic and other minerals in the cavities, mainly to the action of the same waters on the rock—first while melted and then while cooling from a state of fusion. Further, such waters may have carried into the cavities not only hydrous minerals, but also anhydrous. Melted rock rising in a fissure would take in also any gas or vapor that was evolved by the heat from rocks adjacent to the fissure, and would thus become more or less penetrated with the gas or vapor, and by this method carbonate of lime might have been made with any lime in the rock, if carbonic acid were the gas received, or other minerals with the aid of other vapors, even, it might be, metallic ores. If cavities exist they are sure to become filled, because all solutions would pass into them and keep depositing until they were full; and the rock, if the cavities were numerous, might thus be drained of a large part of the minerals formed, the chlorite excepted. These cavities have no doubt sometimes received mineral material from infiltrations of later date, but not so the body of the rock. J. D. DANA.

3. *Geological and Topographical Survey of the Territories.*—Concerning the work of the season just ended, the Secretary of the Interior makes the following Report to Congress:

"The first division of the survey under Mr. Hayden completed the unfinished work of the preceding season in the central portion of Colorado Territory, and extended its operations westward over that portion of said territory lying between the one hundred and eighth and one hundred and tenth meridians of west longitude. About eighteen thousand square miles were surveyed, covering a section of country probably more generally elevated above the sea-level than any other within the borders of the United States. As an illustration of the uniform great elevation of extensive sections of this region, it may be mentioned that one of the subdivisions of the survey, in exploring an area of nearly three thousand square miles, was compelled to operate above the timber-line (about 11,500 feet above the level of the sea) for over a month. The necessary materials have been collected for constructing accurate maps of the region surveyed, which will require for illustration six sheets or maps of the physical atlas. Special attention was given to the mining and agricultural resources of the country, and those portions of it which can be redeemed by irrigation will be properly indicated on the maps. The San Juan mining region in southern Colorado was included in the survey, and over fifty mines therein were properly located. Many valuable specimens of ores, minerals, fossils, Indian art, &c., were collected. Numerous ruins of towns and dwellings of an extinct race of people which once inhabited the mèsas and cañons of western Colorado were found, and remarkable fortifications of hewn stone laid in mortar discovered in the sides of deep cañons, many of which are situated a thousand feet vertically from the stream below. The structure of these fortifications and dwellings, and the peculiar glazed pottery in the vicinity, indicate the existence of a people inhabiting this region many centuries ago, who were much further advanced in the arts than any of the Indian tribes of the present day. The results of the work of the past season will exceed in quantity and interest those of any previous year.

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* The conclusion, as far as it regards the trap of the Connecticut Valley, is sustained by the microscopic researches of Mr. E. S. Dana, an abstract of whose paper is given on page 390 of the last volume of this Journal.

The field of operations during the past season of the second division, under Mr. Powell, was the central and northeastern portions of Utah Territory, and its labors were principally confined to the completion of the unfinished work of the preceding year. The main party is still in the field, so that the full results of the season's survey cannot at this date be given. It may be stated, however, that material has been collected for the mapping of an extensive region of country heretofore but little known; that the positions of many of the more important mineral lodes have been determined, and will be represented on the "general" maps; and that the area and distribution of such portions of the country surveyed as can be redeemed by irrigation will be properly indicated on the "special" maps. Extensive coal-beds have been discovered and traced, interesting and valuable specimens of fossils, rocks, minerals, and ores obtained, and a large collection made of Indian relics and articles, illustrating the arts existing among the Indians inhabiting that region. Mr. Powell had, in former surveys, discovered many ruins of towns and hamlets once occupied by the ancient inhabitants of the valley of the Colorado River; and during the past season many other such ruins have been found, some of their ancient picture-writings and many of their stone implements collected. The positions of many scores of these ruined towns will be accurately indicated on the "general" maps. The researches of this division among the extinct races, as well as the present inhabitants of this interesting region, have embraced polity, mythology, traditions, language, poetry, arts, habits, customs, and the means of obtaining subsistence, together with pre-historic remains; and when the results obtained shall have been published, it is believed that they will constitute an important contribution to the ethnography of American tribes.

These surveys have, so far as they have been prosecuted, resulted in affording much information of great value to our people, as well as to the scientific world. The construction of a physical atlas of the Territories, which will show all the results of the surveys as rapidly as they can be prepared for publication, is designed to preserve, for convenient reference, the information thus obtained; and if a continuation of the surveys should be authorized, such an atlas would become, in time, of intrinsic value, not only to the people at large, but to other nations.

In view of these and other considerations, I regard the moderate cost of these surveys as more than compensated by the value of the information thereby obtained, and therefore cordially recommend a continuation of the United States geological survey of the Territories."

The many photographs taken by the artist, Mr. Jackson, during the past season, are admirable specimens of the photographic art. There is not as bold scenery as in some Rocky Mountain views, but there is wonderful beauty of landscape, which is heightened by the perfect aerial perspective given the pictures and the well-selected foreground. Lakes, rivers, cañons, hills, eroded strata, mountain ridges and distant snowy ranges, are the different elements in the views. They are from the Middle Park, the Grand River region and other places. A considerable number of them represent cave-dwellings high up on cliffs, in the cañon of the Mancos, and near the McElmo, and ruins of walls and other structures of stone in the vicinity of the same—the dwellings and fortified places of a semi-civilized race now extinct.

4 *On the more rapid deposition of Sediment in Salt than in fresh water.*—Prof. T. STERRY HUNT, in a recent article in the Proceedings of the Boston Natural History Society, calls attention to the fact that the effect of salt in water on the rate of deposition was first mentioned by Mr. Slidell, in Humphrey and Abbott's Report on the Mississipi River, and then explains it on the ground of the less cohesion between the particles of salt water, as proved by the fact that drops of salt water are smaller than those of fresh water.

5. *Artesian boring at the St. Louis Insane Asylum.*—Mr. G. C. BROADHEAD, State Geologist of Missouri, gives the details respecting this boring, in the Transactions of the St. Louis Academy of Sciences, vol. iii, 216. The whole depth is 3,843·5 feet; of this the last 40 feet were through Archæan granite; above, the beds

were of the Lower Silurian and Carboniferous formations. The Carboniferous at the well had a thickness of 873 feet—80 of it Coal-measures, 670 feet Subcarboniferous, 93 feet Choutean group (referred by most authors to the Subcarboniferous, and called in Iowa and Illinois the Kinderhook group). The Lower Silurian, beginning above, consists of Trenton 421 feet; first magnesian limestone 148 feet; sandstone (called saccharoidal sandstone, used in glass-making) 133 feet; second limestone 517 feet; second sandstone 82 feet; third limestone 838 feet; third sandstone 98 feet; fourth limestone 384 feet; fourth sandstone, called Potsdam, 299·5 feet. Salt water was obtained (in place of fresh) at 1,220 feet, and a sulphur water at 2,140 feet. At 2,256 the water in the sand-pump indicated 3 per cent of salt; at 2,957 feet, 4½ per cent; at 3,293 feet, 2 per cent; at 3,367 feet, less than 2 per cent; at 3,384 feet, 3 per cent; and below 3,545 feet, 7 to 8 per cent. With a Fahrenheit self-registering thermometer, the temperature obtained at depths from 3,127 to 3,837 varied from 107° to 104½°, being 105° at the lowest point reached.

In an artesian well at Belcher's sugar refinery, St. Louis, salt water was obtained at 610 and 849 feet below the surface at the place, or 790 and 1,029 feet below the level of the surface at the asylum well.

6. *Return of Professor Marsh's Expedition.*—Professor MARSH returned to New Haven, Dec. 12th, after an absence of two months in the West. The object of the present expedition was to examine a remarkable locality of fossils, discovered during the past summer in the Bad Lands south of the Black Hills. The explorations were very successful, notwithstanding extremely cold weather and the continued hostility of the Sioux Indians. The latter refused to allow the expedition to cross White River, but a reluctant consent was at last obtained. They afterward stopped the party on the way to the Bad Lands, attempted a night attack on their camp, and otherwise molested them, but the accompanying escort of United States troops proved sufficient for protection. The fossil deposits explored were mainly of Miocene age, and, although quite limited in extent, proved to be rich beyond expectation. Nearly two tons of fossil bones were collected, most of them rare specimens, and many unknown to science. Among the most interesting remains found were several species of gigantic *Brontotheridæ*, nearly as large as Elephants. At one point these bones were heaped together in such numbers as to indicate that the animals lived in herds, and had been washed into this ancient lake by a freshet. Successful explorations were made, also, in the Pliocene strata of the same region. All the collections secured go to Yale College, and will soon be described by Professor Marsh.

7. *Survey of the 40th Parallel.*—The preparation of the final reports on this Survey, by CLARENCE KING, is in rapid progress, and their publication is expected to go forward during the present year. Prof. ZIRKEL, of Leipsic, was in the country during the month of September, at Mr. King's solicitation, to examine rock

specimens, and to take for study the thin sections of the rocks which had been made—more than a thousand in number—and will prepare a report upon them for publication in the 40th Parallel series.

8. *Geological Sketch of the State of Missouri, illustrated by maps*; by Geo. C. Swallow, A.M., M.D., late State Geologist, and Prof. Agric., Geol. and Bot. in the Univ. of the State of Missouri. 10 pp. large 4to. St. Louis, 1873. (R. A. Campbell.) This sketch by Professor Swallow presents a brief review of the Geology of the State of Missouri and is accompanied by a colored section and a colored geological map. The section makes the third and fourth magnesian limestones, with the intervening sandstone, equivalents of the Primordial.

9. *Das Elbthalgebirge in Sachsen* of Dr. Geinitz.—The fifth number of Part II has been issued by the publisher, T. Fischer, of Cassel. It contains the Gasteropods and Cephalopods, with plates 29 to 36 inclusive.

10. *Preliminary Notice of Chondrodite Crystals from the Tilly-Foster Iron Mine, Brewster, N. Y.*; by Edward S. Dana.—The occurrence of chondrodite in large quantities at the Tilly-Foster Iron Mine has already been described by Professor Dana in an article upon "Serpentine Pseudomorphs," published in the two preceding numbers of this Journal. In addition to the common massive variety of the species, and that occurring in large coarse crystals, there have been found also, though very rarely, some small but very fine crystals, admitting of the most accurate measurements. These crystals are of a rich garnet-red color, with absolutely faultless luster, and are modified by a very large number of planes. The special interest of chondrodite arises from its relation to, or more properly identity with, the Vesuvian humite, whose remarkable crystallographic character in its three types of forms has been made known in full by the labors of Secchi and vom Rath. Kokscharow has shown that the chondrodite of Pargas is identical in angle with the *second* type of humite, and vom Rath has since obtained the same result for the Swedish specimens. The examination of the Brewster crystals, which I have been carrying on for the past few months, shows that they also belong, for the most part, to the *second* type, though among a considerable number of crystals I have identified also representatives of *the third type*. A few angles will show how close is the resemblance to the humite. (The letters for the planes are those used by vom Rath.)

	II Type.			III Type.	
	Humite (v. Rath).	Chondrodite (E. S. Dana).		Humite.	Chondrodite.
$A : \frac{1}{7}r$	135° 18′	135° 19′	$A : \frac{1}{8}r =$	111° 51′	111° 49′ 111° 44′
$A : \frac{1}{5}r$	125° 49′	125° 50′	$A : i =$	109° 28′	109° 25′
$A : e$	108° 58′	109° 3′	$A : \frac{1}{2}i =$	125° 15′	125′ 13′
$A : i$	122° 28′	122° 28′			

The planes, which occur constantly, are the same that are found on humite, and the kind of hemihedrism is the same. This is true for both types; thus in type II the planes identified on chondrodite are A, B, C, $+r$, $+\frac{1}{5}r$, $-\frac{1}{3}r$, $-\frac{1}{7}r$, n, $\frac{1}{3}n$, $-m$, i, e, $\frac{1}{3}e$, etc. Type III, chondrodite, $+r$, $+\frac{1}{5}r$, $+\frac{1}{9}r$, $-\frac{1}{3}r$, $-\frac{1}{7}r$, $-\frac{1}{11}r$, n, $\frac{1}{3}n$, $\frac{1}{7}n$, i, $\frac{1}{3}i$. etc.

In addition to these prominent planes, I have measured, and more or less surely determined, upwards of a *hundred* other planes, very minute, and yet flat and giving in many cases perfectly reliable angles. As would naturally be expected, the indices of these planes are in general anything but simple. The discussion of these, and the description of other points of interest which have been observed, including several twins, must be deferred till the final completion of the memoir, which, it is hoped, will not be long delayed.

11. *Livingstonite.*—This mineral, recently described by Señor M. Bárcena (see this Journal, last vol., p. 145), has been analyzed by its describer with the following results: Sulphur 29·08, antimony 53·12, mercury 14·00, iron 3·50 = 99·70; whence the atomic ratio for the sulphur, antimony, mercury and iron 18·17 : 8·7 : 1·4 : 1·2 = (nearly) 15 : 7 : 1 : 1.

The livingstonite occurs at Huitzuco, in the State of Guerrero, in a matrix of carbonate and sulphate of lime, along with native sulphur, cinnabar, valentinite and stibnite. The author mentions the occurrence of some specimens of cinnabar at the locality which have the form of livingstonite, and which, therefore, are pseudomorphous.

12. *On Dawsonite, a new mineral;* by B. J. HARRINGTON, Chemist and Mineralogist to the Geological Survey of Canada. (Canadian Naturalist, vii, August, 1874.)—Dawsonite is from a whitish felsitic dike near the western end of McGill College, Montreal. Hunt obtained for the rock (which he calls trachyte), after separating 9·55 by nitric acid (Geol. Can., 1863, 659, 660), silica 63·25, alumina 22·12, lime 0·56, potash 5·92, soda 6·29, volatile 0·93=99·07. The portion dissolved out by nitric acid contained: Silica 1·43, alumina 2·43, peroxide of iron 2·40, red oxide of manganese 1·31, lime 0·60, potash 0·40, soda 0·98=9·55. In the joints of this rock occurs a white-bladed mineral, which is the Dawsonite.

The following are its characters. Hardness 3; G=2·40; luster vitreous; transparent to translucent. The blades are somewhat fibrous in structure, and the crystallization is probably monoclinic, with the inclination of the vertical axis about 75°. It shows bands of colors in polarized light. B. B. becomes opaque and often exfoliates or swells into cauliflower-like forms. In the closed tube yields water and carbonic acid. Dissolves completely in cold dilute hydrochloric or nitric acid. Two analyses, made at different times, the last from portions of several specimens, afford Mr. Harrington—

$\ddot{\mathrm{Si}}$	$\ddot{\mathrm{C}}$	$\dddot{\overline{\mathrm{Al}}}$	$\dot{\mathrm{Mg}}$	$\dot{\mathrm{Ca}}$	$\dot{\mathrm{Na}}$	$\dot{\mathrm{K}}$	$\dot{\mathrm{H}}$
0·40	29·88	32·84	tr.	5·95	20·20	0·38	11·91=101·56.
——	30·72	32·68	0·45	5·65	20·17		[10·32]=100.

Mr. Harrington inclines to the conclusion that the mineral is a hydrous carbonate of alumina, lime and soda; and that it is related therefore to hovite, which the Messrs. Gladstone made a carbonate of alumina and lime. Its crystalline character and its uniformity in optical and chemical characters show that it is not a mechanical mixture.

13. *Tables for the Determination of Minerals, by their Physical properties, ascertainable by the aid of such simple instruments as every student in the field should have with him.* Translated from the German of WEISBACH. Enlarged, and furnished with chemical formulas, a column of specific gravities and one of the characteristic Blowpipe reactions; by PERSIFOR FRAZER, Jr., A.M., Assist. Geol. in the Second Geol. Survey of Pennsylvania, etc. 118 pp. 8vo. Philadelphia, 1875. (J. B. Lippincott & Co.)—All helps to the determination of minerals will be welcomed by the student in the science, and Weisbach's tables are among the best of those based chiefly on the physical characters. We notice that the species of minerals that are peculiarly American are mostly omitted, and many of the recently described foreign species; the addition of these will give increased value to another edition of the work. The volume is very clearly printed, on good paper.

14. *Notes on the Tree-Ferns of British Sikkim, with Descriptions of three New Species, and a few Supplemental Remarks on their Relations to Palms and Cycads;* by JOHN SCOTT, Curator of the Royal Botanical Gardens, Calcutta. This paper, recently published in the thirtieth volume of the *Transactions of the Linnean Society*, was read in Feb., 1870. It is illustrated by 18 plates, eleven of which relate to the structure of the stem, etc., a subject which is here treated with freshness and ability, as well as tersely, and to which interest is added by some ingenious speculations as to history and origin. In the concluding summary, considering the relationship of Ferns, "as founded on the structure of the stem," the author remarks: "First, then, they agree with Palms in their mode of growth and in the manner in which the leaves are arranged. I fail to see why the growth of the one should be termed endogenous, and the other acrogenous. If the peculiarity of endogeneity is the downward growth of a series of vascular bundles from the leaves in a curvilinear mode, from the interior to the periphery, not a few of the tree-ferns have very similar characteristics. In both, diametrically and longitudinally, growth is strictly apical; in both the stems are limited in their diametrical growth, and from the first traces of their formation the axial and peripheral developments are simultaneous; and as soon as the normal thickness has been attained, all further peripheral increase ceases, and the axis grows upward in a cylindrical form. In Palms the growing point is conical, and in tree-ferns terminally flattened; so that in the one case the nascent vascular bundles are almost horizontally arranged at the apex, in the other they are from the beginning vertical; but in both all cellular increase of the body of the axis has ceased ere the fronds have

attained their full development. Another point in which they agree is in the development of a system of free vascular bundles, which, originating in the apex of the stem, grow upward into the fronds and downward into the axis. In other respects they present many important points of difference, as in the free anastomoses of the fibro-vascular bundles, and the formation of a woody circle surrounding the central cellular axis, which, however, is partially represented in certain Palms (vide plate XII), as *Euterpe oleracea*, by a dense zone of vascular bundles, which very distinctly separates the central, in which the cellular tissue predominates, from the peripherical and essentially vascular. The tissues of this median zone are developed, I believe, exclusively from below upward, the growing points surrounding the apex of the stem and passing outward into the fronds. Dense though this zone is, the bundles do not, in any case that I have observed, inosculate with each other. Another important difference is presented in the passage of these bundles to the fronds. In Ferns, small ramifications of the bundles only pass into any given frond, while in Palms the entire bundle enters the frond. I need scarcely remark that there are also important differences in the minute structure of the woody bundles of Ferns and Palms, the former being much more simple, though apparently similarly limited in their period of growth. In thus noting the structural affinities and differences of the stems of tree-ferns and palms, I by no means ascribe to them an equal grade of organization; tree-ferns are decidedly inferior in rank. They have, however, sufficient in common to justify the opinion of their being differently diverged and progressed forms of a common ancestor, which must have been of great antiquity, considering that both apparently presented much the same characteristics in the Carboniferous epoch as they do now." (p. 28–29.)

The analogies between the monocotyledonous stem and that of *Nymphæa* is next referred to, and also of *Cacti* with *Cycas*. More practical is the author's correction of the current statement that *Cycas* has a circinate vernation, whereas *C. circinalis* and all the rest have a perfectly straight vernation, with only the leaflets involute. A. G.

15. ***Flora Brasiliensis.***—Fasciculus 53, issued in April last, contains the *Polygaleæ*, by A. W. Bennett of London, 82 pages of letter press and 30 plates, a liberal allowance. *Polygala* is largely represented in the Brazilian empire, viz: by 86 species; *Monnina* by 11, and *Securidaca* by 18 species. *Krameria* is included in the order, and has six species. It was this latter genus, and not *Polygalaceæ* in the restricted sense, that the present writer thought nearer *Leguminosæ Cæsalpineæ* than to anything else in general floral structure, and he was disposed to regard the contrary position of the odd sepal and petal, and the simple carpel, as more important than they seem to subsequent authors; also, the floral structure of *Polygaleæ* most resembles *Sapindaceæ*. But, in fact the order is very peculiar.

Fasciculus 64, of the same work, issued in May, comprises the concluding half of J. Muller's elaboration of the Brazilian *Euphorbiaceæ.* This and fasc. 61 together comprise vol. xi, pars ii, a very substantial volume indeed, filled by this one order; 752 pages of letter press, 104 plates—a great undertaking, well and happily brought to a conclusion by the excellent botanist to whom this task was assigned. A. G.

16. *Journal of the Linnean Society,* No. 77, issued in October, contains ten papers, by Mr. Moseley and several others, collected under the general head of *Contributions to the Botany of the Expedition of H. M. S. Challenger.* They relate mainly to Marine Algæ and the maritime plants, and to Algæ from hot springs in the Azores. The value of the observations upon the vegetation of thermal waters is much diminished by the total lack of all record of the precise temperature of the waters in question.

A. G.

17. *Transactions and Proceedings of the Botanical Society.* Edinburgh. Vol. xi, part iii, 1873.—This part completes the eleventh volume of the series. It contains the President's (Prof. Wyville Thompson's) Address, delivered in November, 1872, a notable production, which was printed in some of the journals at the time, and excited much attention, being a lucid exposition of fermentation and putrefraction, and of the Mould-Fungi that produce and govern these decompositions. Biographical notices of the late Hugo von Mohl and of Andres Œrstedt are appended. Prof. William R. M'Nab contributes an elaborate paper on the organization of *Equisetum* and *Calamites.* Mr. Wilson's interesting paper here appears, in which he disproves the popular and long-current notion that Darnel grain is poisonous or even injurious. His paper was noticed in this Journal; but it may not be amiss to recapitulate, in the author's own words, that, after feeding upon the seeds himself freely, he concludes that, in his own case "darnel is not justly called *temulentum;*" its seeds and husks are not "infelix," are not "acrid," are not "unwholesome," are not "injurious," do not cause "delirium," do not produce "stupefaction," and are not "poisonous" nor productive of "fatal" results. Dr. Robert Brown (may he prove himself worthy of the name!) gives some notes upon Dichogamy and other allied subjects, instancing *Clerodendron Thompsoniæ* as an admirable example of "protandrous dichogamy;" the first time, so far as we know, in which this has been spoken of in Great Britain, although here long familiar. Other Clerodendrons show it, but none so beautifully. W. Ramsay Wright gives a digest of Dr. Eichler's well-known essay, entitled: "Are the Coniferæ gymnospermous or not?" These are only the more scientifically interesting articles.

A. G.

18. *Florida Plants.*—Dr. Edward Palmer made last year a collection of dried specimens of the plants of East Florida and the Keys. Four or five sets of these, named, are on sale, and may be ordered of S. Watson, Harvard University Herbarium, Cambridge. These sets range from 250 to 630 species, and cost $8 the hundred.

Dr. Palmer is now on his way to an exploration of the Islands of the southern part of California. A. G.

19. *The American Naturalist* closes its 8th volume with the December number just issued, and appeals for the moderately larger support needful to make this, our only natural history magazine, self-sustaining. We well understand what that means. When the paid-up subscription rises so as to cover the actual outlay in the production, the public should still take to heart the fact that all the scientific talent and effort, and the editorial labor, will still be unpaid for. The editors in this case are entitled to great credit for the zeal, perseverance and sacrifice with which they have kept this magazine, not only alive, but every year more worthy of life; let us add that they are also entitled to a more generous patronage. There are many more than the five hundred new subscribers for whom they now call, to whom the Naturalist would be of great use and interest. A. G.

20. *Botanical Necrology.*—On turning over the pages of the *Botanische Zeitung* for the year 1874 (received down to the end of November), we find no record of the death of any botanist, except that of Pritzel.

George Aug. Pritzel, of Berlin, was born in 1815, and died on the 14th of June last. His only botanical publication was a revision of the genus *Anemone*, contributed to the Linnæa in 1842. The valuable services which he rendered to the science, and which should preserve his memory, were by the faithful preparation of the indispensable *Thesaurus Literature Botanicæ* in 1851; of the *Iconum Botanicarum Index locupletissimus* in 1855, with a re-issue and supplement in 1866, and by the new edition of the *Thesaurus* in 4to, of which four parts, carrying the alphabetical portion of the work down to the word *Tournefort*, were issued in 1872. A severe illness, from which the author appears never to have recovered, interrupted the publication. This work and the *Iconum Index* are models of editorial and typographical excellence.

Rev. Richard Thomas Lowe, a much respected English botanist, born in 1802, who passed most of his life since 1828 in Madeira, and diligently investigated its botany, was lost at sea in April last, by the foundering of the Steamer Liberia, in which he was a passenger on his way to Madeira. His principal publications were the *Primitiæ Faunæ et Floræ Maderæ*, which appeared in the Cambridge Philosophical Transactions in 1830, with a supplement (*Novitiæ*, &c.) in 1838; and a *Manual Flora of Madeira*, begun in 1857, continued into the second volume in 1872, and left unfinished.

Mrs. Hooker.—With inexpressible sorrow we record the death of this excellent and accomplished lady, the wife of Dr. Hooker, Director of the Royal Gardens at Kew, and the President of the Royal Society, and the daughter of the late Professor Henslow. This sad event occurred on the 13th of November, of heart-disease. Of the great practical services rendered to science by most efficient aid to her husband we may not speak; but her name has of late become well known in Botany, through her able translation of Le Maout and Decaisne's *Traité de Botanique*, for the English edition edited by Dr. Hooker.

JOHN TRAHERNE MAGGRIDGE (as the English journals at this moment inform us) died at Mentone, on the 24th of November, at the age of 32 years. His contributions to the Flora of Mentone, with its exquisite illustrations by his facile pencil, and not less admirable letter-press, his work on Harvesting Ants and Trap-door Spiders (published by Reeve & Co., London), and many short articles or notes in Nature and other journals, have made this charming young man's talents and keen powers of observation most favorably known to the scientific world. He has been for years an invalid, and took up his residence in the Riviera for the sake of a better winter climate than his native England. Here his scientific investigations have mainly been prosecuted,—were perhaps undertaken for the sake of occupation and interest during the slow progress of fatal pulmonary disease. A. G.

21. *Traité de Botanique conformé a l'état présent de la Science*, par J. SACHS; traduit de l'Allemand sur la 3^{e} édition et annoté par Ph. Van Tieghem. Paris, 1874. *Lehrbuch der Botanik*, 4te Auflage, Leipzig, 1874.—The third edition of Professor Sachs's treatise was noticed in this Journal, May, 1873.

Professor Van Tieghem's translation and the new German edition are both revisions and demand a further notice.

The French revision is an accurate translation and displays great editorial skill. The entire treatise has been divided into topical paragraphs, to each of which a telling title has been given. In not a few instances, the heading embodies the substance of the section, and greatly increases the value of the book as a work of reference. This may be illustrated by a single chapter,—On the relation between the morphological nature of organs and their adaptation to the conditions of plant life. This chapter in the German edition occupies seven closely printed pages and has no division into subordinate sections, but in the translation the following headings are given:

Metamorphosis, adaptation and utility express one and the same fact, and are synonymous terms.

The same members of a plant can adapt itself to the most varied functions, and the same function can be performed by most widely different members.

Organic arrangements useful to the plant are reached by most varied adaptations.

Examples.—1. Vertical growth of the stem. Its utility. Different ways in which it is secured. By (a) Ascending stems. (b) Twining stems. (c) Climbing with tendrils, different adaptations of tendrils. (d) Stems climbing without tendrils.

2. Subterranean growth. Its utility. Different ways in which it is brought about.

3. Dissemination of seeds. The methods by which it is secured. There are gradual transitions in adaptations.

The editorial notes communicated by Professor Van Tieghem, in the departments of microscopic anatomy and morphology, are numerous and important. Considerable prominence is given to his well known view that the elements of a fibro-vascular bundle are differently arranged in the root, stem and leaf.

The new German edition gives evidence of thorough revision. Numerous slight changes in phraseology, greatly for the better, have been made throughout the book, and several chapters have been added. The most important of them are the following: Supplementary growth in thickness; alternation of generation; periodical movements of developing organs.

A change in arrangement which will provoke much adverse criticism is the new classification of the Cryptogamic plants. This re-arrangement is based partly on morphological and partly on physiological grounds, and brings into strange fellowship plants which have heretofore been placed in quite different orders, as the following synopsis will show:

First Group. THALLOPHYTES.

Class I. PROTOPHYTES.

Forms containing chlorophyll.—A. Cyanophyceæ.—B. Palmellaceæ.

Forms devoid of chlorophyll.—A. Schizomycetes.—B. Fermentation-fungi.

Class II. ZYGOSPOREÆ.

Forms containing chlorophyll.—A. Volvocineæ.—B. Conjugatæ and Diatoms.

Forms devoid of chlorophyll.—A. Myxomycetes.—B. Zygomycetes.

Class III. OOSPOREÆ.

A. Sphæropleæ.—B. Cœloplastæ. Vaucheria, &c. Saprolegnia, Peronospora.—C. Œdogoniæ.—D. Fucaceæ.

Class IV. CARPOSPOREÆ.

Forms containing chlorophyll.—A. Coleochætæ.—B. Florideæ.—C. Characeæ.

Forms devoid of chlorophyll (or true Fungi.)—A. Ascomycetes.—B. Æcidiomycetes (Uredineæ.)—C. Basidiomycetes.

An examination of the foregoing synopsis will plainly show that Professor Sachs is largely influenced by the theory of descent. It is worth while to compare his views with those of Professor Fischer, as communicated by Dr. Brefeld (Lehrb., p. 248–9). After presenting a sketch of Fischer's new classification, Professor Sachs says: "Professor Fischer regards Fungi and Algæ as two completely separated series developing in parallel lines, while I assume that in each class Fungi have diverged as branches from the different types of Algæ.

In conclusion, the hope must be expressed that the forthcoming English translation of this admiriable work will be as worthy of honest praise as the French edition. G. L. G.

22. *Notice of some Fresh-Water and Terrestrial Rhizopods.*—Prof. LEIDY stated that among the amœboid forms noticed by him n the vicinity of Philadelphia, there was one especially remarkable or the comparatively enormous quantity of quartzose sand which it

swallowed with its food. The animal might be viewed as a bag of sand! It is a sluggish creature, and when at rest appears as an opaque white, spherical ball, ranging from $\frac{1}{8}$ to $\frac{3}{8}$ of a line in diameter. The animal moves slowly, first assuming an oval and then a clavate form. In the oval form one measured $\frac{3}{5}$ of a line long by $\frac{2}{5}$ of a line broad, and when it became clavate it was $\frac{2}{3}$ of a line long by $\frac{1}{3}$ of a line broad at the advanced thick end. Another, in the clavate form, measured $\frac{7}{8}$ of a line long by $\frac{1}{3}$ of a line wide at the thick end. The creature rolls or extends in advance, while it contracts behind. Unless under pressure it puts forth no pseudopods, and the granular entosarc usually follows closely on the limits of the extending ectosarc. Generally the animal drags after it a quantity of adherent dirt attached to a papillated or villous discoid projection of the body.

The contents of the animal, besides the granular matter and many globules of the entosarc, consists of diatoms, desmids, and confervæ, together with a larger proportion of angular particles of transparent and mostly colorless quartz. Treated with strong mineral acids so as to destroy all the soft parts, the animal leaves behind more than half its bulk of quartzose sand.

The species may be named **Amœba sabulosa**, and is probably a member of the genus *Pelomyxa*, of Dr. Greef (Archiv f. Mik. Anat., x, 1873, 51).

The animal was first found on the muddy bottom of a pond, on Dr. George Smith's place in Upper Darby, Delaware County, but has been found also in ponds in New Jersey.

When the animal was first noticed with its multitude of sand particles, it suggested the probability that it might pertain to a stage of life of *Difflugia*, and that by the fixation of the quartz particles in the exterior, the case of the latter would be formed. This is conjectural and not confirmed by any observation.

A minute amœboid animal found on *Spirogyra* in a ditch at Cooper's Point, opposite Philadelphia, is of interesting character. The body is hemispherical, yellowish, and consists of a granular entosarc with a number of scattered and well-defined globules, besides a large contractile vesicle. From the body there extends a broad zone, which is colorless and so exceedingly delicate that it requires a power of 600 diameters to see it favorably. By this zone the animal glides over the surface. As delicate as it is, it evidently possesses a regular structure, though it was not resolved under the best powers of the microscope. The structure probably consists of globular granules of uniform size alternating with one another, so that the disk at times appears crossed by delicate lines, and at others as if finely and regularly punctated. The body of the animal measures from $\frac{1}{66}$ to $\frac{1}{50}$ of a line in diameter, and the zone is from $\frac{1}{333}$ to $\frac{1}{200}$ of a line wide. The species may be named **Amœba zonalis.**

The interesting researches of Prof. Richard Greef, of Marburg, published in the second volume of Scholtze's Archiv f. Mikrokopische Anatomie, on Amœbæ living in the earth (*Ueber einige* *in der Erde lebende Amœben, etc.*), led me to look in similar positions for Rhizopods.

In the earth, about the roots of mosses growing in the crevices of the bricks of our city pavements, in damp places, besides finding several species of *Amœba*, together with abundance of the common wheel-animalcule, *Rotifer vulgaris*, I had the good fortune to discover a species of *Gromia*. I say good fortune, for it is with the utmost pleasure I have watched this curious creature for hours together. The genus was discovered and well described by Dujardin, from two species, one of which, *G. oviformis*, was found in the seas of France; the other, the *G. fluviatilis*, in the River Seine.

Imagine an animal, like one of our autumnal spiders, stationed at the center of its well-spread net; imagine every thread of this net to be a living extension of the animal, elongating, branching, and becoming confluent so as to form a most intricate net; and imagine every thread to exhibit actively moving currents of a viscid liquid both outward and inward, carrying along particles of food and dirt, and you have some idea of the general character of a *Gromia*.

The *Gromia* of our pavements is a spherical cream-colored body, about the $\frac{1}{16}$th of a line in diameter. When detached from its position and placed in water, in a few minutes it projects in all directions a most wonderful and intricate net. Along the threads of this net float minute Naviculæ from the neighborhood, like boats in the current of a stream, until reaching the central mass they are there swallowed. Particles of dirt are also collected from all directions and are accumulated around the animal, and when the accumulation is sufficient to protect it, the web is withdrawn and nothing apparently will again induce the animal to produce it.

From these observations we may suppose thet the GROMIA TERRICOLA, as I propose to name the species, during dry weather remains quiescent and concealed among accumulated dirt in the crevices of our pavements, but that in rains or wet weather the little creature puts forth its living net, which becomes so many avenues along which food is conveyed to the body. As the neighborhood becomes dry, the net is withdrawn to await another rain. The animal with its extended net can cover an area of nearly half a line in diameter. The threads of the net are less than the $\frac{1}{30000}$th of an inch in diameter.—*Proc. Acad. Nat. Sci. Phil.*, 1874, p. 88.

23. *Nature of the Sea-bottom.*—Prof. C. WYVILLE THOMSON has presented to the Royal Society a paper entitled Preliminary Notes on the nature of the Sea-bottom procured by the soundings of H. M. S. Challenger during her cruise in the Southern Sea in the early part of the year 1874, which are published in "Nature" for December and January. The following are extracts from it.

a. *Green-sand.* In the region of the Agulhas Bank, off the south extremity of Africa, from 98 and 150 fathoms, a greenish sand was brought up which was found to consist of casts of foraminifers in a glauconite-like material. The genera included Miliola, Biloculina, Uvigerina, Planorbulina, Rotalia, Textularia, Bulimina and Nummulina, and, of much less abundance, Globigerina, Orbulina and Pulvinulina.

b. *Globigerina, gray and red ooze.* From Cape Agulhas to lat. 46° 16′, the greatest depth was 1,900 fathoms; and the bottom was Globigerina ooze, it consisting mostly of the shells of Globigerina, more or less broken, with a few of Pulvinulina and Orbulina.

The limit of the pure Globigerina ooze is near 2,250 fathoms. Passing to greater depths, the material of the bottom is first a "gray ooze," and then generally for all depths below 2,500 or 3,000 fathoms it is the "red ooze" or clay, which consists of a silicate of alumina and red oxide of iron. The transition is gradual, the shells first losing their sharpness of outline and becoming mixed with more or less of red-brown amorphous powder, which is so fine as to take days to settle. In the section between Teneriffe and St. Thomas, at the first four casts, the Globigerina ooze was obtained at depths from 1,525 to 2,220 fathoms; then at a depth of 2,740 fathoms, 500 miles from Teneriffe, and of 2,950 fathoms, 750 miles off, the ooze is gray but is largely the red clay mixed with some of the calcareous; and at 3,150 fathoms, 1,150 miles from Teneriffe, the bottom was a pure and smooth clay with scarcely a trace of lime. From this great depth the bottom rises, and the gray color and calcareous nature of the ooze returns, soundings in 2,050, 1900 and 1950 fathoms on the "Dolphin Rise" bringing up the Globigerina ooze. But passing from the middle plateau of the Atlantic into the western trough, where the depth is a little over 3,000 fathoms, the red clay returned in all its purity.

Prof. Thomson states that Mr. Murray, after a careful study of all the facts, has come to the conclusion, presented years ago by Professor Bailey, that the Rhizopods are not living in the deep ocean, but belong to the superficial waters. Facts prove that they are very abundant in these upper waters, that the shells have a perfection in the living state that is not found in the deep water specimens. With regard to the red ooze, which is almost destitute of life, the opinion is put forth that beyond a certain depth the shells of the Globigerinæ and other Rhizopods are dissolved, and that the alumina and iron which experiment has proved they contain are the source of the broad and barren regions of clay.

24. *Index to Volumes I to XIII of Observations on the Genus Unio*, together with descriptions of new species of the Family Unionidæ, and descriptions of new species of the Melanidæ, Paludinæ and Helicidæ, etc.; by Isaac Lea, LL.D. Vol. III.—This is a supplementary Index to the previous Indexes published in 1867 and 1869, giving what was omitted in the others, references to anatomical and structural details, habits, etc.

25. *Reliquiæ Aquitanicæ*, being Contributions to the Archæology and Palæontology of Périgord and the adjoining Provinces of Southern France, by E. Lartet and H. Christy, edited by Thomas Rupert Jones, F.R.S., &c.—Part xv of this valuable work, for September, 1874, contains pages 205–224, 173 to 182, and Plate A xlii.

III. ASTRONOMY.

1. *The Transit of Venus, December 8th.*—Telegrams announce successful observations of the Transit at Yokohama, Nagasaki, Vladivostock, Teheran, Cairo, Suez and Thebes, at points in India, China, Adelaide and Melbourne, and less perfect at Hobart Town. One hundred photographs are reported from India, and thirteen from that at Vladivostock. Failures are reported at Shanghai, and at the Russian stations of Omsk, Orenburg, Kasan, Uralsk, Astrachan, Kertch, and Tiflis. Twenty other Russian stations, Kerguelen's Land, the Sandwich Islands, Mauritius, and many other points, are yet to be heard from. The probabilities are that greater success has been attained, on the whole, than could reasonably have been expected, and that the successes have, in the main, been at the more important stations for determining the parallax.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Exploration in South America.*—Prof. ALEXANDER AGASSIZ left the country near the close of November, on a trip to South America, especially to Chili and Bolivia. At Lake Titicaca he expects to dredge, besides making temperature observations. Mr. Garman, of the Museum of Comparative Zoology, accompanied him, and will go directly to Lake Titicaca to collect. Mr. Agassiz's health has been poor, since the great afflictions which befel him in the death of his father and, almost immediately after, that of his gifted wife. It is now somewhat improved, and it is believed that the journey will make the restoration complete.

2. *Observatory at Cordoba, Argentine Republic.*—Dr. B. A. GOULD, the astronomer, after a brief visit to his home and friends at Boston, has left again for Cordoba, to resume his great work there with regard to the southern constellations, which is now far advanced toward completion.

3. *Cañons of the Colorado.*—Major J. W. POWELL, the explorer of the Cañons of the Colorado, has a valuable article on the Colorado Basin and its Cañons in the January number of Scribner's Monthly. With the usual liberality of this excellent Monthly, the article is illustrated by many fine engravings, from the photographs taken by the artist of Powell's expedition.

4. *Statistical Atlas of the United States;* by FRANCIS A. WALKER. Authorized by the Act of Congress of March 3, 1873.—The great variety of valuable material locked up in a Census Report can be brought out for use only through laborious and critical study on the part of one who understands fully the philosophical bearing of the facts. Such a study has been made of the returns of the recent United States Census by Mr. Walker. His Statistical Atlas, Parts II. and III. of which have recently been published, presents to the eye the chief results arrived at, and attests, in every part, to the learning, judgment, and thoroughness of the author. By means of maps and diagrams the great facts in political and social science are made readily and impressively

intelligible in all their relations. Part II. embraces the department of *Social and Industrial Statistics.* The charts show, by colors, the growth of the United States in population from 1790, when the first census was taken, through eight decades to 1870; also the center of population for the different periods; the constituents of the population of each State at the present time; the distribution, absolute and relative, of the colored population; of foreigners and persons of foreign birth; as to illiteracy; also the distribution of wealth over the country; of public indebtedness; of taxation; of the most important crops; the relative amount of church and school accommodation; and so on. Part III. covers the *Vital Statistics*, and contains eighteen charts illustrating the distribution of population by age and sex; of native and foreign population; prevalence of various diseases for the two sexes, in different parts of the country and for the successive seasons of the year; relative birth rate; relative death rate for different diseases; relative number of blind, deaf, insane and idiotic persons, with the rate of increase or decrease for the decade from 1860 to 1870, and many other topics. The charts with reference to the blind and idiocy were prepared by F. H. Wines.

Part I, devoted to the *Physical Features of the United States*, is now in the press.

5. *Cave Hunting: Researches on the evidence of Caves respecting the early inhabitants of Europe*; by W. Boyd Dawkins, M.A., F.R.S., F.G.S., Curator of the Museum and Lecturer in Geology in the Owens College, Manchester. 456 pp. 8vo. Illustrated by a colored plate and many woodcuts. London, 1874. (Macmillan & Co.)—The author of this new volume on British and European Caves is one of the most prominent of British geologists, and one who has made a special study of the caves and the cave animals. The first and second chapters treat chiefly of the history of cave exploration, and the physical history of caves. Next follows chapters on the historic caves of Britain and their relics; caves used in the ages of Iron and Bronze; caves of the neolithic age; Range of neolithic dolicho-cephalic and brachy-cephalic men; Pleistocene caves of Germany and Great Britain; evidence from the cave inhabitants as to the Atlantic and Mediterranean coast line in the Pleistocene, illustrated by a map; the European climate in the Pleistocene; and on the instruments and methods of cave-hunting. Prof. Dawkins recognizes the existence of two Glacial eras in Great Britain: the older, or that commonly so called, one of greater elevation than now, was followed by an era of subsidence; and this by another era of elevation and glaciers, but of less extent of ice than in the first glacial era. The illustrations are numerous and excellent, and the work, while exact and thorough in its science, is popular in style and full of interest to all classes of readers.

Report of the Commissioner of Agriculture for 1873; 496 pp. 8vo. Washington, 1874.

Recent change of level on the Coast of Maine; by N. S. Shaler. 20 pp. 4to. From the Memoirs of the Boston N. H. Society. Volume II, Part 3, No. 3.

OBITUARY.

Francis Walker.—An exception to the precedent of speaking well of the dead is offered by the Entomologist's Monthly Magazine in its obituary notice of the late Francis Walker as an entomologist. And we who have written of Mr. Walker's work, after having examined his material in the British Museum Collection, can only endorse what is actually a repetition, in other words, of our statements in the Transactions of the American Entomological Society for July, 1868. With regard to the responsibility for the indifferent work in the British Museum lists, this lies, as the writer in the Entomologist's Monthly Magazine says, and as we had previously pointed out, with the authorities in the British Museum who authorized the publication of these lists. As their preface gives their authority, their publication is due to Mr. John Edward Gray, to whom science is so widely indebted that this error in judgment on his part must be condoned. For our part, from first to last, so far as our special branch is concerned, we have steadily opposed any total rejection of Mr. Walker's works as affording a bad precedent, and this contrary to the opinions of those yet eminent entomologists in whom we have learned to have confidence. At this moment, when a kind and amiable man has left us, we renew our appeal made in 1868, that Mr. Walker's material in the British Museum Collection be worked over by a competent hand. In this work reference should be had to the specimens and the descriptions, and every *tenable* name should be fixed, all doubtful names, founded on irrecognizable specimens, should be withdrawn, and in this way Mr. Walker's reputation would be saved from further obloquy, the ends of science furthered and the authorities of the British Museum atone amply for their mistake. Private collectors, who have Mr. Walker's types, would heartily join, doubtless, in a work which would be to their advantage, and a miserable scandal would be done away with forever. This, and so desirable a consummation, remains to be gained by the proper authorities of the institution which allowed the fault to be committed. What greater work have they in prospect that this duty shall be set aside? We close this paper with a kindly remembrance of the dead, and with the words we first used on the occasion of appearing as Mr. Walker's critic, "trusting that nothing we have ever said can be taken as reflecting personally upon a gentleman, whose courtesy and the extent of whose literary labors invite every consideration." A. R. GROTE.

Edwin Lankester.—Dr. Lankester, well known for his zoological publications, died October 30th, at the age of sixty. He was elected a fellow of the Royal Society in 1845. For eighteen years he has edited, in conjunction with Mr. Busk, the Quarterly Journa of Microscopic Science.

Dr. **Thomas Anderson**, late Professor of Chemistry in the University of Glasgow, died at Chiswick on the 2d of November He was born in 1819.

Sir William Jardine, the zoologist, and especially distinguished for his labors in Ornithology, died at Sandown, in th Isle of Wight, on the 21st of November, aged 74.

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[THIRD SERIES.]

Art. X.—*Jeffries Wyman. Address of Professor* A. Gray at the Memorial Meeting of the Boston Society of Natural History, Oct. 7, 1874.*

When we think of the associate and friend whose death this society now deplores, and remember how modest and retiring he was, how averse to laudation and reticent of words, we feel it becoming to speak of him, now that he is gone, with much of the reserve which would be imposed upon us if he were living. Yet his own perfect truthfulness and nice sense of justice, and the benefit to be derived from the contemplation of such a character by way of example, may be our warrant for reasonable freedom in the expression of our judgments and our sentiments, taking care to avoid all exaggeration.

Appropriate and sincere eulogies and expressions of loss, both official and personal, have, however, already been pronounced or published; and among them one from the governors of that institution to which, together with our own society, most of Professor Wyman's official life and services were devoted,—which appears to me to delineate in the fewest words the truest outlines of his character. In it the President and Fellows of Harvard University "recall with affectionate respect and admiration the sagacity, patience and rectitude which characterized all his scientific work, his clearness, accuracy and conciseness as a writer and teacher, and the industry and zeal with which he labored upon the two admirable collections which remain as monuments of his rare knowledge, method and skill. They commend to the young men of the university this signal example of a character modest, tranquil, dignified and independent, and of a life simple, contented and honored."

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* From the "Proceedings" of the Society.

What more can be or need be said? It is left for me, in compliance with your invitation, Mr. President, to say something of what he was to us, and has done for us, and to put upon record, for the use of those who come after us, some account of his uneventful life, some notice, however imperfect, of his work and his writings. I could not do this without the help of friends who knew him well in early life, and of some of you who are much more conversant than I am with most of his researches. Such aid, promptly rendered, has been thankfully accepted and freely used.

Our associate's father, Dr. Rufus Wyman,—born in Woburn, graduated at Harvard College in 1799, and in the latter part of his life physician to the McLean Asylum for the Insane,—was a man of marked ability and ingenuity. Called to the charge of this earliest institution of the kind in New England at its beginning, he organized the plan of treatment and devised the excellent mechanical arrangements which have since been developed, and introduced into other establishments of the kind. His mother was Ann Morill, daughter of James Morill, a Boston merchant. This name is continued, and is familiar to us, in that of our associate's elder brother.

JEFFRIES WYMAN, the third son, derived his baptismal name from the distinguished Dr. John Jeffries, of Boston, under whom his father studied medicine. He was born on the 11th of August, 1814, at Chelmsford, a township of a few hundred inhabitants in Middlesex County, Mass., not far from the present city of Lowell. As his father took up his residence at the McLean Asylum in 1818, when Jeffries was only four years old, he received the rudiments of his education at Charlestown, in a private school; but afterward went to the Academy at Chelmsford, and, in 1826, to Phillips Exeter Academy, where, under the instruction of Dr. Abbot, he was prepared for college. He entered Harvard College in 1829, the year in which Josiah Quincy took the presidency, and was graduated in 1833, in a class of fifty-six, six of whom became professors in the university. He was not remarkable for general scholarship, but was fond of chemistry, and his preference for anatomical studies was already developed. Some of his class-mates remember the interest which was excited among them by a skeleton which he made of a mammoth bull-frog from Fresh Pond, probably one which is still preserved in his museum of comparative anatomy. His skill and taste in drawing, which he turned to such excellent account in his investigations and in the lecture room, as well as his habit of close observation of natural objects met with in his strolls, were manifested even in boyhood.

An attack of pneumonia during his Senior year in college caused much anxiety, and perhaps laid the foundation of the pulmonary affection which burdened and finally shortened his life. To recover from the effects of the attack, and to guard against its return, he made, in the winter of 1833–34, the first of those pilgrimages to the coast of the Southern States, which in later years were so often repeated. Returning with strength renewed in the course of the following spring, he began the study of medicine under Dr. John C. Dalton, who had succeeded to his father's practice at Chelmsford, but who soon removed to the adjacent and thriving town of Lowell. Here, and with his father at the McLean Asylum, and at the Medical College in Boston, he passed two years of profitable study. At the commencement of the third year he was elected house-student in the Medical Department, at the Massachusetts General Hospital,—then under the charge of Doctors James Jackson, John Ware and Walter Channing,—a responsible position, not only most advantageous for the study of disease, but well adapted to sharpen a young man's power of observation.

In 1837, after receiving the degree of Doctor of Medicine, he cast about among the larger country towns for a field in which to practice his profession. Fortunately for science, he found no opening to his mind; so he took an office in Boston, on Washington street, and accepted the honorable, but far from lucrative post of Demonstrator of Anatomy under Dr. John C. Warren, the Hersey Professor. His means were very slender, and his life abstemious to the verge of privation; for he was unwilling to burden his father, who, indeed, had done all he could in providing for the education of two sons. It may be interesting to know that, to eke out his subsistence, he became at this time a member of the Boston Fire Department, under an appointment of Samuel A. Eliot, Mayor, dated Sept. 1, 1838. He was assigned to Engine No. 18. The rule was that the first-comer to the engine-house should bear the lantern, and be absolved from other work. Wyman lived near by, and his promptitude generally saved him from all severer labor than that of enlightening his company.

The turning point in his life, i. e., an opportunity which he could seize of devoting it to science, came when Mr. John A. Lowell offered him the curatorship of the Lowell Institute, just brought into operation, and a course of lectures in it. He delivered his course of twelve lectures upon Comparative Anatomy and Physiology in the winter of 1840–41; and with the money earned by this first essay in instructing others, he went to Europe to seek further instruction for himself. He reached Paris in May, 1841 and gave his time at once to Human Anatomy at the School of Medicine, and Comparative Anatomy and Natural History at the Garden of Plants, attending the lectures of Flourens, Majendie and Louget on Physiology, and of de Blainville, Isidore St. Hilaire, Valenciennes, Dumeril and Milne-Edwards on Zoology and Comparative Anatomy. In the summer, when the lectures were over, he made a pedestrian journey along the banks of the Loire, and another along the Rhine, returning through Belgium, and by steamer to London. There, while engaged in the study of the Hunterian collections at the Royal College of Surgeons, he received information of the alarming illness of his father; he immediately turned his face homeward, but on reaching Halifax he learned that his father was no more.

He resumed his residence in Boston, and devoted himself mainly to scientific work, under circumstances of no small discouragement. But in 1843 the means of a modest professional livelihood came to him in the offer of the chair of Anatomy and Physiology in the medical department of Hampden-Sidney College, established at Richmond, Virginia. One advantage of this position was that it did not interrupt his residence in Boston, except for the winter and spring; and during these months the milder climate of Richmond was even then desirable. He discharged the duties of the chair most acceptably for five sessions, until, in 1847, he was appointed to succeed Dr. Warren as Hersey Professor of Anatomy in Harvard College, the Parkman professorship in the Medical School in Boston being filled by the present incumbent, Dr. Holmes. Thus commenced Prof. Wyman's most useful and honorable connection as a teacher with the university, of which the President and Fellows speak in the terms I have already recited. He began his work in Holden Chapel, the upper floor being the lecture-room, the lower containing the dissecting-room and the anatomical museum of the college, with which he combined his own collections and preparations, which from that time forward increased rapidly in number and value under his industrious and skillful hands. At length Boylston Hall was built for the anatomical and the chemical departments, and the museum, lecture and working-rooms were established commodiously in their present quarters; and Prof. Wyman's department assumed the rank and the importance which it deserved. Both human and comparative anatomy were taught to special pupils, some of whom have proved themselves worthy of their honored master, while the annual courses of lectures and lessons on Anatomy, Physiology, and for a time the principles of Zoology, imparted highly valued instruction to undergraduates and others.

In the formation and perfecting of his museum—the first of the kind in the country, arranged upon a plan both physiological and morphological—no pains and labors were spared, and long and arduous journeys and voyages were made to contribute to its riches. In the summer of 1849,—having replenished his frugal means with the proceeds of a second course of lectures before the Lowell Institute (viz., upon Comparative Physiology, a good condensed short-hand report of which was published at the time),—he accompanied Capt. Atwood of Provincetown, in a small sloop, upon a fishing voyage high up the coast of Labrador; in the winter of 1852, going to Florida for his health, he began his fruitful series of explorations and collections in that interesting district. In 1854, accompanied by his wife, he traveled extensively in Europe, and visited all the museums within his reach. In the spring of 1856, with his pupils, Green and Bancroft, as companions and assistants, he sailed to Surinam, penetrated far into the interior in canoes, made important researches upon the ground, and enriched his museum with some of its most interesting collections. These came near being too dearly bought, as he and his companions took the fever of the country, from which he suffered severely, and recovered slowly. Again, in 1858–9, accepting the thoughtful and generous invitation of Capt. J. M. Forbes, he made a voyage to the La Plata, ascended the Uraguay and the Parana in a small iron steamer which Capt. Forbes brought upon the deck of his vessel; then, with his friend George Augustus Peabody as a companion, he crossed the pampas to Mendoza, and the Cordilleras to Santiago and Valparaiso, whence he came home by way of the Peruvian coast and the Isthmus.

By such expeditions many of the choice materials of his museum and of his researches were gathered, at his own expense, to be carefully prepared and elaborated by his own unaided hands. A vast neighboring museum is a splendid example of what munificence, called forth by personal enthusiasm, may accomplish. In Dr. Wyman's we have an example of what one man may do unaided, with feeble health and feebler means, by persistent and well-directed industry, without eclât, and almost without observation. While we duly honor those who of their abundance cast their gifts into the treasury of science, let us not—now that he cannot be pained by our praise—forget to honor one who in silence and penury cast in more than they all.

Of penury in a literal sense we may not speak; for although Prof. Wyman's salary, derived from the Hersey endowment, was slender indeed, he adapted his wants to his means, foregoing neither his independence nor his scientific work; and I suppose no one ever heard him complain. In 1856 came unexpected and honorable aid from two old friends of his father who appreciated the son, and wished him to go on with his scientific work without distraction. One of them, the late Dr. William J. Walker, sent him ten thousand dollars outright; the other, the late Thomas Lee, who had helped in his early education, supplemented the endowment of the Hersey professorship with an equal sum, stipulating that the income thereof should be paid to Prof. Wyman during life, whether he held the chair or not. Seldom, if ever, has a moderate sum produced a greater benefit.

Throughout the later years of Prof. Wyman's life a new museum has claimed his interest and care, and is indebted to him for much of its value and promise. In 1866, when failing strength demanded a respite from oral teaching, and required him to pass most of the season for it in a milder climate, he was named by the late George Peabody one of the seven trustees of the Museum and Professorship of American Archæology and Ethnology, which this philanthropist proceeded to found in Harvard University; and his associates called upon him to take charge of the establishment. For this he was peculiarly fitted by all his previous studies, and by his predilection for ethnological inquiries. These had already engaged his attention, and to this class of subjects he was thereafter mainly devoted,—with what sagacity, consummate skill, untiring diligence and success, his seven annual Reports—the last published just before he died,—his elaborate memoir on shell-heaps, now printing, and especially the Archæological Museum in Boylston Hall, abundantly testify. If this museum be a worthy memorial of the founder's liberality and foresight, it is no less a monument of Wyman's rare ability and devotion. Whenever the enduring building which is to receive it shall be erected, surely the name of its first curator and organizer should be inscribed, along with that of the founder, over its portal.

Of Prof. Wyman's domestic life, let it here suffice to record, that in Dec., 1850, he married Adeline Wheelwright, who died in June, 1855, leaving two daughters; that in August, 1861, he married Anna Williams Whitney, who died in February, 1864, shortly after the birth of an only and a surviving son.

Of his later days, of the slow, yet all too rapid progress of fatal pulmonary disease, it is needless to protract the story. Winter after winter, as he exchanged our bleak climate for that of Florida, we could only hope that he might return. Spring after spring he came back to us invigorated, thanks to the bland air and the open life in boat and tent, which acted like a charm; —thanks, too, to the watchful care of his attached friend, Mr. Peabody, his constant companion in Florida life. One winter was passed in Europe, partly in reference to the Archæological Museum, partly in hope of better health; but no benefit was received. The past winter in Florida produced the usual amelioration, and the amount of work which Dr. Wyman undertook and accomplished last summer might have tasked a robust man. There were important accessions to the archæological collections, upon which much labor, very trying to ordinary patience, had to be expended. And in the last interview I had with him, he told me that he had gone through his own museum of comparative anatomy, which had somewhat suffered in consequence of the alterations in Boylston Hall, and had put the whole into perfect order. It was late in August when he left Cambridge for his usual visit to the White Mountain region, by which he avoided the autumnal catarrh; and there, at Bethlehem, New Hampshire, on the 4th of September, a severe hemorrhage from the lungs suddenly closed his valuable life.

Let us turn to his relations with this society. He entered it in October, 1837, just thirty-seven years ago, and shortly after he had taken his degree of Doctor in Medicine. He was Recording Secretary from 1839 to 1841; Curator of Ichthyology and Herpetology from 1841 to 1847, of Herpetology from 1847 to 1855, of Comparative Anatomy from 1855 to 1874. While in these later years his duties may have been almost nominal, it should be remembered that in the earlier days a curator not only took charge of his portion of the Museum, but in a great degree created it. Then for fourteen years, from 1856 to 1870, he was the president of this society, as assiduous in all its duties as he was wise in council; and he resigned the chair which he so long adorned and dignified only when the increasing delicacy of his health, to which night-exposure was prejudicial, made it unsafe for him any longer to undertake its duties. The record shows that he has made here one hundred and five scientific communications,* several of them very important papers, every one of some positive value; for you all know that Prof. Wyman never spoke or wrote except to a direct purpose, and because there was something which it was worth while to communicate. He bore his part also in the American Academy of Arts and Sciences, of which he was a Fellow from the year 1843, and for many years a Councillor. To it he made a good number of communications; among them one of the longest and ablest of his memoirs.

Then he was from the first a member of the Faculty of the Museum of Comparative Zoology, where his services and his advice were highly valued. He was chosen president of the American Association for the Advancement of Science for the year 1857, but did not assume the duties of the office.

Some notice—brief and cursory though it must be—of such portion of Dr. Wyman's scientific work as is recorded in his published papers, should form a part of this account of his life

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* The Royal Society's Catalogue of Scientific Papers enumerates sixty-four by Prof. Wyman alone, and four in conjunction with others.

His earliest publication, so far as we know, was an article in the Boston Medical and Surgical Journal, in 1837, signed only with the initials of his name. It is upon "The indistinctness of images formed from oblique rays of light," and the cause of it. The handling of the subject is as characteristic as that of any later paper. In January, 1841, we find his first recorded communication to this society, "On the Cranium of a Seal." The first to the American Academy is the account of his dissection of the electrical organs of a new species of Torpedo, in 1843, part of a paper by his friend Dr. Storer, published in the American Journal of Science. In the course of that year, eleven communications were made to our society, besides the Annual Address, which he delivered on the 17th of May. The most important of these was the memoir, by Dr. Savage and himself, on the Black Orang or Chimpanzee of Africa, *Troglodytes niger*, published in full in the Journal of this society, the anatomical part by Professor Wyman. Two other papers of that early year, on the Anatomy of two Mollusca, *Tebennophorus Carolinensis* and *Glandina truncata*, published in the fourth volume of the society's Journal, each with a copper plate, are noteworthy, as showing that he possessed from the first that happy faculty of clear, terse, and closely relevant exposition, and that skill and neatness of illustration with his pencil, which characterize all his work, both of research and instruction.

Another paper of that year, "On the microscopic structure of the teeth of the *Lepidostei*, and their analogies with those of the *Labyrinthodonts*," read to this society in August, and published in this Journal in October, 1843, was important and timely. In it he demonstrated that the labyrinthine structure of the teeth, considered at the time to be peculiar to certain sauroid reptiles, equally belonged to the gar-fishes, and consequently that many fossil teeth which had been referred by the evidence of this character alone to a group of reptiles founded upon this peculiarity, might as well belong to ancient sauroid fishes.

Although not of any importance now to remember, I may here mention his report to this society on the *Hydrarchos Sillimani* of Koch, a factitious Saurian of huge length, successfully exhibited in New York and elsewhere under high auspices, and I think also in Germany, but which Dr. Wyman exposed at sight, showing that it was made up of an indefinite number of various cetacean vertebræ, belonging to many individuals, which (as was afterward ascertained) were collected from several localities.

But the memoir by which Professor Wyman assured his position among the higher comparative anatomists was that communicated to and published by this society in the summer of 1847, in which the Gorilla was first named and introduced to the scientific world, and the distinctive structure and affinities of the animal so thoroughly made out from the study of the skeleton, that there was, as the great English anatomist remarked, "very little left to add, and nothing to correct." In this memoir the "Description of the habits of *Troglodytes Gorila*" is by Dr. Thomas S. Savage, to whom, along with Dr. Wilson, "belongs the credit of the discovery;" the osteology of the same and the introductory history are by Dr. Wyman. Indeed, nearly all since made known of the Gorilla's structure, and of the affinities soundly deduced therefrom, has come from our associate's subsequent papers, founded on additional crania brought to him in 1849, by Dr. George A. Perkins of Salem; on a nearly entire male skeleton of unusual size, received in 1852, from the Rev. William Walker, and now in Wyman's museum; and on a large collection of skins and skeletons placed at his disposal in 1859 by Du Chaillu, along with a young Gorilla in spirits, which he dissected. It is in the account of this dissection that Professor Wyman brings out the curious fact that the skull of the young Gorilla and Chimpanzee bears closer resemblance to the adult than to the infantile human cranium.

In Professor Wyman's library, bound up with a quarto copy of the Memoir by Dr. Savage and himself, is a terse but complete history of this subject, in his neat and clear hand-writing, and with copies of the letters of Dr. Savage, Professor Owen, Mr. Walker, and M. du Chaillu.

In the introductory part of the Memoir, Professor Wyman states that "the specific name, *Gorilla*, has been adopted, a term used by Hanno in describing the wild men found on the coast of Africa, probably one of the species of the Orang." The name *Troglodytes Gorilla* is no doubt to be cited as of Savage and Wyman, and it was happily chosen by Professor Wyman, after consultation with his friend, the late Dr. A. A. Gould, for the reason just stated. But it is interesting to see, in the correspondence before me, how strenuously each of the joint authors deferred to the other the honor of nomenclature. Dr. Savage from first to last insists, in repeated and emphatic terms, that the scientific name shall be given by Dr. Wyman as the scientific describer, and that he could not himself honestly appropriate it. Professor Wyman, in his MSS. account, after mentioning what his portion of the Memoir was, and that "the determination of the differential characters on which the establishment of the species rests was prepared by me," briefly and characteristically adds: "In view of this last fact, Dr. Savage thought, as will be seen in letter, that the species should stand in my name; but this I declined."

This Memoir was read before this society on the 18th of August, 1847, and was published before the close of the year. But it had not, as it appears, come to Professor Owen's knowledge when the latter presented to the London Zoological Society, on the 22d of February, 1848, a memoir founded on three skulls of the same species, just received from Africa through Captain Wagstaff. When Professor Owen received the earlier Memoir, he wrote to compliment Professor Wyman upon it, substituted in a supplementary note the specific name imposed by Savage and Wyman, and reprinted in an appendix the osteological characters set forth by the latter. "It does not appear, however (adds Dr. Wyman), either in the Proceedings or the Transactions of the (Zoological) Society at what time our Memoir was published, nor that we had anticipated him in our description."

It is safe to assert that in this and the subsidiary papers of Dr. Wyman may be found the substance of all that has since been brought forward, bearing upon the osteological resemblances and differences between men and apes. After summing up the evidence, he concludes:—

"The organization of the anthropoid Quadrumana justifies the naturalist in placing them at the head of the brute creation, and placing them in a position in which they, of all the animal series, shall be nearest to man. Any anatomist. however, who will take the trouble to compare the skeletons of the Negro and Orang, cannot fail to be struck at sight with the wide gap which separates them. The difference between the cranium, the pelvis, and the conformation of the upper extremities in the Negro and Caucasian, sinks into comparative insignificance when compared with the vast difference which exists between the conformation of the same parts in the Negro and the Orang. Yet it cannot be denied, however wide the separation, that the Negro and Orang do afford the points where man and the brute, when the totality of their organization is considered, most nearly approach each other."

Selecting now for further comment only some of the more noticeable contributions to science, we should not pass by his investigations of the anatomy of the Blind Fish of the Mammoth Cave. The series began, in that prolific year, 1843, with a paper published in this Journal, and closed with an article in the same Journal in 1854. Although Dr. Tellkamph had preceded him in ascertaining the existence of rudimentary eyes and the special development of the fifth pair of nerves, yet for whole details of the subject, and the minute anatomy, we are indebted to Professor Wyman. Many of the details, however, as well as the admirable drawings illustrating them, remained unpublished until 1872, when he placed them at Mr. Putnam's disposal, and they were brought out in his elaborate article in the "American Naturalist." Here the extraordinary development of tactile sense, taking the place of vision, and perfectly adapting the animal to its subterranean life, is completely demonstrated.

If Professor Wyman's first piece of anatomical work was the preparation of a skeleton of a bull-frog, in his undergraduate days, his most elaborate memoir is that on the anatomy of the nervous system of the same animal (*Rana pipiens*), published in the "Smithsonian Contributions," in 1852 (51 pages, royal 4to, with 2 plates).

Anything like an analysis of this capital investigation and exposition would much overpass our limits. For, although the special task he assigns to himself is the description of the nervous system of a single Batrachian, chiefly of its peripheral portion, and of the changes undergone during metamorphosis, he is led on to the consideration of several abstruse or controverted questions; such, for instance, as the attempts that have been made to homologize the nervous system of Articulates with that of Vertebrates, upon which he has some acute criticism; the theories that have been propounded respecting the functions of the cerebellum and its relation to locomotion, which he tests in a characteristic way by a direct appeal to facts; the supposition of Cuvier that the special enlargements of the spinal cord are in proportion to the force of the respective limbs supplied therefrom; which he controverts decisively by similar appeal, an extract from which I beg leave to append in a note.*

So, in describing the structure of the optic nerves in the frog, and the development of the eye and optic lobes, he proceeds to remark, that—

"The instances of *Proteus* and *Amblyopsis* naturally suggest the questions, whether one and the same part may not combine functions wholly different in different animals, and whether the same may not hold true with regard to the cerebral organs which is known to obtain with regard to the skeleton, the teeth, the tongue, and the nose, that identical or homologous parts in different animals may perform functions wholly distinct. If the doctrine here suggested can be admitted (and if this were the place, facts could be cited in support of it), may we not find in it an explanation of many inconsistences which now exist between the results of comparative anatomy and of physiology?"

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* "If by force is meant the muscular energy and development of the limbs, this statement does not appear to be sustained in the present instance, nor in many other instances brought to notice by comparative anatomy. In man the branchial enlargement is always larger than the crural, though the legs are so much more powerfully developed than the arms, and the same is true of the greater number of mammals. In frogs there is a still greater disproportion between legs and arms, yet there is not a corresponding difference in the size of the bulgings. They cannot, therefore, be said to be in proportion to the muscular force only of the limbs, but correspond far more nearly to the acuteness of the sense of touch, which in man and mammals is more delicate in the hands and arms than in the legs and feet. In bats, it is true that the muscular force of the arms is greater than that of the legs, and that the branchial far surpasses the crural enlargement; but, at the same time, the sense of touch in the membranes of the wings is exalted to a most extraordinary degree. In birds the posterior bulging is almost universally the largest, though this condition is in part dependent upon the presence of the rhomboidal sinus. In these animals, while the muscular energy of the wings is the most developed, the sensibility of the feet is the more acute."

Then, in his chapter on the philosophical anatomy of the cranial nerves and skull, after showing that there are but three pairs of cranio-spinal nerves, he takes up the controverted question as to the number of vertebræ which compose the skull, and supports the opinion that they also are only three in a characteristic manner.*

Of this whole memoir it is thought that, notwithstanding the great advance which has been made in comparative anatomy during the twenty-five years which have elapsed since it was published, its importance to the student has not at all diminished.

Next to this in extent and value may be ranked Prof. Wyman's paper on the development of the common skate of our waters (*Raia Batis*), communicated to the American Academy in 1864, and published among its Memoirs. It gives an account of the peculiar egg-case of the Selachians, and of the several stages of the development of the embryo skate, expressed in the concise and clear language—as little technical as possible—for which he was distinguished, and leading up to not a few problems in comparative anatomy, morphology, or systematic zoology,—problems which Professor Wyman never evaded when they came directly in his way, and seldom handled without making some real contribution to their elucidation. For instance, in describing the external branchial fringes of the young skate, he notes the agreement in this character with the Batrachians; and in studying the seven branchial fissures of the embryo, he is brought into contact with the view of Huxley, that the formation of the external ear is by involution of the integument. After confirming the contrary observations of Reichert, on the embryo pig, he concludes that "the first of the seven branchial fissures of the embryo skate is converted into the spiracle, which is the homologue of the Eustachian tube and the outer ear-canal." After a full discussion of the homology of the upper jaw in sharks and skates, under the light afforded by his investigation of the embryo skate, he suggests that the cartilage which extends from the olfactory fossæ toward the pectoral fin is the probable homologue of a maxillary bone, and that in the lobe the homologue of an intermaxillary; that if so, the skates and proteiform reptiles agree in having the nostrils open in front of the dental arch; that while in all Batrachians the nasal groove becomes closed, in the skate it remains permanently open; and finally that this view, if confirmed, "will add another feature which justifies Owen, Agassiz and others, in dissenting from Cuvier so far as to give the Selachians a place in the zoological series higher than that of the bony fishes. But at the same time, it will give corroborative proof of the correctness of Cuvier's view, that 'the rudiments of the maxillaries, and intermaxillaries, ... are evident in the skeleton.'"

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* "The conclusions which have been drawn from the statements made above are as follows: that in frogs the *vagus* comprises the glosso-pharyngeal and accessory nerves; that the *trigeminus* comprises the facial, the abducens, and in the salamanders the patheticus and portions of the motor communis; that other evidence sustains the hypothesis, that the whole of the motor communis is a dependence of the trigeminus; if to these we add the *hypoglossus* (which in frogs is exceptionally a spinal nerve), we shall have three pairs of cranial nerves, each having all the characters of a common spinal nerve, namely, motor and sensitive roots and a ganglion; that there are no nerves to indicate a fourth vertebra, unless the special sense nerves are considered; if these are admitted as indications, then we must presuppose either two pairs of nerves to each vertebra, or the existence of six vertebræ, which is a larger number than can be accounted for on an osteological basis. The functions and mode of development of the special sense nerves we have taken as affording sufficient grounds for considering them as of a peculiar order, and not to be classed with common spinal nerves."

[To be continued.]

ART. XI.—*On some Points in the Geology of the Blue Ridge of Virginia;* by WM. M. FONTAINE.

[Concluded from page 22.]

MY next examination was made in the vicinity of Lynchburg, sixty miles southwest of this point. There the James River, and the canal along its banks, cut the strata nearly at right angles to their strike, thus affording unsurpassed facilities for the study of the country. In order that some idea may be had of the nature of the very considerable area lying between Charlottesville and Lynchburg, I give here a section made by Prof. Rogers, across the valley, about twenty miles northeast of Lynchburg. This is the only connected examination, so far as I know, that has ever been made across this part of the valley, which includes a belt sixty miles long and twenty wide. Of course, many variations from this section are to be expected, in such an extended area. Commencing at the western base of the Blue Ridge, at "Tye River Gap," we have—I. Sandstones and slates (Silurian), with high northwest dip; width three-quarters of a mile, extending up the mountain. II. A broad range of granitic rocks; soft, and decomposing, without stratification. These continue three miles to the summit and one mile down the east side. They contain frequent beds of epidotic granite, and a beautiful dark-gray syenite. The latter two are hard and permanent. In this space occur two bands of slaty rocks, one on the west side of the summit, and the other on the east side. That on the west side is one mile below the top, is 200 yards wide and contains gray and reddish slates, with associated greenstone-slate, sometimes amygdaloidal. Dip, moderate, southeast. The other band is half a mile below the summit on the east side. It presents repeated alternations of red and gray slates, white sandstones, and conglomerates, with a high dip, to the southeast, and a width of half a mile. Entire distance four miles. Down the east slope we find gray granitic rocks, with some bands of argillaceous slate, and toward the east have two or three dykes of *greenstone* (pinite porphyry ?). Extent four miles. III. From the last point, on the junction of the north and south forks of Tye River, the country up to two miles beyond Lovingston is mainly gneissic, with frequent beds of true granite near Lovingston. Dip, high, southeast. Extent fourteen miles. IV. Greenish chloritic granite; gneiss; and bluish mica slate. Dip, vertical. Interval occupied, three miles. This last is the position of Buffalo Ridge. V. Findlay's Mountain, one mile wide, composed of coarse quartzite. Dip vertical. After this the dip changes to northwest, and then to southeast, which it retains for a long distance as we pass east.

It will be noticed that in this section the quartzites composing the Ragged Mountains in the central portions of the valley are not found. In No. III the granite mentioned forms the most western ledges of the eruptive rocks to be described near Lynchburg. The chloritic granite is the representative, most probably, of the epidosites of the northeast. If so, this rock, which is *protogine*, is a metamorphic product from argillaceous slates. In the Catoctin Mountains, to the northeast of Lynchburg, various useful minerals occur; among which, copper ores, galena and magnetic iron may be mentioned. These may, however, be of Triassic age, as great masses of Triassic trap occur in them near these deposits.

Coming now to the vicinity of Lynchburg, and commencing with the examination of Buffalo Ridge, three miles southeast of the town, we find massive mica slates and schists composing the ridge and having the form of a closed anticlinal. No metamorphic core is present. Similar rocks occupy the whole distance up to the town. They are disposed in several rather open anticlinals. Near Mt. Athos, four miles down the river from the ridge, we find magnesian limestone associated with some quartzites, mica and talcose slates, containing deposits of magnetic and specular iron. Here occurs the curious rock called "catawbarite," by Lieber. This is an intimate mixture of talcose matter with specular and magnetic iron, containing sometimes enough of the latter to be worked as an ore.

Just opposite the town of Lynchburg occurs the most eastern of the ledges of the syenitic rock, which forms an important feature of the east and central portions of the valley. Since the section given above was made by Rogers, the railroad from Washington to Lynchburg has been made. This runs for sixty miles among these ledges, and exposes by numerous cuts their nature. One of the most striking features presented is the great amount of decay exhibited. Often cuts twenty feet deep fail to give perfectly sound material. It weathers to a rusty brown mass, presenting rounded outlines. Some ledges, however, are almost perfectly fresh. The bedding, and relation to the enclosing mica slates, are well shown both here and at Lynchburg, in the natural section presented along James River.

Returning to the latter place, we find, as we proceed up stream, numerous ledges of this rock succeeding each other at short intervals, being overlaid unconformably by mica slate, which passes occasionally into mica schist. These ledges occupy a belt extending to the west some five or six miles, but diminish in frequency westward until mica slates again occupy the entire surface. These latter continue to the distance of some ten miles from the town, when the coarse granites and syenites of "Tobacco Row," and "No Business" Mountains make their appearance.

The ledges in question often present the appearance of low dykes with rounded summits, over which the slates arch unbrokenly. Then they commonly lie in heavy layers, resembling stratified beds to a certain extent. This, however, I consider to be produced by that concentric structure so often seen in cooling masses, and which, when formed on a large scale and viewed in small segments, is easily mistaken for stratification. The rock sometimes breaks through the slates, and one ledge reverses the dip developed by its neighbor, crumpling back the slates, contorting and crushing them. The width varies greatly. I saw one ledge 600 feet wide near the town, but many are no more than 100 feet wide.

The principal component is hornblende, in pretty large crystalline particles, imbedded in a rather scanty cement of fine granular, white feldspar. This is too fine to show crystalline faces, and hence the species could not be determined. Taking the features all together, it most resembles a syenite, formed by imperfect fusion through metamorphic action on some underlying rock rich in basic compounds. Its texture is rather loose, none of the components are perfectly crystallized, or fused together, and hence perhaps the ease with which it decomposes.

The following accessory minerals occur occasionally: garnet, rutile, rubellan, pyrite.

For much of the distance above Lynchburg mica slate predominates. It shows mostly a dip south-southeast, but several reversals occur, and continue for a considerable distance. The fact is to be noted, that in this region there is no constancy of dip indicating a great succession of closed folds. The mountains some ten miles above Lynchburg, containing the granites and syenites of the central parts of the valley, I had no opportunity to examine. From hand specimens obtained, the granite is an exceedingly coarse-grained, rough-looking rock, composed principally of quartz, with a smaller amount of black mica and white orthoclase. The mica has a tendency to aggregate into lumps, so as to give the rock a rather singular aspect, the white ground of the quartz and feldspar being marked with black spots, sometimes of large size. These rocks are to be regarded as forming one system with the syenites at Lynchburg, as is shown in the cuttings along the railroad, which runs through them in the valley.

My detailed examination was resumed on James River, about three miles from where it issues from the east base of the Blue Ridge. The ridge here is a composite one, being formed of several parallel chains; those on the east being composed of crystalline rocks, and those on the west of Silurian strata. The foot hills on the east often rival in height the main chain.

The first rock seen here is a mica schist, with a dip 30° northwest. This on the west is succeeded by a mass of gneiss 900 feet wide, apparently intruded among the slates. Its dip is so confused by vertical joints that I could not make it out. The composition is principally of uncrystallized feldspar, dark mica in fine lines, and quartz distributed in numberless seams of almost microscopic fineness, showing that the rock is highly altered. This is succeeded by a narrow band of mica slate, dipping southeast, toward the gneiss. The slate is succeeded by a wide belt of coarse syenitic rock, which forms some of the highest foot hills of the ridge here. This rock extends for a half mile, and is overlaid unconformably by mica slates and schists, through which it has evidently been protruded. It is a massive coarse-grained rock, composed principally of red feldspar (orthoclase), quartz, and a smaller amount of hornblende. None of the constituents are well crystallized, or distinctly segregated. The feldspar forms large particles and the hornblende shows a tendency to arrange itself in irregular patches and lines. Some greenish feldspar occurs. The junction with the slates on the west side well shows the relation of the two systems. The slates lean upon the syenite with a dip of 45° to the northwest, showing a sharply distinct line of contact. This mass is plainly an outlier of a larger body of similar rock, which is to be found in the main mountain just west of it. The slates and schists continue 200 yards, when they are thrown into a gentle arch by the protrusion of a ledge of pinite porphyry, 300 feet wide. This in all respects resembles the rock at Harper's Ferry. It occupies here the same position relative to the Silurian strata, being about three miles distant from them. After an interval of 150 yards of slate, we have a second outcrop of pinite porphyry, in a ledge about eighty feet wide. This is followed by several hundred yards of mica slates, showing a moderate southeast dip. For some distance the rock is concealed, and then we have about 400 yards of argillites, the first appearance of this rock on this section. These slates do not have the heavy bedding and chloritic composition of those described farther north, but are thinly laminated and cleave well. Their western edge is thrown into a crushed anticlinal, changing from a previous dip to the southeast. This is caused by the appearance of an extensive ledge of pinite porphyry about 600 feet wide. Succeeding this we have a belt of mica slate, about half a mile wide, extending to the crystalline massive rock of the main ridge. These latter slates dip high to the southeast, being almost vertical and regulated in dip and strike by the course of the massive rock of the Blue Ridge. These dips given as southeast are more properly south-southeast and even south in some places, according to the curves of the crystalline mass which determines the position of the main chain.

A few hundred feet from the main mountain we see a ledge of highly-altered quartzite, about forty feet wide, reaching over toward the ridge. This rock is significant, for it resembles a highly indurated form of the Silurian quartzites, which are to be seen on the opposite side, arching over with a reversed dip to meet it. This ledge, however, is much thinner than the Silurian strata, and is impregnated with chlorite and epidote in places, having its texture perfectly compact. The slates between it and the syenite are much altered, being cut by joints, and having lost their slaty cleavage. They are also impregnated with siliceous and ferruginous matters, causing them to assume a red color on weathering. Their edges, viewed across the chasm cut by the stream, show this change very strikingly. They are seen to arch over the east slope of the crystalline rock in the form of a shell.

Passing through the gorge cut by the river in the most eastern of the principal elevations, we find that the river skirts its west base for a short distance. Turning here so as to front this mountain from the west, we see that its entire face, for more than 1,000 feet, is composed of a rugged, precipitous mass of unstratified crystalline rocks, while a band of reddish rocks, eighty or one hundred feet thick, show their edges skirting the summit. This band is the west edge of the slates which were seen resting on the mountain along the east side.

The Blue Ridge in this part of the State is, as stated before, composed of several parallel ranges of nearly equal height. The one just mentioned is composed of a species of coarse syenite, while the other ranges more to the west are made up of the durable quartzites which here form the Lower Silurian strata.

The entire zone of crystalline, massive rocks here seen is about one mile wide. The most eastern portion, composing the main ridge just described, is a rock in all respects like the coarse syenite mentioned as composing the foot hills farther east. It is truly a protean rock. No two hand specimens will show exactly similar physical features, although the essential constituents are pretty constant. Red orthoclase, in coarse, badly crystallized particles, forms the chief component, but white and green feldspar of the same species also occur. Quartz is next in abundance. The hornblende, which is scanty in amount, never forms distinct crystals. Rutile not well segregated, and films of hematitic matter, are also seen. For some distance next to the slates, on the east, the rock has the fine, well-crystallized texture of a diorite. We have here without doubt an eruption of true igneous rock along the line of junction of the two systems, an occurrence not uncommon.

On the west side we see at the base of the main mass a ledge about 100 feet wide, of a rock of similar composition with the coarse syenite, but of much finer texture; it being in some parts almost compact, and impregnated in spots with chlorite and epidote, so as to give it a bright green color. This again seems to be a modification produced by its position on the border of the principal mass. West of it, the rock changes again, and for about 1,200 feet we have a variety of syenite of fine and more uniform texture, in which the minerals are more individualized, and better crystallized. This extends up to the Lower Silurian strata, which rest against it with a steep northwest dip, the lower beds being so much altered as entirely to lose their characteristic features. This portion of the syenite betrays its more durable character by presenting extensive ledges in the bed of the river, while the coarse rock to the east has been worn down almost entirely. I consider this to be the equivalent of the peculiar syenite to be described as occurring at the Peaks of Otter.

My next examination was made twenty-five miles to the southwest, along a line ten miles in length, extending westward, from Liberty in Bedford County to the Peaks of Otter.

The Catoctin Mountains, as cut by the extension of this line, do not form a continuous range. In its stead, we have isolated elevations, composed of ribs of quartzite standing nearly vertical. To the west of Liberty, for four or five miles, we have mica schists and hornblende schists, much decayed and eroded. Succeeding these come hornblende schists, presenting the same character, and all with moderate southeast dip. On reaching "Big Otter Creek," about two and a half miles from the Blue Ridge, we see a ledge of protogine, showing a width of 600 feet. This has thrown off the hornblende schist nearly vertically to the southeast. This rock, which from its position is the equivalent of the pinite porphyry, is composed of quartz, dull white feldspar, and talc, arranged with granitoid texture. The quartz often appears in shot-like lumps, and sometimes shows quite large irregular masses. It presents in this condition a peculiar sky-blue color and waxy luster. There can be no doubt, I think, that it is the same mass which appears farther north as pinite porphyry, but here like most of the rocks of this region, from more complete metamorphism, having assumed more of the character of a true igneous rock.

Next to this, on the west, we have a belt a quarter of a mile wide of heavy-bedded mica schist, which passes into a bedded syenite or hornblende rock. The exposures here are bad, and the passage from one rock to another cannot be traced, for the surface of all these rocks is much decayed and concealed by clay. Dip of both southeast. This syenite is composed of well segregated hornblende, in large crystals, combined with large crystals of a yellowish-white feldspar, too much decayed to show its true character, but apparently triclinic. This rock is succeeded by a wide belt of massive syenite, which is more igneous-looking in its durability, want of bedding, and apparent protrusion. The latter contains white orthoclase, much hornblende and a little quartz. This extensive belt reaches to within half a mile of the main mountain. It is here succeeded by a very coarse syenite, which exactly resembles that described at Balcony Falls, in the preceding section, with the exception that the red feldspar, which principally composes it, shows distinct cleavage. This extends to within a few hundred yards of the base of the mountain. The interval is occupied by a valley, in which a stream flows, whose alluvial deposits, combined with the vast quantity of transported boulders, conceal the rock. It is apparently the same with the preceding, for the principal mass of the mountain which rises from the valley is composed of the same coarse syenite, showing, however a considerable proportion of white triclinic feldspar. The topography of the locality presents some noteworthy features.

The Blue Ridge is here again a single chain, composed entirely of crystalline rock. There are no considerable hills lying to the east of the main ridge. This latter rises immediately from the valley mentioned, with a rounded, gently swelling slope, bare of surface-earth, but covered profusely with immense boulders derived from the mass composing the Peaks proper.

After attaining the summit of the slope, we find ourselves on a level with a valley lying to the west. This lies along the foot of a chain of mountains situated farther west, which rivals the Blue Ridge in height, and is composed of Silurian strata. The summit of the slope is about 1500 feet above the valley on the east.

On each side of this point rises abruptly a mountain 1600 feet above the pass. That on the right, composed of the same rock with the one on the left, is not an isolated peak, but it is the end of a great mass of mountains which extend northeast as far as the eye can reach, and even surpass in height this *peak*. The elevation on the left, which, with the one just mentioned, forms the Peaks of Otter, is an isolated eminence. It stands up abruptly from the coarse syenitic mass forming its base, like a huge chimney, showing by its bare, abrupt faces, that it is composed of different material from the softly rounded, well-worn base on which it stands. In ascending it we find the coarse syenite accompanying us to within 600 feet of the top of the crag, which forms the highest point. This would seem to indicate that in the pass at least 1000 feet of the older coarse rock has been scooped out, for the two peaks facing each other present similar features. To the southwest, the great elevation seen at the Peaks is soon lost. There the crystalline mass rapidly sinks down into several low finger-like spurs, and farther on no elevation which can be called a mountain occurs. The Virginia and Tennessee railroad passes the Blue Ridge at Buford's Gap without tunnelling, and by moderate grades. In this region, as Rogers has stated, the Silurian range to the west is called the Blue Ridge.

The following then is the structure of the mountain at the Peaks. The main mountain rises in the form of a broad, softly-rounded ridge, composed of coarse syenites, like those described at the east base, and like the rocks on James River west of Lynchburg. From this, as a base, rises abruptly on the west side near the east limit of the Silurian strata, a broad ledge of a totally different rock, which will be presently described. To this latter mass, which is about 900 feet wide, the mountains owe their additional height of 1600 feet.

Many peculiar features of erosion and the transport of matter occur, to adequately describe which too much space would be required. These can best be explained by assuming the action of ice through this gap. In passing over the coarse decomposing syenites, which form the lower part of the mountain, we see several ledges of a much more crystalline rock penetrating them. This, which has all the characters of a true igneous substance, has made its exit principally on the west side, in the broad ledge which forms the two Peaks. It is a dark, greenish rock, very tough, and exceedingly durable. This furnishes the boulders which cover the east slopes. As the more easily decomposing, supporting framework of coarse syenite crumbled away, this more durable substance fell, and made its way down the gradual slope, perhaps aided by ice.

It is composed principally of a peculiar orthoclase of a sea-green color. This by its predominance gives a green color to the entire rock. The other constituent is a granular mixture of hornblende with a notable amount of magnetic iron. All the constituents are well crystallized, and show brilliant cleavage faces. Sometimes the feldspar occurs in quite large masses. Its crystals are always much larger than those of the hornblende and magnetic iron. Some pyrite is present. From some trials, I am inclined to think that the green color of the feldspar is due to finely disseminated hornblende. It is plainly a true igneous rock, of later date than the coarse syenites This then would be a true eruptive syenite, whose emission may have aided in producing the great disturbances seen in the Lower Silurian here and elsewhere in the State.

This belt of rocks passes out of Virginia into North Carolina, and is there continued in the broad area marked by Kerr as Upper Laurentian, in his map of that State. I will defer any remarks on the age of the different rocks here presented until after the presentation of some facts gathered from the study of the Silurian strata which join them, for these have an important bearing on this question.

In this article I have devoted most space to the geological structure of the two mountain chains and to the massive crystalline rocks, since my object in making the examination was to discover whether any igneous rocks existed in them, and, if so, to determine the part they played in their structure.

Morgantown, West Virginia, Oct. 16th.

Art. XII.—*Notice of the Chemical and Geological Essays of T. S. Hunt;** by James D. Dana.

Mr. Hunt has brought together, in this volume of Essays, various memoirs which have been published by him in this Journal and elsewhere. The chemical papers are important contributions to science, and show that the author was among the first to appreciate the principles which lie at the basis of what is called modern chemistry. He further applied the principles to the department of mineralogy; and in the view which he presented in 1859 with regard to the molecular relations of the feldspars, he appears to have anticipated Tschermak by ten years. The chapters on chemical geology contain much that is valuable, though not all original, on the origin of igneous and metamorphic rocks, of dolomite and gypsum, of volcanoes, of mountains, and on other topics, yet coupled with opinions, of fundamental importance, especially with reference to the making of mountains, metamorphism and the origin of some kinds of rocks, which science, we think, will never sustain. There is an important chapter on "Bitumen and Pyroschists,"† pointing out the relations in chemical constitution between mineral oil and certain vegetable and animal tissues, and throwing light on the origin of the oil. The memoir on the Chemistry of Natural waters is well worthy of study. Another, on the Cambrian and Silurian in North America, is an important historical document—although failing to show that any good would come from separating the Primordial group from the Silurian and calling it Cambrian, when, so far as the fauna and stratification go, there is the closest relation between it and the overlying beds.

The reader of the volume will observe that in the Third Chapter the White Mountain series and Green Mountain series of rocks are made (as had been done by other geologists) Lower Silurian, and Upper Silurian and Devonian, in age, while in the Thirteenth Chapter (as also mentioned in the preface to Chapter III), both are pronounced pre-Silurian. In this, the older view, as I believe I have proved, is the one sustained by the facts. The new view is wholly speculative, being based on no careful stratigraphical study of the regions, but mainly upon the assumption that certain kinds of crystalline rocks are a test of geological age the world over. Since the first announcement of this doctrine by Mr. Hunt, I have spent many months in the study of the Green Mountain rocks and those of some other parts of New England, in order to ascertain whether there is any virtue in the criterion; and I have found none. Mr. Hunt makes staurolite evidence of pre-Silurian age; while, as I have shown, its crystals occur in crystalline rocks of New England that are not older than Upper Silurian. Such erroneous conclusions make it apparent that in reading the work the judgment should be held in reserve until the other side is heard.

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* Chemical and Geological Essays, By Thomas Sterry Hunt, LL.D. 490 pp. 8vo. Boston. 1875. (James R. Osgood & Co.)

† Mr. Hunt's convenient term "pyroschists," applied to shales containing carbonaceous material, is objectionable in that the rocks are shales and not schists; and on this account I have not adopted it in my Geology.

There is also another more serious reason for this reserve. For the volume contains a series of misrepresentations of the views of others wholly unnecessary to the presentation of the author's opinions, and difficult to find excuse for. These misstatements have already been the occasion of various corrections by the writer in this Journal. But, as they are now reproduced, and with additions, a few further words have become necessary.

The controversy between us, which Mr. Hunt here details so far as his side is concerned, relates chiefly to the misrepresentations of my views, as well as those of others, which are contained in his Address before the American Association in 1871, making Chapter XIII of his volume of Essays.

After many years of labor for the progress of truth, it was not a little disappointing to find myself made accountable for views I did not hold, and have them denounced as "sophistries" and "contrary jargon," even though placed under this condemnation in the good company of Gustaf Rose, Haidinger and "many others," all equally innocent of them.* Soon after receiving a copy of the Address, in the autumn of 1871, I informed Mr. Hunt of the incorrectness of his statements in long friendly letters. Obtaining no satisfaction in this. I published my review of his Address in this Journal early in 1872,† and in it denied that I entertained the views he had attributed to me, referring him to my Geology for evidence. Mr. Hunt, notwithstanding my disavowal, repeated the charges in a reply, rejecting the Geology as evidence;‡ and as a specimen of his reasoning, and his way of treating facts, I cite the following from my rejoinder:§

"Mr. Hunt's most extraordinary feat is his making out that the writer has virtually sustained the view of the *metamorphosis of granite or gneiss to limestone*, when, as I said before, it is an idea that never entered my head until the reading of Mr. Hunt's caricature of the subject. The proof which he gives is remarkable. In the first place, he says that my Mineralogy contains the fact that calcite is sometimes found pseudomorphous after quartz; and, in another place, the fact that calcite is found pseudomorphous after feldspar. Hence the conclusion, *granite or gneiss to limestone*. Q. E. D.

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* Essays, p. 287. † This Journ., III, iii, 86, 1872. ‡ Ib., iv, 41, 1872. § Ib., iv, 97.

Now, if the facts respecting the pseudomorphs *were facts*, it would still require great constructive powers to make out from the statements the conclusion that I ever held to the 'metamorphosis of granite or gneiss into limestone.' But, as to the facts: (1) The Mineralogy *does not* mention any case of the pseudomorphism of calcite after quartz; and (2) the pseudomorphs of calcite after feldspar are spoken of as examples, not of an *alteration* of the feldspar, but of its removal and the substitution of calcite (4th edit., p. 249, and also 5th edit., p. 361). Now, by this substitution process, the above mentioned metamorphosis would consist (*supposing* fact No. 1 to be a fact, and that mica crystals may be similarly changed to calcite, which Mr. Hunt omits to include) in a removal of all the materials of the granite by a process of solution, and the cotemporaneous or subsequent substitution of calcite!

All will admit that the use of facts and not-facts exhibited in the above charge is most extraordinary; and can judge from it, and from other like cases stated, of Mr. Hunt's ability to appreciate, or do justice to, the views of others."

During the past year he has repeated anew his assertions, in a note before the Boston Natural History Society;* and to this I gave a brief denial in the last volume of this Journal on pages 221, 222.

Among the various other persons persistently misrepresented by him, no one has been more grossly so than the late Dr. Naumann. Mr. Hunt claimed in his Address that Naumann agreed with him in his doctrine that pseudomorphism in the case of certain silicates was simply "envelopment." I showed, by a direct citation of facts from Naumann's work on Mineralogy, that he held, on the contrary, the views of the men Mr. Hunt had denounced, and I proved that the letter from Naumann to Delesse, which Mr. Hunt cited to sustain his opinion, did not refer at all to the point in question. (See page 91 of vol. iii, 1872, of this Journal.) Naumann sustained my statement in an indignant note to the *Jahrbuch für Mineralogie*, of which the following is a translation:

Dresden, November 17, 1872.

Professor Sterry Hunt, in his Address before the American Association of the 16th of August, 1871, has quoted extracts of a letter of mine to Delesse, which were printed in the *Bulletin de la Société Géologique*, vol. xviii, p. 678 of the Second Series, and which seem to show that my views agree with his theory of the formation of serpentine and the allied rocks.

It is certainly true that in that letter I have expressed myself satisfied with this: that my friend Delesse, in his Treatise (*Recherches sur les Pseudomorphoses*, *Ann. des Mines*, V, xvi, 317, et seq.), has separated many examples of what are really cases merely of regular envelopment, (*enveloppemens avec orientation*) from the *pseudomorphs* with which, although having nothing in common with them, they had been associated. In connection, I took occasion to remark that it seemed to me an *analogous* error, when *all* gneisses, amphibolites, and so on, are regarded as metamorphic and not originally-formed rocks; and that the confounding of the two ideas of pseudomorphism and metamorphism has had many unfortunate results; and further that I recognize as pseudomorphs only those cases in which the form of the altered crystal is still retained. The mixture or mutual envelopment of two minerals is generally due indeed to simultaneous crystallization; and yet there are cases of envelopment which may be regarded as pseudomorphs, provided the form of the enveloped crystal is still recognizable.

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* Proceedings for 1874, p. 334.

Although I have several times opposed the exaggerated use of the doctrine of metamorphism, yet it is still incomprehensible to me how Professor Sterry Hunt can consider the portions of my letter to Delesse, quoted by him, as proofs that I regard those cases of pseudomorphism, upon which the theory of metamorphism is based, as for the most part only examples of association and envelopment as well as results of a simultaneous and original crystallization, or that this view of mine is identical with that proposed by himself in the year 1853.*

Nothing but an incomprehensible misunderstanding can explain such an opinion, which, moreover, has been already sufficiently disposed of by Dana in the American Journal of Science for February and August of 1872. CARL NAUMANN.

In Mr. Hunt's new book that letter to Delesse is again appealed to, to show that Naumann holds what he rejects, and what every one who is acquainted with his Mineralogy knows that he has never held; for the work does not contain a word on "envelopment" in the chapter on Pseudomorphism, and both in that chapter and elsewhere he presents the ordinarily accepted view. Naumann's note to the *Jahrbuch* was not called out by anything I had written him; I never addressed a letter on the subject to anyone in Europe.

Mr. Hunt endeavors to make out that Naumann knew the contents of his Address only through my "misleading criticisms," and that therefore his letter is not to be taken as meaning what it says. But Naumann, after giving his view of his own letter, is equally precise in his statement of Mr. Hunt's doctrine. Moreover, Naumann shows that he *did* have Mr. Hunt's writings before him; for he refers to Mr. Hunt's views in "1853," as quoted

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*The sentence of Mr. Hunt's, published in 1853, which is here referred to, is as follows: "The generally admitted notions of pseudomorphism seem to have originated in a too exclusive plutonism, and require such varied hypotheses to explain the different cases that we are led to seek for some more simple explanation, and to find it, in many instances, in the association and crystallizing together of homologous and isomorphous species."—*Am. J. Sci.*, II, xvi, 218.—EDS.

in his Address, (see foot-note to the preceding page,) to which date or citation I have at no time until now made any allusion; and the fact that Mr. Hunt, as he admits, omitted part of the letter, is sufficient to explain Naumann's reference to *extracts*, if it be not also true that only *extracts* were published in the *Annales des Mines.* Naumann was too thorough, accurate, and conscientious a man in all his scientific work to have written that criticism of Mr. Hunt without having read carefully Mr. Hunt's writings on the subject. "There is a confusion, not to say contradiction, in these expressed views of the venerable teacher not easy to explain,"* if we take Mr. Hunt as our expositor. But all is clear enough in the words and the works of Naumann.

Mr. Hunt, in his new volume, instead of offering an apology for his misrepresentations, gives the following excuse for his course:

> "Nothing has been further from my intention than to misrepresent the views either of Naumann or of Dana; and my error, if I have fallen into one, arises from the difficulty of knowing their real opinions upon the matters in discussion. Let Professor Dana define, as clearly as I have done, his present views as to the origin of magnesian rocks, both those made up of chrysolite and pyroxenic minerals, and those composed of serpentine, steatite and chlorite, which he has supposed to come from an epigenesis of the former; let him tell us whether he holds the doctrine of pseudomorphic metamorphism which he taught in 1845, 1854 and 1858, and which, as I have shown, was held by Delesse as late as 1857, or that doctrine so long maintained by me, which the latter adopted in 1861. Such a definition would be eminently satisfactory to those who look to him as a teacher in science, and would prevent any further misconception or unintentional misrepresentation of his views."

Such questions, after all that I have written on the subjects mentioned, are strangely out of place, and wrong in their implications; for I have expressly stated my present relation to the views I held in 1845, 1854 and 1858, *in my Notices of his Address;* and the difficulty complained of in the opening part of the paragraph is, as shown beyond, wholly a subjective one.

When I referred Mr. Hunt for my present opinions to my Manual of Geology (published in 1862), where they are drawn out at length, and denied that I held, or the book contained, the doctrine that "metamorphism was pseudomorphism on a broad scale," and did the same again in my rejoinder, I supposed that I had been as explicit as possible; and yet I am asked to make another assertion on the subject. In both of my notices of Mr. Hunt's Address (see the citations beyond,) I have stated that I had regarded serpentine and some other hydrous magnesian rocks as examples of pseudomorphism on a broad scale, but I have not since 1858 made the principle a general one, or applied it to any other rocks. That statement occurs only in a book-notice in 1858.*

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* Hunt's Essays, p. 323.

With regard to magnesian rocks, I have stated my views in my first notice of his Address. Again, in my rejoinder to his reply, in August, 1872 (this Journ., III, iv, 103) I observe as follows, after a mention of various facts:

"In view of such facts, the writer still holds, as in 1845, that—

* * * "The same causes that have originated the steatitic scapolites, occasionally picked out of the rocks, have given magnesia to whole rock-formations, and altered throughout their physical and chemical characters. If it be true that the crystals of serpentine are pseudomorphous crystals, altered from chrysolite, it is also true, as Breithaupt has suggested, that the beds of serpentine containing them are likewise altered; though often covering square leagues in extent, and common in most primary formations. The beds of steatite, the still more extensive talcose formations, contain everywhere evidence of the same agents."—*This Journ.*, xlviii, 92, 1845.

"Besides this paragraph, expressive of my views, Mr. Hunt cites also another of the same purport from my Mineralogy of 1854, and in this, also, I see little to modify. It is as follows: that—

"The various examples of pseudomorphism should be understood as cases not simply of alteration of crystals, but in many instances of changes in beds of rock. [Delesse admits this; see p. 99.] Thus all *serpentine*, whether in mountain-masses, or the simple crystal, has been formed through a process of pseudomorphism, or in more general language, of metamorphism; the same is true of *other magnesian* rocks, as steatitic, talcose or chloritic slates.† Thus the subject of metamorphism, as it bears on all crystalline rocks, and of pseudomorphism, are but branches of one system of phenomena."—*Min.*, *4th edit.*, i, 226, 1854.

These statements respecting hydrous magnesian rocks ought to be considered clear and satisfactory; for besides declaring my present relation to the views I had expressed in 1845, 1854 and 1858, I stated, with regard to the origin of these rocks, all that I thought to be warranted in the existing state of the science. I have nowhere attempted an explanation of the precise chemical processes in the production of magnesian rocks, while Mr. Hunt has; but I fully believe that his very clear expositions of the subject, which he presents to me as a model, will prove to be almost worthless when the "real" facts are better understood. Believing much in facts, I have been studying the subject the past year; and the observations which I have made at Brewster, New York, and which are published in the last volume of this

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* Amer. Journ. Sci., II, xxv, 445. That the expression was a hasty one on my part is evident from the entire rejection of the opinion from my Geology, which was written in 1859 and 1862, and the additional fact that when I read it in Mr. Hunt's Address I could not at first believe that I was its author, and again and again hunted for it before I found it.

† I would now remove from this list talcose slate, as it has been shown to be mostly hydromica slate, and also chloritic slate, which is an ordinary metamorphic rock.

Journal (see pages 371 and 477) enable us to move a step forward, and, at the same time, leave little room for doubt with regard to the small value of Mr. Hunt's speculations.*

I had no occasion to speak of the origin of chrysolite rocks nor do I now see that it was necessary. Such rocks are known to be very common among the oldest terranes of the globe, so that this material for making serpentine was then abundantly supplied. Chrysolite rocks and chrysolite altered to serpentine as Genth has shown, occur in North Carolina "on a broad scale." Hornblende and pyroxene rocks are ordinary metamorphic rocks. A very pure hornblende rock of great thickness occurs in the Helderberg formation of the Connecticut Valley in Bernardston, Massachusetts, and Vernon, Vermont.

Mr. Hunt keeps up his misrepresentations even when he is saying that "nothing is further from my intention than to misrepresent the views either of Naumann or of Dana." His plea (in the very next sentence to this one about his "intention,") as to the "difficulty of knowing their real opinions upon the matters in discussion" is a subterfuge that can do him no service. Mr. Hunt's persistent neglect to look into the works of Naumann for his opinion on the point in dispute, or to refer to them in any way, is a natural source for the difficulty he complains of. His singular refusal to take my Manual of Geology as evidence with regard to my views since 1862 is also a positive source of difficulty. But difficulty from such sources is no excuse. It may be said that the words "difficulty of knowing their real opinions upon the matters in discussion," referred especially to *one* question—the chemical process in the formation of magnesian rocks. But, as regards Naumann, this question has not been in discussion; and, besides, the expression admits of no such restriction.

In order that the absurdity of the claim of "difficulty" may be still better appreciated, and to prevent, if possible, its repetition, I here state that—

I have never held, and my writings no where sustain, the following opinions which Mr. Hunt has attributed to me and others:—

1. The "possibility of converting almost any silicate into any other."
2. The possibility of converting granite into limestone.
3. The possibility of converting gneiss into limestone.
4. The possibility of converting diorite into limestone.

*** Mr. Hunt, in his hypothesis, attributes the origin of beds of serpentine and steatite to the alteration of chemically-deposited beds of different hydrous magnesian silicates related to meerschaum. Serpentine occurs of various ages, and is even found in Cretaceous rocks in California, as shown by Whitney. Certainly if serpentine had been made from the alteration of beds of hydrous magnesian silicates such beds should now exist somewhere in the *unaltered* strata between the Archæan and Cretaceous; and yet they do not.**

5. The possibility of converting granite into serpentine.
6. The possibility of converting granulite into serpentine.
7. The possibility of converting gneiss into serpentine.
8. The possibility of converting diorite into serpentine.
9. The possibility of converting limestone into granite.
10. The possibility of converting limestone into gneiss.

Again, with the exception of the year 1858, I have never held nor taught that metamorphism is pseudomorphism on a broad scale. My Geology gives a very different definition of metamorphism.

I regret that I have been compelled to return to this unpleasant subject. Charged with holding views which I did not entertain, it became a duty to the cause of scientific truth to put in a disclaimer. And now that Mr. Hunt's misstatements have been given new currency by a republication of them, with additions, in his volume of Essays, and since his repeated assertions have led to my being quoted for the views attributed to me, a new explanation and denial seemed to be demanded.

The case is a strange one in the annals of science. Four years have not sufficed to secure a recognition of the facts. Any other person, with hardly an exception, if he had had my denial, and had been referred to publications of mine that gave my views and fully sustained the denial, would have accepted the statement, and made a public correction. The misrepresentations are a blot on the volume of Essays, and one which might have easily been avoided.

ART. XIII.—*Do Varieties wear out, or tend to wear out?* By Professor ASA GRAY.

The following article was published in the N. Y. Tribune, (the Semi-weekly, of Dec. 8,) in response to some questions referred to the author. Although cast in a popular form, for general readers, we deem it well worthy of reproduction in this Journal.—EDS.

THIS question has been argued from time to time for more than half a century, and is far from being settled yet. Indeed, it is not to be settled either way so easily as is sometimes thought. The result of a prolonged and rather lively discussion of the topic about forty years ago in England, in which Lindley bore a leading part on the negative side, was, if we rightly remember, that the nays had the best of the argument. The deniers could fairly well explain away the facts adduced by the other side, and evade the force of the reasons then assigned to prove that varieties were bound to die out in the course o time. But if the case were fully re-argued now, it is by n means certain that the nays would win it. The most they could expect would be the Scotch verdict, "not proven." And this not because much, if any, additional evidence of the actual wearing out of any variety has turned up since, but because a presumption has been raised under which the evidence would take a bias the other way. There is now in the minds of scientific men some reason to expect that certain varieties would die out in the long run, and this might have an important influence upon the interpretation of the facts that would be brought forward. Curiously enough, however, the recent discussions to which our attention has been called seem, on both sides, to have overlooked this matter.

But, first of all, the question needs to be more specifically stated if any good is to come from a discussion of it. There are varieties and varieties. They may, some of them, disappear or deteriorate, but yet not wear out— not come to an end from any inherent cause. One might even say, the younger they are the less the chance of survival unless well cared for. They may be smothered out by the adverse force of superior numbers; they are even more likely to be bred out of existence by unprevented cross-fertilization, or to disappear from mere change of fashion. The question, however, is not so much about reversion to an ancestral state, or the falling off of a high-bred stock into an inferior condition. Of such cases it is enough to say that, when a variety or strain, of animal or vegetable, is led up to unusual fecundity or of size or product of any organ, for our good, and not for the good of the plant or animal itself, it can be kept so only by high feeding and exceptional care; and that with high feeding and artificial appliances come vastly increased liability to disease, which may practically annihilate the race. But then the race, like the bursted boiler, could not be said to wear out, while if left to ordinary conditions, and allowed to degenerate back into a more natural, if less useful state, its hold on life would evidently be increased rather than diminished.

As to natural varieties or races under normal conditions, sexually propagated, it could readily be shown that they are neither more nor less likely to disappear from any inherent cause than the species from which they originated. Whether species wear out, i. e., have their rise, culmination, and decline from any inherent cause, is wholly a geological and very speculative problem, upon which, indeed, only vague conjectures can be offered. The matter actually under discussion concerns cultivated domesticated varieties only, and, as to plants, is covered by two questions.

First, *will races propagated by seed*, being so fixed that they come true to seed, and purely bred, (not crossed with any other sort,) continue so indefinitely, or *will they run out in time*—not die out, perhaps, but lose their distinguishing characters? Upon this, all we are able to say is that we know no reason why they should wear out or deteriorate from any inherent cause. The transient existence or the deterioration and disappearance of many such races are sufficiently accounted for otherwise; as in the case of extraordinarily exuberant varieties, such as mammoth fruits or roots, by increased liability to disease, already adverted to, or by the failure of the high feeding they demand. A common cause, in ordinary cases, is cross-breeding, through the agency of wind or insects, which is difficult to guard against. Or they go out of fashion and are superseded by others thought to be better, and so the old ones disappear.

Or, finally, they may revert to an ancestral form. As offspring tend to resemble grand-parents almost as much as parents, and as a line of close-bred ancestry is generally prepotent, so newly originated varieties have always a tendency to reversion. This is pretty sure to show itself in some of the progeny of the earlier generations, and the breeder has to guard against it by rigid selection. But the older the variety is—that is, the longer the series of generations in which it has come true from seed—the less the chance of reversion: for now, to be like the immediate parents, is also to be like a long line of ancestry; and so all the influences concerned—that is, both parental and ancestral heritability—act in one and the same direction. So, since the older a race is the more reason it has to continue true, the presumption of the unlimited permanence of old races is very strong.

Of course the race itself may give off new varieties: but that is no interference with the vitality of the original stock. If some of the new varieties supplant the old, that will not be because the unvaried stock is worn out or decrepit with age, but because in wild nature the newer forms are better adapted to the surroundings, or, under man's care, better adapted to his wants or fancies.

The second question, and one upon which the discussion about the wearing out of varieties generally turns, is, *Will varieties propagated from buds, i. e., by division, grafts, bulbs, tubers and the like, necessarily deteriorate and die out?* First, Do they die out as a matter of fact? Upon this, the testimony has all along been conflicting. Andrew Knight was sure that they do, and there could hardly be a more trustworthy witness.

"The fact," he says, fifty years ago, "that certain varieties of some species of fruit which have been long cultivated cannot now be made to grow in the same soils and under the same mode of management, which was a century ago so perfectly successful, is placed beyond the reach of controversy. Every experiment which seemed to afford the slightest prospect of success was tried by myself and others to propagate the old varieties of the apple and pear which formerly constituted the orchards of Herefordshire, without a single healthy or efficient tree having been obtained; and I believe all attempts to propagate these varieties have, during some years, wholly ceased to be made."

To this it was replied, in that and the next generation, that cultivated vines have been transmitted by perpetual division from the time of the Romans, and that several of the sorts, still prized and prolific, are well identified, among them the ancient Græcula, considered to be the modern Corinth or Currant grape, which has immemorially been seedless; that the old Nonpariel apple was known in the time of Queen Elizabeth; that the White Beurré pears of France have been propagated from the earliest times; and that Golden Pippins, St. Michael pears, and others said to have run out, were still to be had in good condition.

Coming down to the present year, a glance through the proceedings of pomological societies, and the debates of farmers' clubs, brings out the same difference of opinion. The testimony is nearly equally divided. Perhaps the larger number speak of the deterioration and failure of particular old sorts; but when the question turns on "wearing out," the positive evidence of vigorous trees and sound fruits is most telling. A little positive testimony outweighs a good deal of negative. This cannot readily be explained away, while the failures may be, by exhaustion of soil, incoming of disease, or alteration of climate or circumstances. On the other hand, it may be urged that, if a variety of this sort is fated to become decrepit and die out, it is not bound to die out all at once, and every where at the same time. It would be expected first to give way wherever it is weakest, from whatever cause. This consideration has an important bearing upon the final question, are old varieties of this kind on the way to die out on account of their age or any inherent limit of vitality?

Here, again, Mr. Knight took an extreme view. In his essay in the Philosophical Transactions, published in the year 1810, he propounded the theory, not merely of a natural limit to varieties from grafts and cuttings, but even that they would not survive the natural term of the life of the seedling trees from which they were originally taken. Whatever may have been his view of the natural term of the life of a tree, and of a cutting being merely a part of the individual that produced it, there is no doubt that he laid himself open to the effective replies which were made from all sides at the time, and have lost none of their force since. Weeping willows, bread-fruits, bananas, sugar-cane, tiger-lilies, Jerusalem artichokes, and the like, have been propagated for a long while in this way, without evident decadence.

Moreover, the analogy upon which his hypothesis is founded will not hold. Whether or not one adopts the present writer's conception, that individuality is not actually reached or maintained in the vegetable world, it is clear enough that a common plant or tree is not an individual in the sense that a horse or man, or any one of the higher animals, is—that it is an individual only in the sense that a branching zoophyte or mass of coral is. *Solvitur crescendo:* the tree and the branch equally demonstrate that they are not individuals, by being divided with impunity and advantage, with no loss of life, but much increase. It looks odd enough to see a writer like Mr. Sisley reproducing the old hypothesis in so bare a form as this: "I am prepared to maintain that varieties are individuals, and that as they are born they must die, like other individuals." "We know that oaks, Sequoias and other trees live several centuries, but how many we do not exactly know. But that they must die, no one in his senses will dispute." Now what people in their senses do dispute is, not that the tree will die, but that other trees, established from cuttings of it, will die with it.

But does it follow from this that non-sexually propagated varieties are endowed with the same power of unlimited duration that are possessed by varieties and species propagated sexually—i. e., by seed? Those who think so jump too soon at their conclusion. For, as to the facts, it is not enough to point out the diseases or the trouble in the soil or the atmosphere to which certain old fruits are succumbing, nor to prove that a parasitic fungus (*Peronospora infestans*) is what is the matter with potatoes. For how else would constitutional debility, if such there be, more naturally manifest itself than in such increased liability or diminished resistance to such attacks? And if you say that, anyhow, such varieties do not die of old age—meaning that each individual attacked does not die of old age, but of manifest disease—it may be asked in return, what individual man ever dies of old age in any other sense than of a similar inability to resist invasions which in earlier years would have produced no noticeable effect? Aged people die of a slight cold or a slight accident, but the inevitable weakness that attends old age is what makes these slight attacks fatal.

Finally, there is a philosophical argument which tells strongly for some limitations of the duration of non-sexually-propagated forms, one that probably Knight never thought of, but which we should not have expected recent writers to overlook. When Mr. Darwin announced the principle that cross-fertilization between the individuals of a species is the plan of nature, and is practically so universal that it fairly sustains his inference, that no hermaphrodite species continually self-fertilized would continue to exist, he made it clear to all who apprehend and receive the principle, that a series of plants propagated by buds only must have weaker hold of life than a series reproduced by seed. For the former is the closest possible kind of close breeding. Upon this ground such varieties may be expected ultimately to die out; but "the mills of the gods grind so exceeding slow" that we cannot say that any particular grist has been actually ground out under human observation.

If it be asked how the asserted principle is proved or made probable, we can here merely say that the proof is wholly inferential. But the inference is drawn from such a vast array of facts that it is well nigh irresistible. It is the legitimate explanation of those arrangements in nature to secure cross-fertilization in the species, either constantly or occasionally, which are so general, so varied and diverse, and we may add so exquisite and wonderful, that, once propounded, we see that it must be true. What else, indeed, is the meaning and use of sexual reproduction? Not simply increase in numbers; for that is otherwise effectually provided for by budding propagation in plants and many of the lower animals. There are plants, indeed, of the lower sort, in which the whole multiplication takes place in this way, and with great rapidity. These also have sexual reproduction; but in it two old individuals are always destroyed to make a single new one! Here propagation diminishes the number of individuals 50 per cent. Who can suppose that such a costly process as this, and that all the exquisite arrangements for cross-fertilization in hermaphrodite plants, do not subserve some most important purpose? How and why the union of two organisms, or generally of two very minute portions of them, should re-enforce vitality, we do not know and can hardly conjecture. But this must be the meaning of sexual reproduction.

The conclusion of the matter from the scientific point of view is, that sexually propagated varieties, or races, although liable to disappear through change, need not be expected to wear out, and there is no proof that they do; but, that non-sexually propagated varieties, though not liable to change, may theoretically be expected to wear out, but to be a very long time about it.

ART. XIV.—*Communications from the Laboratory of Williams College;* by IRA REMSEN.

I. *Formation of Paratoluic Acid from Parasulphotoluenic Acid.*

IN an article published a short time ago in this Journal* I stated that when potassic parasulphobenzoate is fused with sodic formate, according to the method of V. Meyer for the synthesis of aromatic acids, terephtalic acid is formed. The reaction was a very clean one, not a trace of the isomeric phtalic acids being produced. I also called attention to the fact that a certain degree of importance was connected with this result, in as much as it showed, more conclusively than had been shown up to that time, that the reaction of Meyer takes place in a normal manner, and is hence adapted to the determination of the constitution of aromatic bodies, notwithstanding the doubts that had been expressed by chemists as to its value for this purpose.

Since the publication of the above article, V. v. Richter† has attempted to prove that Meyer's reaction does not take place in a normal manner, by showing that, when benzoic acid alone is fused with sodic formate, both isophtalic and terephtalic acids are produced. He had evidently not read my article, as it appeared in this Journal, but only a brief preliminary notice which I had previously published‡ in Germany, in which, however, I had distinctly stated that pure terephtalic acid is formed from parasulphobenzoic acid. As this notice was a *brief* one, v. Richter takes the liberty of doubting the assertion clearly made in it. After remarking that the direct replacement of the brom- and sulpho-groups in benzoic acid, by means of the questionable reaction, can only then be looked upon as proved, after it has been shown that the isomeric substituted benzoic acids may be converted into the corresponding dicarbonic acids by the same reaction, he says: "Dieses ist aber trotz verschiedener Versuche nicht gelungen; es existirt nur eine kurze Anzeige von I. Remsen dass er aus der Parasulfobenzoesäure reine Terephtalsäure erhalten;" and he then proceeds to reason exactly as if this brief notice did not exist. Subsequently, V. Meyer§ answered Richter in an entirely satisfactory manner, upholding the correctness of my assertion, and employing it as an argument in favor of his views. I have hence not felt called upon to say a word in my own defense, and I now only take this opportunity to remark that *a priori* I attach as much weight to an assertion made in a brief notice as to one made in a long article, and I shall continue to do so hereafter, whatever deviation from this principle the custom of a few chemists may appear to sanction.

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* Vol. v, 179, 274, 354.
† Berliner Berichte, vi Jahrgang, 876, 879.
‡ Ibid, v Jahrgang, 379.
§ Berliner Berichte, vi Jahrgang, 1146.

In view of the importance of the question under discussion, I have endeavored to furnish further proofs of the value of Meyer's reaction, by employing it for the purpose of effecting new conversions of known compounds into known compounds the constitutions of which are established. Of one of these conversions, viz., that of parasulphotoluenic acid into paratoluic acid, I wish to speak here.

Pure potassic parasulphotoluenate was mixed with an equal quantity of sodic formate, and the mixture then gradually heated to fusion, the mass being constantly stirred. All the phenomena mentioned by Meyer, and afterward noticed by myself, were repeated. Vapors of an exceedingly disagreeable odor were given off, and the mass turned darker and darker until it became almost black. Toward the end of the operation, inflammable gases escaped and took fire at the top of the crucible. The whole was now allowed to cool and then treated with water. With the exception of a small black residue, the mixture dissolved entirely, forming a clear solution. This was treated with sulphuric acid until it showed an acid reaction; and the solution was then agitated with ether until the latter no longer extracted any solid substance. On now evaporating the ether, an almost colorless solid residue remained which exhibited the properties of an acid. It was comparatively easily soluble in boiling water, and separated from this solution, on cooling, in the form of microscopic needles. These were found to sublime with ease, and were thus obtained in the form of beautiful, flat needles. The pure substance fused instantly at 176°. It was converted into the barium salt, and from this reprecipitated. Its fusing point was again found to be 176°.

These properties suffice to characterize the substance as paratoluic acid; but another and more positive proof of its nature was given. The acid was treated with the ordinary oxidizing mixture of potassic dichromate, sulphuric acid and water, and, after boiling for a short time, an insoluble powder was thrown down. This was filtered off, washed and dried, and was then found to have all the properties of terephtalic acid. It was insoluble in water, hot as well as cold, and did not fuse, but sublimed in a capillary tube before an applied flame.

Now as parasulphotoluenic acid is known to yield a cresole, which in turn yields paraoxybenzoic acid, and as paraoxybenzoic and paratoluic acids are conceded to belong to the same series, it follows that, in the instance here considered, the sulpho-group is replaced directly by carboxyl. But, further, paratoluic acid was the only product of the reaction. I was unable to discover a trace of any other acid, although special precautions were taken to prevent errors of observation in this respect. The quantity of the product, too, was sufficient to warrant the conclusion that it was not formed by any secondary reaction.

The results of this experiment then serve to strengthen the conclusion already drawn with reference to Meyer's reaction, and to weaken the arguments of v. Richter. I shall next attempt to convert orthosulphotoluenic acid into orthotoluic acid by the same method, though success is hardly to be anticipated, as Meyer himself failed to convert chlorsalylic acid into the corresponding orthocarbonic acid, the substituting group being replaced by hydrogen instead of by carboxyl.

Another experiment with which I have been occupied, closely allied to that described, has not yet led to decisive results. I stated, in the article already referred to, that having failed to obtain orthosulphobenzoic acid from orthosulphotoluenic acid by oxidation with potassic dichromate and sulphuric acid, I intended to use potassic permanganate in alkaline solution for the purpose of effecting this oxidation. My main object in preparing orthosulphobenzoic acid was, through it, to reach phtalic acid by means of Meyer's reaction. It appears up to the present that orthosulphotoluenic acid is destroyed entirely by potassic permanganate in alkaline solution, though I do not feel prepared to assert this positively. W. Weith * has lately shown that orthotoluic acid may be readily converted into phtalic acid by potassic permanganate in alkaline solution. Though this interesting discovery renders the preparation of phtalic acid, by the method proposed by me, unnecessary from one stand-point, I shall, nevertheless, endeavor to accomplish this.

II. *On Nitroparasulphobenzoic Acid.*

In continuing my investigations on parasulphaminbenzoic acid, I have reached some new results which are herewith communicated.

I first attempted to introduce the group SO^2OH into parasulphaminbenzoic acid in order thus to obtain a sulpho acid which, were it susceptible to ordinary reagents, might lead to definite conclusions concerning the dioxybenzoic acids and the tricarbonic acids of benzine. Common concentrated sulphuric acid dissolves parasulphaminbenzoic acid when heated with it, but does not change it, for, on diluting with water the solution thus obtained, the original acid is precipitated in its characteristic acicular crystals. The same is true in regard to the action of sulphuric anhydride; solution is effected, but from this solution parasulphaminbenzoic acid is precipitated unchanged on the addition of water. On boiling the acid with Nordhausen sulphuric acid, however, a change is accomplished, though not that which was expected and desired. The change consists simply in a regeneration of parasulphobenzoic acid from the amine-acid. In other words, the group NH^2 is converted into OH. The parasulphobenzoic acid was recognized by diluting with water, neutralizing with baric carbonate, evaporating, adding chlorhydric acid and allowing to cool. Thus were deposited the characteristic flat needles of the acid barium salt. This possessed all the properties of the salt, and its identity was further proved by the following analysis:

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* Berliner Berichte, vii Jahrgang, 1057.

0·2475 grams salt lost 0·022 grams on being heated to 200°; and gave 0·0963 grams $BaSO^4$ = 0·05662 grams Ba.

	Calculated.		Found.
$2(C^7H^5SO^5)$	402	67·79	——
Ba	137	23·10	22·88
$3H^2O$	54	9·11	8·88
	593	100·00	

I next endeavored to introduce the group NO^2 into parasulphaminbenzoic acid. Fuming nitric acid was first employed for this purpose. When this is boiled with the amine-acid, solution takes place, and, on diluting this solution with water, nothing is precipitated. The change effected is, however, the same as that effected by Nordhausen sulphuric acid, viz: the NH^2 group is simply converted into OH, and parasulphobenzoic acid thus regenerated. The latter acid was in this instance also readily recognized. The solution was evaporated to dryness on the water bath and all nitric acid thus removed. On now dissolving in water, neutralizing, as above, with baric carbonate, adding chlorhydric acid and allowing to cool, the acid barium salt was deposited. It was identified beyond a doubt by the aid of the following analysis:

0·5325 grams salt lost 0·0483 grams on being heated to 200°; and gave 0·2095 grams, $BaSO^4$ = 0·1232 grams Ba.

	Calculated.		Found.
$2(C^7H^5SO^5)$	402	67·79	——
Ba	137	23·10	23·11
$3H^2O$	54	9·11	9·07
	593	100·00	

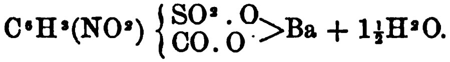
These experiments then only show that parasulphobenzoic acid is an exceedingly stable compound, as it resists the action of Nordhausen sulphuric acid and fuming nitric acid at the boiling temperatures, no matter how long the boiling may be continued. It may, further, be looked upon as an interesting result that the Nordhausen acid should cause a conversion of the NH^{2} group into OH; a fact which at least indicates a remarkable predisposition on the part of the amine-acid to conversion into the corresponding sulpho-acid.

Finally, by employing a mixture of the two agents already employed, I was enabled to obtain a substitution-product of parasulphobenzoic acid. The boiling was continued until red fumes ceased to be given off, and the solution became perfectly clear. On now diluting with water, neutralizing with baric carbonate, etc., a new salt was obtained. This was comparatively easily soluble in hot water, though less so in cold water. It crystallized in long, lustrous needles, arranged concentrically in groups which might be termed fan-shaped. They were colored yellow, and repeated recrystallizations failed to remove this color. The analyses gave the following results, showing the salt to be the neutral barium salt of nitroparasulphobenzoic acid:

I. 0·2345 grams salt lost 0·0147 grams at 200°; and then gave 0·1320 grams $BaSO^{4}$ = 0·077616 grams Ba.
II. 0·2335 grams salt lost 0·0155 grams at 200°; and then gave 0·1325 grams $BaSO^{4}$ = 0·0779 grams Ba.

	Calculated.		Found.	
$C^{7}H^{3}SNO^{7}$	245	59·90	——	——
Ba	137	33·50	33·10	33·37
$1\frac{1}{2}H^{2}O$	27	6·60	6·27	6·64
	409	100·00		

The formula of this salt is thus seen to be



$$C^{6}H^{3}(NO^{2})\left\{\begin{matrix}SO^{2}\,.\,O\\ CO\,.\,O\end{matrix}\right\rangle Ba + 1\tfrac{1}{2}H^{2}O.$$

A nitrosulphobenzoic acid has already been prepared by Limpricht and Uslar.* But, as this was obtained from metasulphobenzoic acid, it is not at all probable that the two are identical. Nothing is known concerning the relative position of the nitro-group in either of these acids, nor have we as yet any data that will enable us to draw conclusions of value with reference to this point. In this connection, the following method of investigation suggests itself. Three isomeric nitrobenzoic acids are known, and the series to which each belongs is also known. Now, if it be possible to introduce into these nitro-acids the sulpho-group, compounds would be obtained which would probably be identical with the two known forms of nitrosulphobenzoic acid. According to the conditions then, we would have data which would enable us to determine the constitution of these latter acids; for we should thus fix the position, first, of the sulpho-group with reference to the carboxyl, and second, that of the nitro-group in one and the same compound.

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* Annalen der Chemie u. Pharmacie, cvi, 27.

I shall endeavor to prove the feasibility of this method of investigation, as soon as possible. It is well to remember, however, that Mulder* has already tried to obtain a nitrosulphobenzoic acid by the action of sulphuric acid on ordinary (meta) nitrobenzoic acid, and that no such product resulted. Possibly the isomeric nitrobenzoic acids may yield more satisfactory results.

III. *On the Action of Potassium on Ethyl succinate.*

Several years ago Fehling† described a peculiar compound which he had obtained by the action of potassium on ethyl succinate. This compound, according to the discoverer, crystallizes in yellowish laminæ which are insoluble in water, difficultly soluble in cold alcohol, easily in hot alcohol. The formula deduced from the analyses was $C^6H^8O^3$, or ethyl succinate minus the constituents of one molecule of alcohol. The exact nature of this body was not determined, nor was much more than its existence and composition proved. Its peculiar nature led me to desire a more thorough knowledge of its relations to other known compounds, and I have hence undertaken its study in the hope of being able to discover its constitution by the aid of appropriate experiments. It appears possible that it may be the representative of a new class of bodies of an interesting nature, which may be prepared under similar circumstances from the various ethers of bibasic fatty acids.

Its preparation is comparatively simple, and the yield is satisfactory. I have prepared a considerable quantity, and have found the statements of Fehling concerning it essentially correct. In a pure condition, it forms long, flat, beautiful needles of a slightly yellowish color, instead of yellow laminæ. It may be boiled with alcohol or water without undergoing change. When sodium-amalgam is added to the alcoholic solution, a very rapid change takes place. A bright red precipitate is formed, which is insoluble in alcohol but easily soluble in water. From the aqueous solution of this new substance chlorhydric acid throws down a white precipitate, which is insoluble in water and difficultly soluble in alcohol. Dilute caustic potassa produces a similar change. The substance dissolves in this very easily, and the solution conducts itself in the same manner as that above mentioned. The action of the amalgam is then due alone to the formation of caustic soda, and not to the action of nascent hydrogen.

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* Annalen der Chemie u. Pharmacie, xxxiv, 297.

† Ibid, xlix, 192.

In order to obtain the red substance above referred to, the original body was dissolved in warm alcohol, and to this solution an alcoholic solution of caustic soda was added. In this manner a bright red voluminous precipitate was obtained, which consisted of very fine lustrous needles. This was filtered off, well washed out with alcohol, pressed with bibulous paper and allowed to dry. In drying, the red color changes to yellow, but the needles remain otherwise unchanged. An addition of alcohol to the yellow substance restores the red color, so that there seems to be no chemical change connected with that of color. Further, this yellow body after being dried may be preserved in the air. Analyses gave as a result 12·37 per cent sodium, but I shall not attempt to deduce a formula until this result has been confirmed by other means.

The precipitate that is produced by chlorhydric acid in the aqueous solution of the red substance proved to be the body $C^6H^8O^3$, so that it would seem that this body plays the part of an acid. Alkaline carbonates, however, do not act in the same manner as the hydroxides. Without the aid of heat, indeed, they do not act at all. When the body $C^6H^8O^3$ is boiled with sodic carbonate, succinic acid is regenerated and is precipitated on the addition of chlorhydric acid to the cooled solution. The same is true when in the case of the hydroxides the solutions are boiled. Very concentrated aqueous ammonia does not act upon the substance $C^6H^8O^3$, even when the two are boiled together.

I intend to study the action of phosphoric chloride and acetyl chloride, and, further, of nascent hydrogen in acid solution, produced by tin and alcoholic chlorhydric acid, upon the body under investigation, and expect in this manner to reach definite results which will lead to a knowledge of its constitution.

October, 1874.

ART. XV.—*On the Detection of Hydrocyanic Acid;* by M. CAREY LEA, Philadelphia.

1. *New Test.*

If a little pure protosalt of iron (I have used ferrous ammonia sulphate) be dissolved together with a little uranic nitrate, we have a solution which, with a soluble cyanide, gives a purple precipitate, or in very dilute solutions of the cyanide, a greyish purple.

This test is very delicate. Used in the manner to be presently described, a solution of potassic cyanide, containing only $\frac{1}{5000}$ of a grain of anhydrous hydrocyanic acid, gives a perfectly distinct reaction. It is therefore not exceeded in delicacy by any known test for that substance.

The solution of iron and uranium must not be acidulated, but on the contrary should be quite neutral, and so dilute as to be nearly colorless. A grain or two of each salt may be dissolved in a half ounce of water. Two or three drops of the mixed solution are to be placed in a clean white porcelain capsule, and a drop or two of the liquid to be tested should be made to slip slowly down, that the reaction at the point of contact may be carefully noted.

Cobaltous nitrate may be substituted for the uranium salt, and gives an almost equally delicate reaction, but the color of the cobalt salt is an objection.

2. *Prussian Blue Test.*

The delicacy of the Prussian blue reaction for the detection of hydrocyanic acid has been much understated. Thus Taylor (quoted in Watts' Dict., ii, 219) affirms that whilst the sulphocyanide reaction is capable of distinctly indicating the presence of $\frac{1}{3930}$ of a grain of anhydrous prussic acid, the Prussian blue test will not detect less than $\frac{1}{780}$ of a grain. A much greater delicacy than this can be obtained by using appropriate precautions.

There must not be too much iron salt present, and the solution of iron salt must not be too strong: these two points are essential. For ferric salt, the ammonia citrate is to be preferred to ammonia ferric alum or ferric chloride. The best mode of proceeding is as follows:

A weak solution of iron is to be made, containing a ferrous salt, to which a little ferric ammonia citrate is to be added. Of this solution, acidified with hydrochloric acid, two or three drops only are to be placed in a white porcelain capsule. A drop of the liquid to be tested is then allowed to slip down the side of the capsule, and this meeting the iron solution will give rise to the production of a blue cloudiness.

If a grain of potassic cyanide be dissolved in four ounces of pure water, rendered slightly alkaline with caustic alkali, and a single drop of the solution be allowed to flow down the capsule in the manner just described, a distinct blue coloration will result. This drop will have contained about $\frac{1}{2000}$ of a grain of cyanide, or about $\frac{1}{5000}$ of a grain of anhydrous prussic acid, a degree of delicacy very far surpassing that found by Mr. Taylor; surpassing, indeed, that claimed by him for the sulphocyanhydric test.

The advantage of using ferric ammonia citrate over ferric ammonia alum may be curiously shown as follows:

Place in a small white porcelain basin about an ounce of a solution containing a few grains each of ferrous ammonia sulphate and ferric ammonia sulphate acidulated with hydrochloric acid. Take about a milligramme of cyanide of potassium, or about as much dust of that substance as can be distinctly perceived and shake it into the basin. Each infinitesimal particle will produce a blue coloration as it touches the liquid, but on agitating, the blue (if the quantity of cyanide has been small enough) will disappear. If now the same experiment be repeated with a solution of ferrous ammonia sulphate and ferric ammonia citrate, also acidulated, the blue color produced does not disappear on shaking, but presently settles or becomes more conspicuous.

For the same reason, ferric ammonia citrate will detect infinitesimal traces of potassic ferrocyanide in the ferridcyanide when ferric chloride and ferric ammonia alum will not, and therefore it should have the preference for testing the purity of potassic ferridcyanide.

Art. XVI.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXX.—*The Gigantic Cephalopods of the North Atlantic;* by A. E. Verrill.

The existence of several distinct species of gigantic ten-armed cephalopods, belonging to more than one genus, has been well established by the researches of Steenstrup, Harting and others.

More recently attention has been repeatedly called to the frequent occurrence of these remarkable animals in the waters of Newfoundland. In an article on this subject, in this Journal, vol. vii, p. 158, Feb., 1874, I was able to enumerate five specimens from American waters, concerning which we had some reliable information.* Since that time much more material has been accumulated, and I am now able to cite twelve American examples. I have also had opportunities to study portions of five of these specimens. These evidently represent two distinct species, both of which belong to the genus *Architeuthis* of Steenstrup (or *Megaloteuthis* of Kent). The largest of these is represented only by the jaws of two specimens, one of which (No. 1 in my former article) was found floating at the Banks of Newfoundland, and the other (which we will designate as No. 10) was taken from the stomach of a sperm whale. The upper jaw of the latter was imperfectly figured by Dr. Packard in his article on this subject.* It is the largest jaw yet known. These belong to an apparently undescribed species, which I propose to name *Architeuthis princeps*,† and shall describe more fully farther on.

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* See articles on this subject by the writer, in the American Naturalist, vol. ix, Jan., 1875; and vol. viii, p. 167; and letters from Mr. Alexander Murray in the Naturalist, vol. viii, p. 120, Feb., 1874.

The second species, which I consider identical with the *Architeuthis monachus* of Steenstrup, is represented by parts of three individuals, and seems to be the species most commonly met with on the coasts of Newfoundland and Labrador.

The most complete specimen that has ever come under scientific observation was captured in November, 1873, at Logie Bay, near St. John's, Newfoundland. It became entangled in herring-nets and was secured by the fishermen with some difficulty, and only after quite a struggle, during which its head was badly mutilated and severed from the body, and the eyes, most of the siphon-tube, and the front edge of the mantle were destroyed. Fortunately this specimen was secured by the Rev. M. Harvey of St. John's. After it had been photographed and measured, he attempted to preserve it entire in brine, but this was found to be ineffectual, and after decomposition had begun to destroy some of the most perishable parts, he took it from the brine and, dividing it into several portions, preserved such parts as were still undecomposed in strong alcohol. These various portions are now in my possession, and with the photographs have enabled me to present a restoration, believed to be quite accurate, of the entire creature (plate II, fig. 1). In this figure the eyes, ears, siphon-tube, and front edge of the mantle have been restored from a small squid (*Loligo pallida*), to which this gigantic species seems to be nearly allied in many respects. The other parts have been drawn directly from the photographs and specimens.‡

Mr. Harvey has published popular accounts of this specimen and the previously captured arm of a still larger one, in the Maritime Monthly Magazine of St. John, N. B., for March, 1874, and in several newspapers.§ To him we are, therefore, mainly indebted for the latest and most important additions to our knowledge of these remarkable animals. The preserved parts of this specimen (No. 5) which I have been able to examine are as follows: the anterior part of the head, with the bases of the arms, the beak, lingual ribbon, etc.; the eight shorter arms, but without the suckers, which dropped off in the brine, and are now represented only by the strong marginal rings; the two long tentacular arms, which are well preserved, with all the suckers in place; the tail; portions of the "pen" or internal shell; the ink-bag; and pieces of the body.

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* American Naturalist, vol. vii, p. 91.

† This species was named and characterized in a communication made to the Connecticut Academy of Sciences, Nov. 18, 1874, and will be described in greater detail in its Transactions. See also American Naturalist, Feb., 1875.

‡ The figure was originally made, from the photographs only, by Mr. P. Rœtter, of the Museum of Comparative Zoology, but after the arrival of the specimens it had to be altered in many parts. These necessary changes were made by the writer, after a careful study of the parts preserved, in comparison with the photographs and original measurements.

§ Acknowledgments are also due to Mr. Alexander Murray, Provincial Geologist, who coöperated with Mr. Harvey in the examination and preservation of these specimens, and who has also written some of the accounts of them that have been published. See also the American Naturalist, vol. viii, p. 122, February, 1874; American Journal of Science, vol. vii, p. 160; Nature, vol. ix, p. 322, February 26, 1874; and Appleton's Journal, Jan. 31, 1874.

The general appearance and form of this species * are well shown by plate II. From the great size of the large suckers on the long arms, I judge it to be a male. The body was relatively stout, and according to the statement of Mr. Harvey, it was, when fresh, about seven feet long and five and one-half feet in circumference. The "tail" or caudal fin (plate IV, fig. 9) is said by Mr. Harvey to have been 22 inches across, but the preserved specimen is considerably smaller, owing, undoubtedly, to shrinkage in the brine and alcohol. It is remarkable for its broad sagitate form. The posterior termination is unusually acute and the lateral lobes extend forward considerably beyond their insertion. In the preserved specimen the total length, from the anterior end of the lateral lobes to the tip of the tail, is 23 inches; from the lateral insertions to the tip 19 inches; from the dorsal insertion 13·5 inches; total

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* Mr. W. Saville Kent, from the popular descriptions of this species, has seen fit to give it new generic and specific names, viz: *Megaloteuthis Harveyi*, in a communication made to the Zoological Society of London, March 3, 1874 (Proceedings Zool. Soc., p. 178; see also Nature, vol. ix, p. 375, March 12, and p. 403, March 19). My identification is based on a comparison of the jaws with the jaws of *A. monachus*, well figured and described by Steenstrup in proof-sheets of a paper which is perhaps still unpublished, though printed several years ago, and referred to even by Harting. Their agreement is very close in nearly all respects, but the beak of the lower jaw is a little more divergent in Steenstrup's figure. His specimen was a little larger than the one here described and was taken from a specimen cast ashore in 1853. Mr. Kent was probably unaware of that specimen when he said (Nature, ix, p. 403) that *A. monachus* "was instituted for the reception of two gigantic Cephalopods, cast on the shores of Jutland in the years 1639 and 1790, and of which popular record alone remains."

His statement that *Architeuthis dux* Steenstrup is known from the beak alone appears to be erroneous, for Steenstrup, Harting, and Dr. Packard, in their articles on this subject, all state that the suckers, parts of the arms, and the internal shell or pen were preserved, and they have been figured by Prof. Steenstrup; Harting has also given a figure of the lower jaw, copied from a figure by Steenstrup. Steenstrup also mentions having the arm-hooks (Tandvæbningen), which would indicate a genus distinct from our species; in the proof-sheets which I have seen, this specimen is referred to as "*A. Titan*," but Harting cites it as *A. dux* Steenstrup. Possibly two distinct species are confounded under this name.

Should the *Architeuthis dux* prove to belong to a genus distinct from this, it might perhaps be taken as the type of *Architeuthis*, and in that case the generic name given by Kent could be retained, and the two species here described would then be called *Megaloteuthis monachus* and *M. princeps*, if my identification of the former species be correct.

breadth about 15 inches; width of lateral lobes 6 inches. The eight shorter arms, when fresh, were, according to Mr. Harvey's measurements, six feet long and all of equal length, but those of the different pairs were respectively ten, nine, eight and seven inches in circumference. They are three-cornered or triquetral in form and taper very gradually to slender acute tips. Their inner faces are occupied by two alternating rows of large obliquely campanulate suckers, with contracted apertures surrounded by broad, oblique, marginal rings, armed with strong, acute teeth around their entire circumference, but largest and most oblique on the outside (plate III, fig. 10). These suckers gradually diminish in size to the tips of the arms, where they become very small, but are all similar in form and structure. The largest of these suckers are said by Mr. Harvey to have been about an inch in diameter, when fresh. The largest of their marginal rings in my possession are ·65 of an inch in diameter at the serrated edge, and ·75 beneath. The rings of the smaller suckers are more oblique and more contracted at the aperture, with the teeth more inclined inward, those on the outside margin being largest. The two long tentacular arms are remarkable for their slenderness and great length when compared with the length of the body. Mr. Harvey states that they were each 24 feet long and 2·75 inches in circumference when fresh. In the brine and alcohol they have shrunk greatly, and now measure only 13·5 feet in length, while the circumference of the slender portion varies from 2·25 to 3·25 inches. These arms were evidently highly contractile, like those of many small species, and consequently the length and diameter would vary greatly according to the state of contraction or relaxation. The length given (24 feet) probably represents the extreme length in an extended or flaccid condition, such as usually occurs in these animals soon after death. The slender portion is three-cornered or triquetral in form, with the outer angle round, the sides slightly concave, the lateral angles prominent, and the inner face a little convex and generally smooth.

The terminal portion, bearing the suckers, is 30 inches in length and expands gradually to the middle, where it is 4·5 to 5 inches in circumference (6 inches when fresh), and 1·5 to 1·6 across the inner face. The sucker-bearing portion may be divided into three parts. The first region occupies about seven inches, in which the arm is triquetral, with margined lateral angles, and gradually increases up to the maximum size, the inner face being convex and bearing about forty irregularly scattered, small, flattened, saucer-shaped suckers, attached by very short pedicels, and so placed in depressions as to rise but little above the general surface. They have narrow marginal rings, with the thin edges nearly smooth, or minutely denticulate, and ·10 to ·12 of an inch in diameter, surrounded by a thick and prominent marginal membrane. These suckers are at first distantly scattered, but become more crowded, finally covering the whole width of the inner face, which becomes here 1·6 inches broad. Scattered among the suckers are about as many low, broad, conical, smooth, callous verrucæ, or wart-like prominences, rising above the general surface, their central elevation corresponding in form and size to the apertures of the adjacent suckers. These, without doubt, are intended to furnish secure points of adhesion for the corresponding suckers of the opposite arm, so that, as in some other genera, these two arms can be fastened together at this wrist-like portion, and thus they can be used unitedly. By this means they must become far more efficient organs for capturing their prey than if used separately. Between these smooth suckers and the rows of large ones there is a cluster of about a dozen small suckers, with serrate margins, mostly less than a quarter of an inch in diameter, attached by slender pedicels.

The second division, 14 inches in length, succeeds the small suckers. Here the arm is well rounded on the back and flattened on the face, where it bears two alternating rows of very large serrate suckers, and an outer row of small ones on each side, alternating with the large ones. The inner edge is bordered by a rather broad, regularly scalloped, marginal membrane, the scallops corresponding to the large suckers. On the other edge there is a narrower and thinner membrane, which runs all the way to the tip of the arm. In one of the rows of large suckers there are eleven, and in the other ten, above half an inch in diameter, but each row has at either end one or two smaller ones, from half an inch to a quarter of an inch in diameter. The largest suckers (plate IV, fig. 11, *a*) are from 1 to 1·15 inches in diameter at the margin. These are attached by strong, though slender, pedicels, so that their margins are elevated about an inch above the surface of the arm. Each one is situated in the center of a pentagonal depressed area, about an inch across, bounded by ridges, which alternate regularly, and interlock on the two sides, so as to form a zigzag line along the middle of the arm. These large suckers are obliquely campanulate; the marginal ring is strong, and sharply serrate all around. The small marginal suckers (fig. 11, *b*) are similar in structure, but more oblique, and mostly only ·3 to ·4 of an inch in diameter; they are attached by much longer and more slender pedicels, and their marginal teeth are relatively larger and more incurved, especially on the outer margin. The terminal division of the arm is 9 inches long. It gradually becomes much compressed laterally, and tapers regularly to the tip, which is flat, blunt, and slightly incurved. Just beyond the large suckers, where this region begins, the circumference is 3·5 inches. The face is narrow and bears a large number of small serrate and pedicellate suckers, arranged in four regular alternating rows, and gradually diminishing in size to the tip of the arm, where the rows expand into a small cluster. These suckers are much like the marginal ones of the previous division, and at first are about ·25 of an inch in diameter, but decrease to about ·10 of an inch near the tip of the arm. The color, where preserved, is pale reddish, with thickly scattered small spots of brownish red.

The form of the jaws of this specimen is well shown by plate III, figs. 3 and 4. When in place, these jaws constitute a powerful beak, looking something like that of a parrot or hawk, except that the upper jaw shuts into the lower, instead of the reverse, as in birds. The color is dark brown, becoming almost black toward the tip, where its substance is thicker and firmer, and smoothly polished externally. The upper jaw (plate III, fig. 3) measures 3·85 inches in total length; 1 inch in greatest breadth; and 2·50 from front to back. The lower jaw (fig. 4) is 3 inches long, 2·75 broad, and 2·65 from front to back.

The small squids of our coast have a very similar pair of jaws. Those of *Loligo pallida* (plate IV, figs. 5, 5a) are here figured twice the natural size, for comparison and to explain the terms used in describing the large jaws.

The most remarkable anatomical character observed in this specimen is found in the form and arrangement of the teeth on the "lingual ribbon," or *odontophore*, for in this respect it differs widely from all other known Cephalopods.

The ordinary squids and cuttle-fishes all have these teeth arranged in seven regular longitudinal rows; those of the three middle rows being generally two or three-pronged, as in *Loligo pallida* (plate IV, fig. 7), while the lateral rows have long, simple, fang-like teeth. But in this species (fig. 6) the teeth are very irregularly scattered over the surface of the broad thin membrane, and it is difficult to trace the rows, if such they can be called, for the arrangement seems to be somewhat in irregular quincunx. The number of rows, however, cannot be less than twenty. These teeth are all simple, but vary considerably in size and form. They are all attached by a more or less rounded, flattened base, and all are considerably curved; some are broad and tapering; others are slender and acute; but the different forms and sizes are irregularly intermingled across the whole breadth of the membrane. Irregular granules of silica are also scattered in great numbers over the membrane among the teeth, and similar grains are embedded in the membrane lining the mouth. This peculiar type of dentition must be regarded as an extremely generalized one.

The portions of the pen in my possession belong mostly to the two ends, with fragments from the middle region, so that although neither the actual length nor the greatest breadth can be given, we can yet judge very well what its general form and character must have been. It was a broad and thin structure, of a yellowish brown color, and translucent. Its anterior portion (plate III, fig. 2) resembles that of *Loligo*, but its posterior termination is entirely different, for instead of having a regular lanceolate form, tapering to a point at the posterior end, as in *Loligo*, it expands and thins out toward the posterior end, which is very broadly rounded or irregularly truncate, fading out insensibly, both at the edges and end, into soft membrane. The anterior end, for about an inch and a half, was rapidly narrowed to a pen-like point, as in *Loligo*; from this portion backward the width gradually increases from 1·2 inches to 5 inches, at a point 25 inches from the end, where our specimen is broken off; at this place the marginal strips are wanting, but the width is 5 inches between the lateral midribs (*d*, *d''*), which were, perhaps, half an inch from the margin. Along the center of the shell, there is a strong, raised, rounded midrib, which fades out a short distance from the posterior end, but is very conspicuous in the middle and anterior sections. On each side of the midrib is a lateral rib of smaller size. These at first diverge rapidly from the central one, and then run along nearly parallel with the outer margin and about ·4 of an inch from it, but beyond 11 inches from the point the margins are torn off. Like the midrib the lateral ribs gradually fade out before reaching the posterior end; near the place where they finally disappear, they are about six inches apart.

The pen of our *Architeuthis* seems to resemble that of the ancient genus *Teudopsis*, found fossil in the Jurassic formations.

From the above description it will be seen that the most important and most characteristic features of this species, or rather of the *genus* to which it belongs, are to be found in the *lingual dentition*, in the *internal shell*, in the *form of the caudal-fins*, and in the cluster of small suckers and tubercles on the long arms. As already stated, the first three of these peculiarities indicate a low or generalized structure, and therefore a low rank in our system of classification, unless it should be found to have some other characters not yet known and of greater importance, which might outweigh those here given. It will appear, therefore, that this genus of huge squids should be classed below *Loligo*, which, in its turn, would go below *Ommastrephes*, to which genus the common small squids of our northern coasts belong, for the latter genus has distinct eyelids, which are not found in *Loligo*, and the internal shell is also more specialized.

I have received through Professor Baird, of the Smithsonian Institution, a pair of jaws and two large suckers of the long arms. which were taken from a specimen (No. 4), cast ashore in Bonavista Bay, Newfoundland. These jaws agree precisely in form and size with those described above, so that the size of these two individuals must have been about the same. The suckers (plate IV, figs. 12, 13) had been dried, and have lost their true form, but the marginal rings are perfect, and only ·92 of an inch in diameter, and though somewhat smaller than in the specimen just described, they have the same kind of denticulation around the margin. Their smaller size may indicate that the specimen was a female, but they may not have been the largest of those on the arm.

EXPLANATION OF PLATES.

Plate II.—Figure 1. *Architeuthis monachus* Steenstrup (No. 5); $\frac{1}{24}$ natural size.

Plate III.—Figure 2. Anterior part of the "pen" of the same; $\frac{1}{5}$ natural size. The dotted lines indicate parts that are wanting.

Figure 3. Upper jaw of the same; natural size.

Figure 4. Lower jaw of the same, lacking a small piece at *a*.

Plate IV.—Figures 5 and 5a Jaws of *Loligo pallida; a*, the rostrum or beak; *a b*, the cutting edge, with a notch at *b; b c*, the anterior edge of the alæ or wings; *d*, the frontal lamina in the upper jaw, or chin-portion (*mentum*) in the lower jaw; *e*, the palatine lamina in the upper jaw, or gular lamina in the lower jaw; twice the natural size.

Figure 6. Part of the lingual ribbon of *A. monachus;* enlarged.

Figure 7. Ditto of *Loligo pallida;* much more enlarged.

Figure 8. Ditto of *Loligo Hartingii*, copied from Harting; enlarged.

Figure 9. Caudal fin of *A. monachus* (No. 5); $\frac{1}{10}$ natural size.

Figure 10. Marginal ring of a sucker from one of the sessile arms; enlarged two diameters.

Figure 11. *a*, A large sucker; and *b*. a small marginal sucker from the tentacular arms of same; natural size.

Figure 12. Large sucker from tentacular arm of No. 4; natural size.

Figure 13. Part of the marginal ring of the same; enlarged.

[To be continued.]

ART. XVII.—*On the Mechanical Work done by a Muscle before Exhaustion;* by F. E. NIPHER, Assistant Professor of Physics in Washington University.

THE work done by a muscle may be classed under two heads: 1st. The exerting of any force (F) through any distance (D). To distinguish this kind of work from the other, we may call it dynamical work. It is measured by the product FD. 2d. Suppose an experimenter to hold a weight on his outstretched arm, and suppose furthermore that the experimenter is one of the weights of a large Atwood's machine, and is moved vertically with an accelerated motion. The dynamical work done by the arm before exhaustion is easily obtained from the equations of dynamics. If now the acceleration becomes zero, and the weight be simply held on the arm at a constant level, the arm very soon tires, showing that work is still done. This kind of work, which Prof. Haughton has called "statical" work, is purely molecular, and we hope to be able to show that it may be referred to and easily measured by the ordinary unit of mechanical work—the kilogram-meter.

In the supposed case,* if the acceleration becomes negative, the time of fatigue increases, until for an acceleration of minus (g), the time required to wholly fatigue the arm is infinite.

These considerations show, not only the reality of "statical" work, but also that it is an important element in all kinds of muscular work.

As the unit of statical work, we shall adopt the work done by the horizontally outstretched arm in sustaining a weight of one kilogram for one second upon a lever-arm of one meter. This work is evidently equivalent to the lifting of this same weight through a certain height.

As the unit of dynamical work, we shall take the dynamical work done by the horizontally outstretched arm, in lifting a weight of one kilogram, through a height of one meter, with an acceleration of one meter per second, on a lever-arm of one meter. The solution of the problem evidently consists in finding the relation between these two units.

In 1871 I published two series of experiments on this subject,† which consisted in lifting a weight w through a height h, in a time t, the intervals t being separated by equal intervals, during which the weight was allowed to drop, being caught on a cushion attached to the leg. In one series w was made variable, in the other series t varied. These experiments were repeated by Mr. Haughton, and in spite of my warnings in regard to the inaccuracy of the method, he has given them a "theoretical" reduction in his Principles of Animal Mechanics; London, 1873, pp. 462 and 475.

In order to eliminate the fatigue caused by the downward-plunging weight, the apparatus shown in figs. 1 and 2 was devised. B is a shelf armed with a plate of car-spring caoutchouc, for the support of the weight. This shelf is fastened by the iron bands I and a vertical back-piece V, to the slide U, being further supported by a cross-piece P. By using suitable blocks, G, the slide, may be raised to any desired height, which height is read off on a scale marked on the upright pieces A. D is used as a support for the arm during the interval of rest, and it can be adjusted to any desired height. S (fig. 2) is a horizontal string attached to the wire K, and to the wall of the room. R is an iron rod for holding the apparatus in position. The weight is a bucket of shot, provided with a stiff bail and wooden handle, so that during the lifting of the weight the line passing through the center of the hand and the center of gravity of the weight is a vertical. The experimenter stands to the right of the apparatus (as in fig. 2) and lifts the weight from the shelf B until his knuckles touch the cord S. (Back of hand turned upward.) The beginning and close of this interval of work is marked by the sharp click of a metronome, the time of whose beat is *t*. At the instant when the weight has reached the highest point, it is grasped by an assistant and lowered to the shelf, the arm of the experimenter being wholly relaxed, and resting upon the stiff bail of the bucket and the support D. The arm of the experimenter moves in the vertical plane which makes an angle of 45°, with the vertical plane passing through the centers of the shoulder joints.

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* We hope soon to be able to commence the series of experiments here indicated, the apparatus for which has been already devised.

† Hinrichs' "School Laboratory." 1871. p. 108.

Fig. 1. Fig. 2.

This process is kept up until the arm can no longer lift the weight to the required height. The determination of the number of lifts, *n*, should *never* be made by the experimenter, who should, furthermore, try to lose all estimate of time during the process. Only in this way can the mind be kept unbiased, so that the determinations are independent of each other.

In the earlier experiments it became evident that the arm not only grew gradually stronger, but also that it varied exceedingly in strength from day to day. In order to get some measure of the strength of the arm, I exhausted the arm each day of experiment with a 5·0 kgrs. weight, and reduced all of the experiments to the mean strength, as shown by the constant experiment. In this series, a weight, *w*, was lifted through a height, $h=0·70$ meters, in a time, *t*, of 1·25 sec. The interval of rest was also 1·25 sec. The experiments were so arranged as to make the constant experiment come, alternately, at 11 A. M. and 4 P. M., the other experiments alternating in like manner. The weight, *w*, was changed each day of experiment, so that in the table below the numbers on the horizontal lines are arranged as observed, the order of time being indicated by the indices in the first vertical column.

The columns headed *c* are the number of lifts n' for the constant experiment $w=5·00$. The mean value of *c* for the weights 3·0, 3·5, etc., 7·5. in all 100 experiments, is 35·79. The determinations of *n* for $w=7·5$ and 8·0 were consciously bad, as the arm was unable to manage such weights at such velocity, so that I was obliged to stop *before the arm was exhausted.**

RIGHT ARM. w variable. $h = 0·70$ meters. $t = 1·25$ sec.

	2·00	c	2·50	c	3·00	c	3·50	c	4·00	c	4·50	c	5·00	c	5·50	c	6·00	c	6·50	c	7·00	c	7·50	c	8·00	c
	239	34	140	33	124	31	69	27	53	34	36	25	32	27	26	32	19	31	16	30	15	36	11	37	7	28
	479	35	186	30	144	32	62	32	57	28	32	25	26	28	25	28	19	30	16	24	11	25	7	26	6	26
	445	35	239	33	138	42	79	37	61	34	38	28	33	32	26	33	19	31	17	30	17	36	10	33	7	28
	707	39	271	33	158	38	79	34	66	35	45	30	38	37	27	30	24	38	19	39	16	40	10	32	8	26
	----	--	208	33	128	37	97	35	80	35	47	39	44	45	25	33	25	37	20	40	14	33	12	37	9	37
	----	--	221	31	122	33	89	31	55	33	44	37	28	28	28	36	23	32	20	36	14	34	12	31	9	37
	----	--	245	30	177	31	124	38	103	48	61	48	41	40	34	39	26	43	19	39	15	35	10	35	7	46
	----	--	520	38	180	39	117	42	71	36	71	42	46	37	34	43	26	42	18	43	15	41	11	37	6	37
	----	--	340	34	216	42	129	39	87	44	67	37	31	34	25	38	23	38	18	47	16	43	8	45	5	39
	----	--	----	--	178	44	100	34	66	46	52	33	43	44	32	44	23	36	16	36	13	41	9	37	7	33

The values of n' for $w < 3·00$, were also rejected in the calculation of the constants, as with such light weights the work varies greatly with a slight variation of strength. [This will be fully discussed hereafter.] Taking the constant c as the measure of the strength, and assuming that the work done with any weight at different times is proportional to the strength, (which we will show to be true for the weights not rejected), and we have

Calling n'= observed number of lifts before exhaustion,
and n=the same, reduced to the basis the mean strength 35·79

$$n = \frac{35·79}{c} n'$$

from which formula we have the following values of n. The mean values of n are given in the eleventh line, and the prob-

	2·00	2·50	3·00	3·50	4·00	4·50	5·00	5·50	6·00	6·50	7·00	7·50	8·00
	251	152	143	91	56	51·5	42·4	29·1	21·9	19·1	14·9	10·7	8·9
	489	222	161	69	73	45·8	33·2	32·0	22·7	23·9	15·8	9·6	8·2
	454	259	118	76	64	48·5	36·9	28·2	21·9	20·3	16·9	10·8	8·9
	648	294	149	80	67	53·7	36·8	32·2	22·6	17·4	14·3	11·2	11·0
		225	124	99	82	43·1	35·0	27·1	24·2	17·9	15·2	11·6	8·7
		255	132	103	60	42·6	35·8	27·8	25·7	19·9	14·7	13·9	8·7
		292	204	117	77	45·5	36·7	31·2	21·6	17·4	15·3	10·0	5·4
		488	165	100	71	60·5	44·5	28·3	22·2	15·0	13·1	10 6	5·8
		358	184	118	71	64·8	32·6	23·6	21·7	13·7	13·3	6·4	4·6
			145	105	51	56·4	35·0	26·0	22·9	15·9	11·3	8·7	7·6
Mean	----	----	152·5	95·8	67·2	51·2	36·9	28·6	22·7	18·1	14·5	10·4	7·8
e----	----	----	5·7	3·5	2·0	1·7	0·9	0·6	0·3	0·2	0·1	0·1	0·4

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* This point is important. Try to lift 20 kgrs. in a second through 0·70 meters. You will fail to lift it once, and *yet not be exhausted.* The question of maximum velocity attainable with different weights is wholly different from the one under consideration. I think Mr. Haughton has overlooked this influence on his own experiments.

able error (e) of this mean is given in the twelfth line, and is calculated from the formula $e=\frac{2}{3}\sqrt{\frac{[d.d]}{v(v-1)}}$, where $v=$ the number of observations and $d=$ difference from the mean.

Assuming the arm to be a uniform cylinder, and denoting by α one-half the weight of the arm, and we have for any weight w the total dynamical work done before exhaustion.

$$\text{Total work} = (w+\alpha)h\,.\,n. \quad . \quad . \qquad (1)$$

The value of α can be determined by direct weighing, as follows: Exhaust the arm thoroughly, then holding it in the same position as when lifting, extend the arm horizontally, resting the hand in the scale-pan of a spring-balance, the dial of which is turned from the experimenter. The reading off of the weight is done by an assistant. After a few minutes the muscles tire so that a practical experimenter can then gradually relax them fully. Untrained muscles, when thus tried, act involuntarily, and precise results can not be obtained. The value of α was thus determined twenty times, the values being here given.

α (obs.)

1·46	1·42	1·63	1·42	1·46
1·42	1·58	1·42	1·48	1·40
1·42	1·52	1·48	1·58	1·50
1·50	1·49	1·62	1·58	1·57

$\alpha=1·50$

The mean is 1·50 kgr. with a probable error of 0·01 kgr. Hence (calling the total work=W), (1) becomes

$$W=(w+1·50)\ 0·70n.$$

Coördinating the values of W and w and the relation appears plainly hyperbolic. Hence the two most probable cases are

$$(w+\alpha)hn=\frac{c'}{(w+\alpha)^{v'}} \ \text{-------------} \ (2) \text{ and}$$

$$(w+\alpha)hn=\frac{c}{w^{v}} \ \text{-------------} (3)$$

where c and v are constants.

From these we readily have

$$\log(w+\alpha)+\log n=k'-v'\log(w+\alpha) \text{---------}(4)$$
$$\log(w+\alpha)+\log n=k-v\log w \text{--------------}(5)$$

These equations are of the form $y=k+vx$.

where y and x can be determined from the observations. They are given in the table below.

These values of x and y for eq. (4) and (5) are coördinated on the chart and a straight line, drawn as nearly as possible through the points, shows the functions to be linear in each case. v is the change in y, for each unit of change in x, and is for eq. (4), 2·58. For eq. (5) it is 1·99, which is essentially 2·0.

w	$(w+a)$	n	$\log w$	$\log (w+a)$	$\log n$	$(\log w+a)+\log n$
3·0	4·5	152·5	0·4771	0·6532	2·1833	2·8365
3·5	5·0	95·8	0·5441	0·6990	1·9814	2·6804
4·0	5·5	67·2	0·6021	0·7404	1·8274	2·5678
4·5	6·0	51·2	0·6532	0·7782	1·7093	2·4875
5·0	6·5	36·9	0·6990	0·8129	1·5670	2·3799
5·5	7·0	28·6	0·7404	0·8451	1·4564	2·3015
6·0	7·5	22·7	0·7782	0·8751	1·3560	2·2331
6·5	8·0	18·1	0·8129	0·9031	1·2577	2·1608
7·0	8·5	14·5	0·8451	0·9294	1·1614	2·0908

As will be hereafter shown, the observations are most nearly represented by eq. (3). It is, however, impossible to decide between equations (2) and (3) with absolute certainty, until the experiments are repeated with other values of h and t. For the present we assume the equation

$$(w+a)hn=\frac{c}{w^2}\text{.................}(6)$$

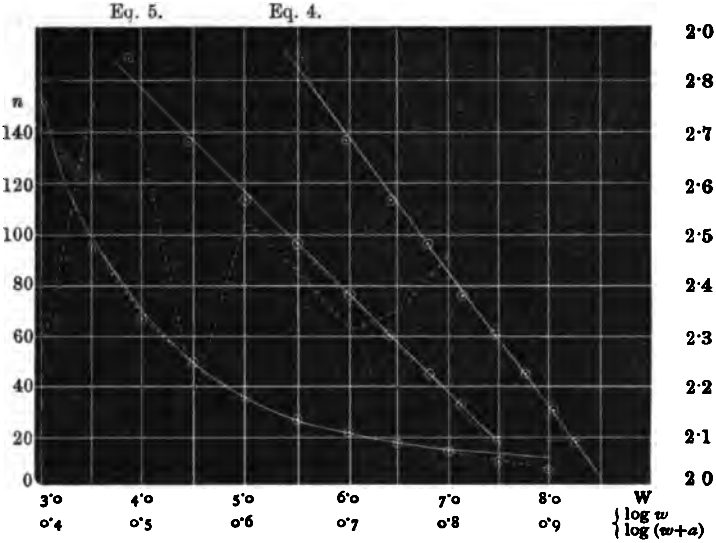
from which we readily have $w^3hn=c-aw^2hn$,
which is of the form of $y=c+au$.

By the method of least squares, the values of the constants are more accurately determined, and are found to be

$$a=1{\cdot}52 \qquad c=42{\cdot}61.$$

Solving (6) for n, and substituting the proper values, and we have the following comparison of the observed and calculated values of n. δn is n (calc.)—n (obs.). e is the probable error of n (obs.), also in per cent.

w	n (obs.)	n (calc.)	δn (%)	e (%)
2·50	283	242	—14·4	7·5
3·00	152·5	150·3	− 1·4	3·7
3·50	95·8	99·4	+ 3·6	3·6
4·00	67·2	69·2	+ 2·9	2·9
4·50	51·2	50·1	− 2·1	3·3
5·00	36·9	37·4	+ 1·3	2·4
5·50	28·6	28·7	+ 0·3	2·9
6·00	22·7	22·5	− 0·9	1·3
6·50	18·1	18·0	— 0·5	1·1
7·00	14·5	14·6	+ 0·7	0·7
7·50	10·4	11·9	+14·4	0·9
8·00	7·7	9·9	+28·5	5·2



The comparison of the calculated and observed values of n is very satisfactory. This is shown graphically on the chart, the small circles representing the observations. The values of δn, with the proper sign, are also there represented by the broken dotted line, the zero line being the horizontal line, which it repeatedly crosses. The observed value of α is 1·50. The calculated value is 1·52. The difference, 0·02 kgr., being 1·3 per cent of α observed.

Instead of eq. (6), Prof. Haughton has found the relation for the case here discussed, to be represented by the equation.*

$$(w+\alpha)^2 n = \text{A} \ldots\ldots\ldots\ldots (7)$$

where for my right arm he finds the values of the constants to be $\text{A}=1000$ and $\alpha=1{\cdot}0$. The experiments from which this formula was obtained are, as before said, unreliable, and the calculated value of α is 33 per cent less than the observed value. This relation would also demand that v', in equation (2), should equal unity. It was found to be 2·58.

In order to test the matter, experimentally, I performed two experiments as follows:

1. The arm alone ($w=0$) was lifted through 90° (from vertical to horizontal) in the time 1·25 seconds, the experiments being conducted exactly as described in my later experiments.

According to (7) for complete exhaustion $n=1000$
" (6) " " $n=\infty$

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* Principles of Animal Mechanics. London, 1873. p. 463.

The arm was lifted 2000 *times without feeling any appreciable exhaustion.*

2. A weight $w=0{\cdot}50$ kgr. was lifted, as before, except that the weight was allowed to *drop* during the interval of rest, as in my earlier experiments.

It was lifted 1500 *times with very little of fatigue.*

According to (7) for complete exhaustion $n=$ 440
" (6) " " $n=12000$

In the latter case the time of exhaustion would be $8\frac{1}{3}$ hours. The work done before exhaustion would be 17,000 kgr.-meters. The daily labor of a working man is about 100,000 kgr. meters. Considering these facts, and also the slight fatigue experienced by me, in experiment 2, I am convinced that my arm could work 8 hours and 20 minutes at the above rate, care being taken to avoid fatigue from other sources (standing on the feet in one position, etc.). It would, however, be a dangerous experiment.

Experiment 2 was also performed with a time $t=1{\cdot}164$ (the time taken in my earlier experiments, reduced by Prof. Haughton) with the same result.

This experiment also shows that eq. (2) does not represent this kind of work, for we deduce, from the chart, for the values of the constants

$$(w+\alpha)h\,.\,n=\frac{21240}{(w+\alpha)^{2{\cdot}58}}\cdot$$

making $w=0{\cdot}5$, and we have (since $\alpha=1{\cdot}5$ and $h=0{\cdot}7$) $n=2500$. The arm would be exhausted for 2500 lifts, which is hardly possible.

Anyone who will take the trouble to coördinate the corresponding values of c and n' as given in the first table, will see clearly that we were justified in retaining those values of n, and only those, which we there reduced. Each curve, however, gives evidence of the fact, that for any weight, n varies as some power of the strength.

We have made several series of experiments to investigate this point, determining the strength (s) of the muscle, by means of a dynamometer. Calling τ, the time of exhaustion I have found that

$$\tau=\alpha(s-\beta)^v,$$

where v is a function of the weight, α and β being also constant for the same weight. If the dynamometer really gave the absolute strength of the muscle, β would be equal to w. Precise values of these constants have not yet been obtained, and we therefore decline to discuss this equation any farther at present.

St. Louis, Oct. 31, 1874.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Constitution of Sulphurous Acid and Sulphites.*—MICHAELIS and WAGNER have made some experiments to determine whether sulphurous acid possesses the constitution assigned to it by Strecker, $H.SO_2.OH$, or has the more usual formula $HO.SO.OH$. Since, if the former formula be correct, the two hydrogen atoms have different positions in the molecule, it is obvious that the replacement of one of them by another atom or atomic group must give rise to two isomers; while only one compound is possible when they are both replaced by the same radical. By this reasoning, only a single ethyl-ether of sulphurous acid can exist, its formula being $C_2H_5.SO_2.OC_2H_5$. But in fact two bodies having the composition of ethyl sulphite are known; one boiling at 161° and the other at 207°. The former is the true ethyl-ether of sulphurous acid. The latter, prepared by the action of sodium ethylate upon ethyl-sulphon-chloride, must from its mode of preparation be $C_2H_5.SO_2.OC_2H_5$. Since the two bodies are not identical, it follows that the only possible formula for the former is $C_2H_5O.SO.OC_2H_5$, corresponding to an acid of the formula $HO.SO.OH$. This view the authors confirmed by acting with phosphoric chloride upon the sulphurous ether produced when sulphur chloride acts on alcohol. Ethoxyl-thionyl chloride, $Cl.SO.OC_2H_5$, was obtained; and this, as shown by Carius, for thionyl chloride, treated with alcohol, yields ethyl sulphite. This result proves that the equivalence of sulphur is different in sulphuric and sulphurous acids, being in the latter a tetrad.—*Ber. Berl. Chem. Ges.*, vii, 1073, Sept., 1874. G. F. B.

2. *On the Organic acids of crude Petroleum.*—HELL and MEDINGER have examined an acid liquid, obtained by agitating the second running (specific gravity 0·875) in the distillation of heavy Wallachian petroleum with caustic soda, and treating the flocculent precipitate thus formed, after solution in water, with sulphuric acid. This oily acid liquid, which collects on the surface, called mineral-oil acid by the workmen, is a mixture of probably homologous acids, whose separation was exceedingly difficult, fractional precipitation yielding semi-fluid products, and distillation producing decomposition. Finally, an ethyl-ether boiling constantly at 236°–240°, was obtained as a colorless oily liquid, highly refractive, and having an agreeable fruity odor and burning taste. Upon saponification, and subsequent decomposition by sulphuric acid, a colorless acid of specific gravity 0·982 at 0° was obtained, which boiled constantly at 258°–261° without decomposition. It is a weak acid, its ammonium salt being decomposed by water in excess. The sodium and potassium salts closely resemble soft-soaps and could not be obtained in the solid form. Analysis gave as the probable molecular formula of the acid $C_{11}H_{20}O_2$. Its reactions show that it does not belong to either

of the three series of fatty acids at present known, and the authors believe it to be a member of a new series characterized by a peculiar mode of union of the carbon atoms.—*Ber. Berl. Chem. Ges.*, vii, 1216, Sept., 1874. G. F. B.

3. *On the Direct Synthesis of Methyl aldehyde.*—**Brodie** has succeeded in effecting a direct synthesis of methyl aldehyde by passing the induction spark through a mixture of carbonous oxide and hydrogen. In his earlier experiments, in which equal (or nearly equal) volumes of the two gases were used, the only other product obtained was marsh gas; and this equally whether carbonous oxide or carbon dioxide was used. By a modification of the conditions of the experiment with carbon dioxide, he has effected the expected synthesis, the 188·6 volumes of gas remaining after the removal of the nitrogen, oxides of carbon, and a trace of oxygen, being composed of 183·2 of hydrogen, 0·2 of marsh gas and 5·2 of methyl aldehyde. This latter amount being equivalent to 2·76 per cent. The reaction appears to be as follows:

$$CO_2+(H_2)_2=COH_2+H_2O$$

Brodie believes that methyl aldehyde was formed in his former experiments, but was subsequently decomposed thus:

$$(COH_2)_2=CO_2+CH_4$$

Ann. Ch. Pharm., clxxiv, 284, Nov., 1874. G. F. B.

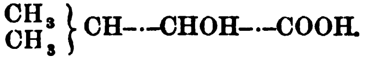
4. *Synthesis of an Isomer of Cane-sugar.*—The relation of the glucoses, or simple sugars, to the saccharoses, or compound sugars, is very obvious from their formulas; two molecules of the former by the loss of a molecule of water being united into one molecule of the latter. While it is quite possible by means of ferments, acids, and the like, to reverse this operation and produce the simple sugars from the compound ones, it has not hitherto been possible to build up the latter from the former. **Gautier** has now succeeded in withdrawing a molecule of water from two molecules of dextrose, one of the simple sugars, and thus forming a body having the composition of a compound sugar. The result was effected by dissolving perfectly pure dextrose in nearly absolute alcohol, cooling the solution by means of ice, and then passing very slowly through it a current of dry hydrochloric acid gas. After saturation, the liquid is evaporated in a vacuum, neutralized by barium carbonate and hydrate, treated with strong alcohol, again evaporated, treated with ether to remove a bitter substance, and finally dried in vacuo at 100°. A colorless body is thus obtained, analogous to gum and dextrin in appearance and taste, very soluble in water, and very hygroscopic. Upon analysis, its composition was found to be $C_{12}H_{22}O_{11}$. It has no sweet taste, is not precipitated by ammoniacal lead acetate, reduces the copper test only with difficulty and is dextrogyrate. It is not fermentable, and is decomposed again on heating its aqueous solution to 160°, into two molecules of a simple sugar; but this sugar appears not to be identical with the primitive dextrose, but to be analogous though not identical with inosite, reducing the

copper test but not being fermentable. The author suggests that the acetylic derivative of dextrose obtained by Schützenberger, $C_{12}H_{14}(C_2H_3O)_8O_{11}$, is the octacetylic derivative of this new compound.—*Bull. Soc. Ch.*, II, xxii, 145, Sept., 1874.

5. *On Maltose.*—**Schulze** has re-investigated and confirmed the statement of O'Sullivan that the action of malt-extract upon starch produces, not dextrose as had been supposed, but a compound sugar having the formula $C_{12}H_{22}O_{11}$, which he called maltose. This sugar reduces the copper test only two-thirds as actively as dextrose, but possesses a much greater rotatory power. Schulze's experiments were conducted with diastase, precipitated from malt-extract by alcohol. Starch paste treated with this, at a temperature of 60°, was rapidly saccharified. After concentration and precipitation of the dextrin by alcohol, the solution was evaporated to a syrup treated with alcohol, the clear solution poured off and evaporated over sulphuric acid. Crystals first appeared on the walls of the vessel, and finally the whole solidified to a crystalline mass. Recrystallized from water or alcohol, maltose retains crystal water, which is readily given up at 100°. In appearance, maltose resembles dextrose but is distinguished from it by its composition, its reducing power, and its action on a polarized ray. By boiling with dilute acids it is converted into dextrose. It is therefore an intermediate product between dextrin and dextrose.—*Ber. Berl. Chem. Ges.*, vii, 1047, Sept., 1874.

G. F. B.

6. *Synthesis of Leucic acid by means of its Nitrile.*—**Erlenmeyer** and **Sigel**, by acting on amyl aldehyde—obtained from the alcohol produced by fermentation—with hydrocyanic acid, have produced the true nitrile of leucic acid, as a colorless, peculiarly smelling oil, insoluble in water. It cannot be distilled, being decomposed into the bodies from which it was formed. Potassium and sodium hydrates also decompose it similarly. Analysis gave it the formula $C_6H_{11}NO$. Fuming hydrochloric acid decomposes it, producing ammonium chloride and leucic acid. On the addition of water the latter product separates as an oil, sinking to the bottom. It is removed by means of ether, which leaves it on evaporation as a syrup, from which after a time large transparent crystal plates of the acid separate, having the composition $C_6H_{12}O_3$. There can be no doubt therefore that leucic acid is thus constituted :—

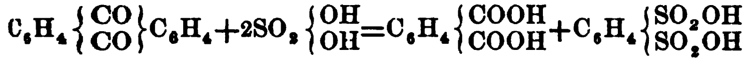


$$\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}CH\text{--}CHOH\text{--}COOH.$$

Ber. Berl. Chem. Ges., vii, 1109, Sept., 1874. G. F. B.

7. *Relation of Phthalic acid to Anthraquinone.*—During the preparation of sulpho-anthraquinonic acid in a Basle alizarin manufactory a considerable quantity of a crystallized sublimate was obtained in large colorless needles, which was soluble in water and crystallized therefrom in brilliant plates. **Weith** and **Bindschedler** examined it and proved it to be phthalic acid, by conversion into the anhydride and into calcium and barium phthalates,

as well as into phthalimide. Further investigation showed that anthraquinone carefully purified from phthalic acid, heated with four parts of fuming sulphuric acid for six hours to 270°, gave colorless needles of phthalic anhydride, nearly an inch in length. Hence anthraquinone must be regarded as the ketone of phthalic acid, the decomposition taking place as follows:



$$C_6H_4\left\{\begin{matrix}CO\\CO\end{matrix}\right\}C_6H_4+2SO_2\left\{\begin{matrix}OH\\OH\end{matrix}\right.=C_6H_4\left\{\begin{matrix}COOH\\COOH\end{matrix}\right.+C_6H_4\left\{\begin{matrix}SO_2OH\\SO_2OH\end{matrix}\right.$$

Ber. Berl. Chem. Ges., vii, 1106, Sept., 1874. G. F. B.

8. *Simple preparation of Uræmatin from Hæmatin.*—Hoppe-Seyler has identified the coloring matter which he had produced by acting upon hæmatin, the coloring matter of the blood, with reducing agents, especially tin and hydrochloric acid, with that obtained from the urine by Jaffe and called urobilin, and that produced from bilirubin by Maly by the action of sodium amalgam. Hence it follows that the coloring matter of normal fæces, and of urine, is only a reduction derivative of the coloring matter of the blood, and that the biliary coloring matters bilirubin and biliverdin are intermediate stages in this reduction. The principal normal coloring matter of the urine, however, is not urobilin but uræmatin; but Hoppe-Seyler states that this is easily derived from urobilin, and that too, by reduction. This discovery opens the way for the solution of interesting questions on the disintegration of the blood-corpuscles under pathological conditions.—*Ber. Berl. Chem. Ges.*, vii, 1065, Sept., 1874. G. F. B.

9. *Asparaginic acid as a product of Pancreatic digestion.*—Since asparaginic acid has been proved to be a product of the splitting up of the albuminates, both vegetable and animal, it occurred to Radziejewski and Salkowski to ascertain whether natural ferments such as that of the pancreatic juice, produced asparaginic acid in their action upon blood-fibrin. Fresh blood-fibrin was digested at 40°–50° with the pancreas of the ox for several hours. Upon testing the solution obtained, asparaginic acid was detected; its identity being established by conversion into the copper salt and by elementary analysis. The formula $C_4H_7NO_4$ requires 36·09 of carbon and 5·26 hydrogen. Analysis gave 35·76 carbon and 5·68 hydrogen.—*Ber. Berl. Chem. Ges.*, vii, 1050, Sept., 1874. G. F. B.

10. *Sympathetic Vibrations*—M. E. Gripon has studied at length the influence exercised on the vibrations of a column of air by neighboring sonorous bodies. He concludes that the pitch of a vibrating mass of air is raised by bringing near its orifice an elastic membrane or a second mass of air which alone would give the same note. A similar effect is produced, but in a less marked manner if the second mass of air or membrane, has a higher pitch than the first. The pitch of a membrane, on the other hand, is lowered by bringing a solid body near it. In order that a layer of air, bounded on one side by a membrane and on the other by a plane parallel to it formed by a solid free around its edge, may be reinforced by a given sound, its thickness must be proportional to its length;

further, this thickness depends on the ratio which exists between the sound proper to the membrane and the given sound, and also on the nature and dimensions of the membrane.

We can, with sounding tubes with flute embouchure, reproduce the principal phenomena obtained with singing flames.

The pitch of a pipe falls when we bring a solid body near its orifice. This flattening is still produced when the pipe is the center of a solid plane which extends indefinitely around the pipe. The change in pitch is proportional to the number of vibrations of the pipe, and within certain limits nearly proportional to the breadth of the solid rim surrounding the pipe; it is, on the contrary, constant starting from a certain breadth, and then proportional to the diameter of the pipe.—*Ann. Chem. et Phys.*, iii, 343–390.

E. C. P.

11. *Electrical Resistance.*—M. Benoit has measured with great precision the electrical resistance of various metals at temperatures from 0° to 860°. He employed both the method of the differential galvanometer and of the Wheatstone's bridge, and for each metal has measured several specimens. The mean of these is given in the following table, the second column giving the resistance of a wire one meter long and having a cross section of one mm. in Ohms, and column three the same quantity in Siemens units. Column four gives the resistance compared with silver:

Metal.	Ohms.	Siemens.	
Silver, A	·0154	·0161	100·
Copper, A	·0171	·0179	90·
Silver, A (1)	·0193	·0201	80·
Gold, A	·0217	·0227	71·
Aluminum, A	·0309	·0324	49·7
Magnesium, H	·0423	·0443	36·4
Zinc, A., at 350°	·0565	·0591	27·5
Zinc, H	·0594	·0621	25·9
Cadmium, H	·0685	·0716	22·5
Brass, A (2)	·0691	·0723	22·3
Steel, A	·1099	·1149	14·0
Tin	·1161	·1214	13·3
Aluminum Bronze, A (3)	·1189	·1243	13·0
Iron, A	·1216	·1272	12·7
Palladium, A	·1384	·1447	11·1
Platinum, A	·1575	·1647	9·77
Thallium	·1831	·1914	8·41
Lead	·1985	·2075	7·76
German Silver, A (4)	·2654	·2775	5·80
Mercury	·9564	1·0000	1·61

A, annealed; H, hardened; (1) silver ·75; (2) copper 64·2, zinc 33·1, lead 0·4, tin 0·4; (3) copper 90, aluminum 10; (4) copper 50, nickel 25, zinc 25.

These results, which are all taken at 0°, agree closely with those obtained by other observers. M. Benoit has extended his observations to a range of temperature much greater than those previously employed for this purpose. He wound the wire around a clay pipe enclosed in a muffle and immersed the whole in a bath of water, mercury, sulphur or cadmium, which was kept at the boiling point by a Perret furnace. Constant temperatures of 100°, 360°, 440° and 860° were thus obtained. Various temperatures below 360° were also obtained by a mercury bath. The measures were also corrected for expansion. Plates annexed to his memoir, presented to the Faculty of Sciences of Paris, show the results graphically. They show that the resistance increases regularly for all metals like tin, lead and zinc up to their point of fusion. This increase, however, differs for different metals. We notice that tin, thallium, cadmium, zinc, lead, are found together in the upper part of the plate; at 200° to 230° their resistance has doubled. Below them are iron and steel; for the last the resistance doubles at 180°, quadruples at 430°, and at 860° is about nine times that at 0°. Palladium and platinum, on the other hand, increase much less and only double their resistance at 400° to 450°. Gold, copper and silver form an intermediate group. In general the conductibility decreases more rapidly in a metal the lower its point of fusion. Iron and steel are an exception to this rule. In alloys the variation is always less than in their constituents, and this is especially the case with German silver.—*Bib. Univ.*, cciii, 284. E. C. P.

12. *Reflection by Glass.*—Dr. P. GLAN has measured the amount of light reflected by a plate of glass and compared the results with those given by theory. The apparatus employed consisted of a doubly-refracting prism attached to the collimating lens of an optical circle, so that its principal section was parallel to the slit. The objective of the observing telescope carried a Nicol with a divided circle reading to minutes; and the spectral analysis of the light was effected by the prisms of a Hoffman spectroscope set on the table of the apparatus. The slit was divided into two parts by a strip of tinfoil, whose breadth was such that the ordinary image of one-half and the extraordinary image of the other were in exact contact, consequently by turning the Nicol the two adjacent spectra could always be brought to precisely the same degree of brightness. Owing to the dispersion of the doubly-refracting prism, perfect contact was only possible for one color, but the spectra were of such a length as to render the contact close enough over a space of considerable width; by turning the doubly-refracting prism this contact could be effected in any part of the spectrum. A rectangular prism was placed over the lower half of the slit and reflected through it the light of a petroleum flame while in front of the upper half a small spectrometer was placed, by which the light reflected from the glass was passed through the apparatus. By turning the Nicol the two images were rendered equal, and each observation was repeated five times, using the light of wave-length equal to that of thallium.

In the following table the first column gives the angle of incidence, the second the mean of twelve observations of the amount reflected by a prism of crown glass, the third the amount reflected according to Fresnel's formula, and the fourth and fifth the observed and computed reflections for a similar prism of flint glass. The incident beam is in all cases taken as 100.

	Crown glass.		Flint glass.	
I.	Obs.	Calc.	Obs.	Calc.
30°	5·5	5·9	7·0	7·1
40	7·2	7·3	8·4	9·3
50	10·4	11·4	12·0	13·3
55	13·3	14·1	16·1	16·2
60	17·4	17·9	20·3	20·3
65	23·1	22·9	25·4	25·7
70	29·3	30·2	32·7	33·0

The indices of refraction were found from the angle of total polarization, since Seebeck has shown that the density and, therefore, the refraction of the surface may be considerably altered by grinding. For the crown glass this method gave $n=1{\cdot}507$, and for the flint glass $n=1{\cdot}577$. It is evident that the agreement with theory is all that could be expected, since the individual observations differ in some cases as much as four per cent from one another.—*Monats. Acad.*, *Berlin*, 1874, p. 511; *Phil. Mag.*, xlviii, 475.

E. C. P.

13. *Electrical Polarization.*—M. G. QUINCKE describes in detail a great number of experiments on the electrical currents accompanying the non-simultaneous immersion of two mercury electrodes in various liquids, and has arrived at the following conclusions:

If two mercury electrodes, connected by the wire of a multiplier, be immersed one after the other in any liquid which is a conductor of electricity (water, alcohol, saline solutions, etc.), an electric current is observed passing from the freshly wetted mercury surface through the liquid to the other mercury surface. The strength of the current diminishes as the resistance of the liquid column between the electrodes is increased. The electromotive force varies with the nature of the liquid and increases as the concentration diminishes, in some cases amounting to 0·6 of a Volt. The electromotive force increases if the boundary surface of mercury with the surrounding liquid in the last immersed electrode is more quickly produced. It soon, however, reaches a maximum, especially in the case of viscous liquids like glycerine. The cause of these currents is probably the alteration in molecular condition (change of density or concentration), which is gradually accomplished in the liquid near the surface of contact after the wetting. Similar effects are obtained with solid metals, as with mercury. The currents formed with acids are due chiefly to chemical action, and are, therefore, secondary phenomena. The surface tension may be either increased or diminished and may change its sign with the direction and duration of the current. The disturbances in capillarity can not be accounted for by electrolysis.—*Pogg. Annal.*, cliii, 161–203; *Phil. Mag.*, xlviii, 479.

E. C. P.

II. GEOLOGY AND NATURAL HISTORY.

1. *On the Cosmical dust which falls to the earth with atmospheric precipitation;* by A. E. NORDENSKIÖLD.—In the early part of December, 1871, the region about Stockholm was visited by a snow fall of unprecedented magnitude. Nordenskiöld availed himself of this opportunity to investigate whether the apparently pure snow did not contain particles of foreign matter. A cubic meter of the snow was collected and melted with all possible precautions: it was taken, moreover, during the latter part of the storm (which lasted several days), when the atmosphere must have been purified from all accidental dust. Notwithstanding this, a residue was obtained consisting of a black powder, from which, when heated in a matrass, a fluid product was distilled off, and which gave, on being ignited, a reddish-brown ash. It contained also magnetic particles, which gave the reaction for iron.

The experiment was not in all respects satisfactory, as it was at least supposable, notwithstanding the precautions taken, that the organic matter might have had its origin from the thousands of chimneys about Stockholm, and that the metallic iron might have been derived from the roofings of the houses, covered up though they were at the time by several feet of snow. In order to obtain more positive results, the experiment was repeated in a remote forest district of Finland. A large body of snow was collected with care and melted. The result in this case also was a sooty powder, consisting of a black coal-like mass with minute magnetic particles, which proved to be metallic iron. The quantity of material obtained was too small to allow of its being tested for cobalt or nickel.

Still more conclusive evidence to the same point was obtained in the winter of 1872, when Nordenskiöld, in connection with the polar expedition, spent the winter on the northern coast of Spitzbergen. At this point (lat. 80° N.) most remote from all human habitations, the investigations were further carried on. The snow which covered the drift ice, which itself had come from much higher latitudes, was found to be thickly dusted with minute black particles; they rested in part on the surface, and were in part enclosed in the icy snow mass some inches below. The dust was black when collected, but became gray on drying. It proved to contain metallic particles which, with sulphate of copper, gave a precipitate of metallic copper. The experiment was repeated later, the material being obtained from a layer consisting of a granular crystalline mass of altered snow, covered by eight millimeters of old hardened snow, and that in turn by 50 mm. more, which was loose and had recently fallen. At this place it was estimated that about 0·1 to 1· milligram of the magnetic particles were contained in a square meter. Enough of the material was obtained to allow of some qualitative experiments, and it was proved to contain, beside metallic iron, also phosphoric acid, cobalt and probably nickel. The portion insoluble in acids consisted of a firm, angular, colorless powder, in which some fragments of diatoms wer observed.

This material has much resemblance to the remarkable dus found by Nordenskiöld scattered on the surface of the ice in th interior of Greenland, as also at a distance of 30 miles from th coast, and to which he gave the name *cryoconite.* This consiste for the most part of minute angular crystalline grains, which wer colorless and transparent, with fragments possibly of feldspar an augite crystals, and some black magnetic particles. In an analysi the cryoconite was proved to consist of silica, alumina, oxide o iron, manganese, magnesia, potash, soda, with traces of chlorin and organic matter, and to give the oxygen ratio for the protoxyds alumina, silica and water, 2 : 3 : 14 : 1. Its specific gravity is 2·63 and the crystalline form is monoclinic.

Nordenskiöld shows that the cryoconite must have had either cosmical origin, or have come from Jan Mayen, or else some unknown volcanic region in the interior of Greenland, while th presence of cobalt, and probably nickel, would seem to prove tha a part of the dust at least had a cosmical origin. He finally come to this conclusion—that small quantities of a cosmical dust, containing metallic iron, cobalt, nickel, and phosphoric acid and als a carbonaceous organic matter, falls upon the earth along wit atmospheric precipitation.—*Pogg. Ann.*, cli, 154, 1874.

[The dust, or cryoconite, has nearly the composition of an oligoglase-trachyte.—E. S. D.]

2. *On Middle Park Mineral Coal;* by E. J. MALLETT. (Fro the *Rocky Mountain News*, Denver, Colorado, of Nov. 19, an dated Territorial School of Mines, Nov. 17, 1874.)—I have examined specimens of coal from Middle Park, and herewith communicate the results, which may be of interest to your readers.

The coal presents somewhat the appearance of bituminous coa of the older epochs, and does not have much tendency to slack o break to pieces. When pulverized, it loses its jet-black colo assuming a dark vandyke-brown. * * * * *

Although it might be classed as caking-bituminous coal, it diffe in the following respects from coals of that class:

First. In the nature of the residue left after distillation—a tru coke, something similar to that which would be derived from distilling sugar, but too porous and crumbling to support burden in furnace.

Second. In the large amount of gas and tarry oil produced.

Third. In the brown color of its powder.

English cannel coal was placed in another retort and heate simultaneously with the Middle Park coal, in order to compa roughly the relative amounts of gas. Apparently the domesti coal gave about thirty per cent more gas, of high illuminatin power, and very much more liquid distillate than the English coa When the coal is boiled with concentrated solution of potas it does not affect the color of the solution. Lignitic or brown co colors potash solution brown.

Considering all of its properties, I may say that it possesses much in common with the recently discovered mineral called *albertite*, a species of solidified petroleum, and also with what is known as *torbanite*. These two varieties are highly valued by gas manufacturers, who mix from five to twenty per cent of these bituminous compounds with less bituminous coal, thereby greatly increasing the yield and quality of the gas. It resembles the former in the large amounts of gas and tarry oil it yields (which may prove as valuable as that derived from albertite), but differs from it in being heavier—the specific gravity of the albertite being 1·090, while this is 1·323,—also in yielding no soluble products when treated with bisulphide of carbon, spirits of turpentine, ether, etc. From torbanite it differs, in not crackling in the fire, in being much heavier, and in melting and intumescing when heated. Analysis shows it to contain in one hundred parts 6·02 per cent of water and moisture, 39·95 per cent of volatile matter (gas and tarry oil), 54·03 per cent of fixed residue, consisting of coke and ash.

As much confusion exists in the nomenclature of the mineral fuel of Colorado, I would propose, as I recently did to Professor Hayden, to give up entirely the term lignite as a special class name. If we call our mineral fuel lignite, we must conclude that lignite can be either bituminous-caking, bituminous-non caking, anthracite, or possess an organic structure. The term lignite should be dropped, as being inapplicable when applied to our mineral fuel as a class.

The coal from Middle Park is of the class caking-bituminous, and being a peculiar variety, might be distinguished as "Byerite."

3. *Volcanic Phenomena of County Antrim and adjoining Districts, Northeastern Ireland;* by Prof. Edward Hull. (Proc. Brit. Assoc., Aug., 1874.)—The Antrim igneous rocks, which include those of the Giant's Causeway, are referred to the closing part of the Eocene period and the whole of the Miocene. The earlier of the ejected rocks—probably later Eocene—are stated to be trachytic lavas; they have a feldspathic base and contain crystals of sanidin and also grains of quartz. They come from many vents and have a thickness in some places of 600 feet. Over these there are sheets of augitic lavas (which are separated often by layers of reddish clayey or soil-like material), and also beds of volcanic ashes. The ash-beds of Ballypalidy, the Causeway and other places contain fossil plants of Miocene character. Following the ash depositions, after a considerable interval, there were other augitic eruptions in vast sheets, covering in some places beds of lignite and others of pisolitic limonite. The whole thickness of the basaltic sheets of Antrim is estimated to be 1100 feet; or 1300 feet for the basaltic and the underlying trachytic sheets. This, though so large, is far short of the thickness on Mull, which is made by Geikie to be 3,000 to 4,000 feet. In both regions the thickness has been reduced by denudation. Mr. Hall states that the thousands of dikes in northeastern Ireland—whose aggregate breadth would be several thousand feet—were made without any crumpling of the strata.

The augitic rock consists of crystalline grains of augite ar
labradorite with grains of titano-ferrite [a titaniferous magnetite ?
and often also of chrysolite. Owing to the amount of iron in t
rock, its decomposition has produced beds of pisolitic limonit
No distinct remains of volcanic cones can now be distinguished
the region.

4. *Report of the Geological Survey of Missouri, includi*
field-work of 1873–4, with 91 Illustrations and an Atlas; by **GA**
LAND C. BROADHEAD, State Geologist. 734 pp. 8vo. Jeffers
City, 1874.—We cite the following from this valuable Report.

The "Historical Notes," with which the Report opens, show th
lead-mining was begun in Missouri in 1719, but was prosecut
only on a small scale for many years. In 1823 the annual yie
was reported at 3,000,000 pounds; in 1873 it reached 27,676,3
pounds. The first iron was made in 1816; the first zinc, in 186

The Lower Drift includes large boulders of granite, red quartzyt
greenstone, etc., which increase in quantity and size as we
north. In Sullivan County, a granite boulder measures 20 by
feet. The Missouri River seems to limit their southern extensio
for only small and scattered ones are seen farther south, and the
mostly rounded, as if by stream action. It is very probable th
anterior to the deposit of this rounded drift, but subsequent to t
age of deposit on the highest land, an immense lake covered t
larger part of the State, including all of North Missouri, St. Lou
County, the counties on the Missouri River above Moniteau Count
and a portion of the border counties, while all Central and Sout
ern Missouri formed an extensive area of dry land. We have e
dence in North Missouri of great erosion previous to the existen
of this immense lake.

The total area of the Missouri coal-field is estimated at 23,1
square miles.

The term Choteau Group is used as the equivalent of Kind
hook, on the ground of priority; but, since it was originally a
plied to but one of the three beds now included, while t
"Kinderhook," from the first, was used to include all three
Choteau Limestone, Vermicular Sandstone and Lithographic Lin
stone—with their equivalents elsewhere, this use seems liable
cause needless confusion. The "greater thickness and better d
velopment" of the group in Missouri does not appear to us s
ficient reason for replacing a name in itself unobjectionable.

After brief summaries of facts concerning caves, water-supp
soils, economic minerals and rocks, and topographical features
the southwest coal field, there follow over three hundred pages
county reports. The following chapters, on lead, zinc and ir
ores, by Schmidt and Leonhard, give many interesting details co
cerning the occurrence and relations of these ores. At Joplin a
Oronogo, the galenite is frequently associated with bitumen, whi
sometimes entirely impregnates the ore. This bituminous galen
has a darker appearance than the common ore, being often de
black in color. Bitumen is of quite common occurrence in t

Joplin mines. Analyses of galenite, from nine different localities, show an average of a little over an ounce of silver to the ton.

At the Oronogo mines, specimens have been obtained, which show very plainly the gradual change from galenite to cerussite. Crystals or masses of galenite, imbedded in altered and softened limestone, seem to have been coated at first by a compact layer of cerussite. Through this layer, solutions containing carbonic acid penetrated and gradually dissolved the galenite, precipitating a part of the lead as carbonate on the inside of the coating, but carrying most of it out of the shell, either to deposit it immediately or to carry it away. When this process has been continued for some time, we find a round or oval shell of compact and generally crystalline cerussite, of the size of a walnut, or sometimes much larger, containing a dark, transparent, watery fluid, in which a piece of galenite, smooth and rounded, but irregular in shape, is lying loose. If this process continues, the galenite dissolves entirely, and a cavity lined with cerussite crystals is the final result.

In Southwest Missouri, chert (of Subcarboniferous age) occurs in heavy beds. The greatest observed thickness is 105 feet. This is here one of the principal ore-bearing rocks, containing galenite in numerous sheets or seams. It is evident, in all places, that the galenite was formed after the chert had been formed and hardened and in many places broken up. The limestone has undergone, in many places, a process of dolomization. Solutions penetrating it have dissolved the carbonate of lime and partly replaced it with carbonate of magnesia. With this change a contraction is connected; cracks are opened in the mass, and filled with crystallized dolomite. The change was always begun either in fissures or on the surfaces of layers or of broken off blocks, and gradually proceeded toward the interior of the rock. It seems to have preceded the formation of the ore, and to have continued during the whole process. The deposition of zinc ores, especially calamine, and of galenite, is evidently intimately connected with the dolomization. At Granby, the alteration extends through horizontal zones of irregular outline, generally from two to six feet high, above which the solid beds are undisturbed. In the Joplin districts, the ore-deposits are mostly in the form of "runs," extending principally in one horizontal direction, rarely more than five feet wide, and limited in height by the layers of chert above and below the limestone layer, in the crevice of which the run originated. Another mode of alteration of these rocks consists in a gradual solution and removal of the limestone, either unaltered or more or less dolomized. This was undoubtedly effected by water containing carbonic acid. Large caves are produced by this dissolving process, whenever the layers of chert above do not happen to break down.

The lead ores of Central Missouri are mostly in the Lower Silurian limestones, though partly, also, in the Subcarboniferous, as are all those of the southwest portion of the State. None of the deposits as yet worked have proved to be paying to a greater depth than about 80 feet below the surface. This fact is independent of the geological position of the deposits. It is a remarkable fact, also, that while barite is entirely missing in the southwestern region, this mineral is, in this central region, a nearly constant associate of the galenite in the Carboniferous as well as in the Silurian rocks. These facts suggest the idea, as Mr. Schmidt observes, that the occurrence of lead ore, with its associates, is not exclusively dependent on the geological formation, but that its deposition may have taken place simultaneously in similar rocks belonging to different geological periods, which rocks happened to be under similar conditions at the time of deposition. If this suggestion is correct, it would throw the origin of all the galenite of the central and southwestern lead regions into a later period than the Subcarboniferous. As galenite is also found in the seams and partings of the coal strata in Simpson's coal-mine, situated in the lead district of southern Moniteau County, which strata, although belonging to a separate basin, must be supposed to have been formed simultaneously, or nearly so, with the North Missouri coal-field, we may conclude that the galenite is of much later origin, even, than the Coal-measures.

The Atlas accompanying the Report contains geological maps of Cedar, Barton, Vernon, Bates, Howard, Madison, Jasper and Newton Counties, with uncolored maps of the lead districts, and three sheets of parallelized sections of the Coal-measures. These last seem to us objectionable, in that they give but little information, while involving great expense. The *field-notes* thus published might better be reserved for the special study of their author, and only the *thoroughly digested general section* sent to the printer.

5. *Geological Survey of Victoria.* Melbourne, 1874. (London: Trübner & Co.).—Two volumes of Reports on the Geology of Victoria have recently been issued: one, a Report of Progress, by R. BROUGH SMYTH, Secretary for Mines for the Colony, with additional reports on the mineral resources of Ballarat, by R. A. F. MURRAY, and on certain Coal-fields; and the other, Decade I, on the Paleontology of Victoria, by FREDERICK McCOY. The plants of the Coal-fields are mostly similar to those of Australia; but no species of *Glossopteris*, the very common Australian genus, have yet been found, and three are mentioned of the genus *Zamites*. *Glossopteris Browniana*, the most abundant Australian species, has, however, been reported from Southeastern Tasmania, along with *Pecopteris Australis*, a plant of both the Australian and Victoria Coal-fields. Professor McCoy makes all these Coal-measures Mesozoic. While no Lepidodendrids occur in these Coal-measures, a species is reported from a sandstone of Gippsland, which is very similar to the European Carboniferous *Lepidodendron tetragonum* Sternb. These beds are supposed to be Subcarboniferous.

The most interesting discovery mentioned in the Reports is that of the occurrence of many American species of Graptolites in the slates of Victoria. Mr. McCoy has identified 12 to 14 species of this group; and, of these, Decade I contains descriptions and figures of the following, which are described by Hall from the

Quebec group of the Lower Silurian: *Phyllograptus typus* Hall, *Didymograptus mucronatus* Hall, *Diplograptus pristis* (*Prionotus pristis*, var. B., Hall), *D. bicornis* Hall, *Graptolites fruticosus* Hall, *G. quadribrachiatus* Hall, *G. bryonoides* Hall, *G. octobrachiatus* Hall, *G. Logani* Hall. The last four are very common species both in Canada and Victoria, though rare in Europe.

6. *Report upon Vertebrate Fossils discovered in New Mexico, with descriptions of new species;* by Prof. E. D. COPE, Paleontologist. 18 pp. 8vo. Geogr. Expl. and Surveys west of the 100th Meridian, First Lieut. G. M. WHEELER, Corps of Engineers, U. S. Army, in charge.—Professor Cope states that the Eocene discoveries show that an Eocene lake extended over the part of New Mexico now drained by the tributaries of the Chama River on the east and the San Juan on the west. The Mammalian remains differ from those of the Fort Bridger Eocene in the absence of species of the genus *Palæosyops*, and its replacement by *Bathmodon* Cope, and by the presence of only one small *Hyrachyus*. Four new species of Toxodonts are among the discoveries reported; they are referred to the new genera *Ectoganus* and *Calamodon*.* The teeth of six or seven species of sharks and one *Ostrea* have been found with the Mammalian remains.

7. *Twenty-sixth Annual Report on the New York State Museum of Natural History;* by the Regents of the University of the State of New York. Transmitted to the Legislature, May 2, 1873. 192 pp. 8vo. Albany, 1874.—The series of Reports on the New York State Museum, which has now reached its 26th number, is of great value to science on account of the descriptions of new species contained in it, and especially of species of fossils by Professor JAMES HALL. The volume just issued contains descriptions of over 50 species of Bryozoans and Corals of the Lower Helderberg group, by Professor Hall; besides, also, Entomological Contributions, No. III, by J. A. LINTNER, including notes on the larves or transformations of various insects, and descriptions of some new species; and also a report of the Botanist, C. H. PECK, containing among its tables a list of plants hitherto unreported from the State, with various notes.

8. *Wind-drift erosion: Note by* G. K. GILBERT. (Communicated.)—At the Hartford meeting of the American Association for the Advancement of Science, a paper was presented by the writer upon "Erosion by Sand in the Western Territories," and an abstract of the same is already in type for the forthcoming volume of the Proceedings. One of the topics discussed was the degradation of desert plains by drifting sand—a degradation evinced by the peculiar worn surfaces of the pebbles which strew the plains. The hard pebbles exhibit a high polish; the softer parts of the heterogeneous are deeply scored; and pebbles of limestone are carved with an arabesque plexus of furrows. These phenomena were briefly noted by Dr. J. S. Newberry in 1861 (Geol.

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* According to Professor Marsh, this genus is identical with his *Stylinodon*, described in this Journal in vol. vii, p. 532, May, 1874, where its resemblances to *Toxodon* were mentioned.—EDS.

Ives' Exped., pp. 17 and 24), and his observation was cited as the first on record. I have recently had my attention called to the fact that the phenomena were earlier observed by Prof. W. P. Blake, who published descriptions in this Journal and elsewhere in 1855, prior to the organization of Lieut. Ives's Expedition. (Pacific Railroad Rept., v, 108, 230 and 232; Am. Jour. Sci., 2d ser., xx, 180; Proc. Amer. Assoc. Adv. Sci., 1855, p. 218.) His description, which is more extended than Dr. Newberry's, touches the majority of the features to which I called the attention of the Association at Hartford; and to him belongs the credit of having first discovered and truly explained the facts.

9. *Zircon-syenyte of the Canaries.*—M. Meunier mentions in the Comptes Rendus (lxxix, p. 594, Sept., 1874) the occurrence of zircon-syenyte in the Canary Islands, the rock being identical with that long known from Scandinavia.

10. *Text-book of Geology, designed for Schools and Academies;* by JAMES D. DANA. 2d edition, 358 pp. 8vo. Illustrated by 400 woodcuts. New York and Chicago, 1874. (Ivison, Blakeman, Taylor & Co.)—This Geological Text-book of Professor Dana has been thoroughly revised, and thereby adapted to the arrangement of the new edition of the Manual and to the present state of geological science. The work is somewhat enlarged by the addition of new facts and illustrations, but without adding to the number of pages, the page having been increased in size to balance the additions, and also—as is true equally of the much-enlarged Manual—without an increase of price. The book is well printed on good paper.

11. *New American Geological Reports.*—The following Reports have been received, and will be noticed in another number of this Journal.

The Geology of New Hampshire, C. H. HITCHCOCK, State Geologist, and J. H. HUNTINGTON, Principal Assistant. Part I, Physical Geography. 668 pp. royal 8vo, with many plates and wood-cuts.

Contributions to the Fossil Flora of the Western Territories: Part I, *The Cretaceous Flora;* by LEO LESQUEREUX. 136 pp. 4to, with thirty lithographic plates, constituting volume VI of the quarto Reports of the United States Geological Survey of the Territories, F. V. Hayden, U. S. Geologist in charge. Department of the Interior.

First Annual Report of the Geological and Agricultural Survey of Texas; by S. B. BUCKLEY, A.M., Ph.D., State Geologist. 142 pp. 8vo. Houston, Texas, 1874.

Geological Survey of California, J. D. WHITNEY, State Geologist. *Contributions to Barometric Hypsometry,* with tables for use in California. 88 pp. roy. 8vo. 1874. Also map of California and Nevada. 1874.

The Surface Geology of Ohio: from vol. ii, of the Reports of the Geological Survey of Ohio; by J. S. NEWBERRY.

12. *Memoirs of the K. K. geologische Reichsanstalt.*—The following very fully illustrated memoirs in 4to, have been recently published by the K. K. geologische Reichsanstalt, Vienna.

Die Fauna der Schichten mit *Aspidoceras acanthicum;* by Dr. M. Neumayr. 1873.

Ueber einen neuen fossilen Saurier aus Lesina; by Dr. A. Kornhuber. 1873. The species is named the *Hydrosaurus Lesinensis.*

Ueber die triadischen Pelecypoden-Gattungen *Daonella & Halobia;* by Dr. E. M. v. Mojsvár. 1874.

Ueber die palæozoischen Gebilde Podoliens und deren Versteinerungen; by Dr. A. v. Alth. 1874.

Die Cephalopodenfauna der Gosauschichten in den nordöstlichen Alpen; by A. Redtenbacher. 1873.

13. *Manual of Determinative Mineralogy, with an Introduction on Blow-pipe Analysis;* by GEORGE J. BRUSH, Professor of Mineralogy in the Sheffield Scientific School. 8vo. New York, (John Wiley & Son.)—Professor Brush calls his work a compilation based on the tenth edition of Von Kobell's Tables. But, while this is true of it, he has contributed largely to the work from his own long labors in blowpipe mineralogy. Moreover, the species, with their blowpipe characters, and also, in additional columns, the several physical characters, have been arranged in a series of extended tables, which will be found exceedingly convenient by the student and sure of leading him, if he faithfully uses it, to the name of any mineral in hand. All species are included in it, even those recently announced.

This volume, as the Preface states, constitutes the Determinative Part of Dana's System of Mineralogy.

14. *Pharmacographia: a History of the Principal Drugs of Vegetable origin met with in Great Britain and British India;* by F. A. FLUCKIGER, Ph.D., Professor in the University of Strassburg, and DANIEL HANBURY, F.R.S., &c. London: Macmillan & Co., 1874. pp. 704, 8vo.—A standard work like this has long been wanted; and to Mr. Hanbury, botanists and druggists have been looking for it. The part taken by his German colleague is not specified; but the result is in every way satisfactory. It is not a treatise on Materia Medica, at least in the modern sense; the therapeutical applications of the drugs are indicated only in a general way; still less is it concerned with pharmaceutical details and manipulations. Nor is it a Medical Botany, although accurate botanical knowledge is turned to the best account; and the arrangement follows the natural orders of the plants yielding the drugs. It is the drugs themselves that are identified and described, rather than the plants that produce them. Their botanical origin is briefly discussed; their history is next given, generally with fulness and freshness, condensing the results of much research and literary labor; chemical composition is noted, but with no superfluity of symbolic formulas; under "Production and Commerce" much statistical and trade information is given; medical and economical uses are indicated; the nature of the principal adulterations noted, and the means of detecting them pointed out; and, finally, occasional substitutes, not of the nature of adulteration, are specified. In describing a root, bark, or the like, the microsopic

structure plays an important part, and is duly attended to. It has well been said that this can be made clear and explicit only by means of figures; but figures in sufficient number to be of much account would greatly increase the size and enhance the price of the work. A hand-book like this must do without them. A. G.

15. *Hymenomycetes Europœi, sive Epicriseos Systematis Mycologici editio altera;* scripsit ELIAS FRIES (Sumptibus auctoris). Upsaliæ, 1874.—The preface to this classical volume (of 755 8vo pages) is dated August 15, 1874, upon the venerable author's eighty-first birthday. He began to publish upon these *Fungi* sixty years ago, has all along been the acknowledged head and master of this recondite department, as well as one of the most learned and critical of Phanerogamous botanists; and his wonderful powers and sure judgment appear to be almost unabated. The full index fills nearly 50 pages. A. G.

16. *Miocene Fossil Plants of Greece.*—In the *Revue Bibliographique* of the Bull. Bot. Soc. France, 1874, p. 114, an abstract is given of a critical examination of Fossil Plants of Koumi, in Eubœa, by Count Saporta, a paper contributed to the *Ann. Sci. de l'Ecole Normale Supérieure*, ser. 2, t. 2. The main interest lies in the discovery of a Miocene flora, rich in peculiar species, and allied on one hand to the present Mediterranean vegetation, on the other to that of South Africa. It has *Widdingtonia*, *Podocarpus*, *Cussonia*, *Myricœ*, and *Anacardiaceœ*, &c., of the Cape types, and an *Encephalartos*, the first fossil *Cycadea* clearly made out as of an existing genus. It contains, moreover, *Glyptostrobus Europœus* of Heer, which passes insensibly into the existing representative, *G. heterophyllus* of China; and a *Sequoia* (*Fournalii*), which so closely approaches *S. sempervirens* of California, "que l'on ne saurait marquer aucune divergence sensible entre ces deux espèces." A. G.

17. *Mace.*—The mace of nutmeg, once taken as the type of an *arillus*, was, a good while ago, distinguished as a *arillode*, or false aril, by Planchon, on finding that it developed from the micropyle, while a true aril is a growth from the hilum or summit of the funiculus. Hooker and Thomson's statement, that the mace develops from both the micropyle and the hilum, has been confirmed by Baillon (*Comptes Rendus*, 78, p. 779, abstracted in *Rev. Bibliogr.* of Bull. Bot. Soc. France, l. c.). The consequence is that the distinctions between *arillus* and *arillode*, *caruncle* and *strophiole* become not exactly superfluous, but systematically unimportant. A. G.

18. *Organogeny of the Andrœcium and its bearings upon natural affinities*, by M. CHATIN.—A long and important series of papers in the Comptes Rendus, t. 78, 1874, of which a full and clear abstract is given in the Revue Bibliographique, above cited, occupying six pages. A. G.

19. JOHN TRAHERNE MOGGRIDGE's name was misprinted *Maggridge* in the January number, p. 69. He was, as we learn, a grandson of *Dillwyn* of Swansea; and so his taste for natural history came by inheritance. A. G.

20. *On the effects of certain Poisons on Mollusks;* by William North Rice, of Middletown, Conn. (From the Proceedings of the American Association for the Advancement of Science, Portland Meeting, August, 1873.)—Professor Rice closes his paper on this subject with the following paragraph:

Among the most interesting results of the experiments was the observation that certain poisons which act with extreme violence upon the mammalia, are very feeble in their action on the mollusca. This is especially true of hydrocyanic acid and woorara. Specimens of *Ilyanassa obsoleta*, immersed in dilute hydrocyanic acid on Friday, showed somewhat feeble signs of life on the following Tuesday. A specimen of *Lunatia heros*, into which a quantity of woorara had been injected, was found the next day to show no sign of any injury. Indeed, both of these poisons seemed to produce death very little sooner than the animals would have died in stale water. The sudden introduction of a large amount of carbonic acid in the manner which has been described, seemed to produce no decided effect. On the other hand, chloral hydrate seems to be very suddenly fatal, the animals treated with it becoming instantly contracted, and not resuming their activity when kept for a number of hours in sea water. Cyanide of potassium is similar in its effects, though not quite so instantaneously fatal. The effects of quinine are similar, though less energetic. Chloroform produces instantaneous contraction, and probably death; but, as the animals treated with this poison were not afterward kept for a time in pure sea water to give them an opportunity to revive, it is not certain that they were really dead.

21. *On the mode in which Amœba swallows its Food.*—Professor Leidy remarked that he had supposed that Amœba swallows food by this becoming adherent to the body, and then enveloped, much as insects become caught and involved in syrup or other viscid substances. He had repeatedly observed a large Amœba, which he supposes to be *A. princeps*, creep into the interstices of a mass of mud and appear on the other side without a particle adherent. On one occasion he had accidentally noticed an Amœba, with an active flagellate infusorium, a Urocentrum, included between two of its finger-like pseudopods. It so happened that the ends of these were in contact with a confervous filament, and the glasses above and below, between which the Amœba was examined, effectually prevented the Urocentrum from escaping. The condition of imprisonment of the latter was so peculiar that he was led to watch it. The ends of the two pseudopods of the Amœba gradually approached, came into contact, and then actually became fused—a thing which he had never before observed with the pseudopods of an Amœba. The Urocentrum continued to move actively back and forth, endeavoring to escape. At the next moment a delicate film of the ectosarc proceeded from the body of the Amœba, above and below, and gradually extended outwardly so as to convert the circle of the pseudopods into a complete sac, inclosing the Urocentrum. Another of these crea-

tures was noticed within the Amœba, which appeared to have been inclosed in the same manner.

This observation would make it appear that the food of the Amœba ordinarily does not simply adhere to the body, and then sink into its substance, but rather, after becoming adherent or covered by the pseudopods or body, is then inclosed by the active extension of a film of ectosarc around it.—*Proc. Acad. Nat. Sci. Philad.*, p. 143.

22. *On the Motive Power of Diatoms.*—Professor Leidy made some remarks on the moving power of Diatoms, Desmids, and other Algæ. While the cause of motion remains unknown, some of the uses are obvious. The power is considerable, and enables these minute organisms, when mingled with mud, readily to extricate themselves and rise to the surface, where they may receive the influence of light and air. In examining the surface-mud of a shallow rain-water pool, in a recent excavation in brick clay, he found little else but an abundance of minute diatoms. He was not sufficiently familiar with the diatoms to name the species, but it resembled *Navicula radiosa.* The little diatoms were very active, gliding hither and thither, and knocking the quartz sand grains about. Noticing the latter, he made some comparative measurements, and found that the Naviculæ would move grains of sand as much as twenty-five times their own superficial area, and probably fifty times their own bulk and weight, or perhaps more.—*Ibid*, p. 113.

23. *The Common Frog;* by St. George Mivart. Nature series. Macmillan & Co., 1874.—This little treatise contains not only a large amount of information concerning the comparative anatomy of frogs, but also numerous illustrations of the anatomy of other animals, and of man, introduced for the sake of comparison with the corresponding parts of the frog. It is illustrated by eighty-eight cuts, most of them excellent. V.

24. *Animal Mechanism: a Treatise on Terrestrial and Aerial Locomotion;* by E. J. Marey. With one hundred and seventeen figures. International Scientific series. D. Appleton & Co., New York, 1874.—In this work the Subject of animal locomotion is treated in a very clear, analytical and concise manner, while the numerous illustrations render even the most complicated mechanisms easily understood. The first part of the book treats of the various forces and organs concerned in locomotion; the second treats of terrestrial locomotion; the third, of the flight of insects and birds. The author presents in this work the results of his own extensive investigations of this subject, and gives full accounts of the numerous mechanical contrivances by which the wings, legs, and bodies were made to record their own motions. Many of the tracings are reproduced in the cuts. Several machines, so constructed as to imitate faithfully the essential motions and effects of the wings of insects, are described and figured. The entire subject is so well explained and illustrated that the book cannot fail to be interesting and instructive, even to non-scientific readers. V.

III. ASTRONOMY.

1. *The Transit of Venus, Dec.* 8, 1874.—The following notices present a summary of the stations at which successful observations of the Transit were made so far as information has yet been received. The stations are arranged in the order of latitude:

In the Northern Hemisphere.

1. *Tschita.* Lat. 52° 0′ N., long. 7h. 34m. E. Russian station. Contacts observed and four series of measures with heliometer.
2. *Nertchinsk.* Lat. 51° 18′ N., long. 7h. 58m. E. Russian station. Three contacts observed, and two diameters and twenty distances of the planet measured with heliometer.
3. *Kiachta.* Lat. 50° 20′, long. 7h. 6m. E. Russian station. Eight photographs taken.
4. *Habarovka.* Lat. 48° 16′, long. 8h. 58m. E. Russian station. First two contacts and some chords observed.
5. *Jassy.* Lat. 47° 3′, long. 1h. 50m. E. German station. Last external contact observed.
6. *Wladiwostok.* Lat. 43° 7′, long. 8h. 47m. E. American and Russian station. First and second contacts observed, 13 photographs taken, and numerous distances of the two limbs were measured.
7. *Port Possiet.* Lat. 42° 42′, long. 8h. 43m. E. Russian station. Two interior contacts observed and 38 photographs taken.
8. *Orianda.* (North of the Crimea?) Russian station. Satisfactory observation of last two contacts.
9. *Pekin.* Lat. 39° 54′, long. 7h. 46m. E. American and French station. First and second contacts observed and photographs taken.
10. *Tschifu.* Lat. 37° 30′, long. 8h. 5m. E. German station. The observation of contact, the heliometer measurement, and the photographs succeeded splendidly.
11. *Teheran.* Lat. 35° 37′, long. 3h. 25m. E. Russian station. Observations successful.
12. *Yokohama.* Lat. 35° 36′, long. 9h. 19m. E. Russian station. Observation successful.
13. *Kobe.* Lat. 34° 40′, long. 9h. 1m. E. French station. Successful observations.
14. *Nagasaki.* Lat. 32° 45′, long. 8h. 39m. E. American and French station. Second contact observed well, first and third contacts observed through clouds. 60 good photographs. 150 micrometric measurement of cusps, separation of limbs and diameter of Venus.
15. *Ispahan.* Lat. 32° 40′, long. 3h. 27m. E. German station. 19 photographs taken.
16. *Cairo.* Lat. 30° 6′, long. 2h. 5m. E. English station. Last two contacts well observed.
17. *Suez.* Lat. 29° 58′, long. 2h. 10m. E. English station. Last two contacts observed satisfactorily.
18. *Roorkee.* Lat. —, long. —. East India Co. station. 100 photographs taken.
19. *Thebes.* Lat. 25° 43′, long. 2h. 10m. E. English station. Last two contacts well observed. 50 photographs taken. Russian station. Splendid weather; very important observations.
20. *Honolulu.* Lat. 21° 18′, long. 10h. 31m. W. English station. First two contacts well observed. Photographs unsatisfactory. The complete disc of Venus was seen twelve minutes before the internal contact. The first contact was observed at 3h. 7m. 1s.; the time computed in the Eng. Naut. Alm. was 3h. 5m. 18s; O—C= +1m. 43s. Second contact obs. 3h. 35m. 55·7s.; comp. 3h. 33. 0s; O—C= +2m. 55·7s.

In the Southern Hemisphere.

1. *Sydney.* Lat. 33° 31′ S., long. 10h. 5m. E. English Observatory. Observations satisfactory.
2. *Adelaide.* Lat. 34° 40′, long. 9h. 15m. E. English Observatory. Last two contacts well observed. The third contact was observed at 3h. 4m. 43·4s.; the time computed by the American Commission was 3h. 5m. 49s.; O—C = —1m. 6s. Fourth contact obs. 3h. 34m. 7·5s.; comp. 3h. 35m. 39s.; O—C=—1m. 32s.
3. *Melbourne.* Lat. 37° 49′, long. 9h. 40m. E. English Observatory. Observations successful.
4. *Queenstown*, New Zealand. Lat. ——, long. ——. American station. Observations successful. Ingress observed and 237 photographs taken.
5. *Hobart Town.* Lat. 43° 0′, long. 9h. 49m. E. American station. 113 photographs taken.
6. *Christ Church*, New Zealand. Lat. 43° 20′, long. 11h. 31m. E. English station. Failure from clouds.

2. *Aurora Australis at Melbourne, Victoria.*—Traces of the aurora were seen on the evenings of the 7th and 8th of March; there were a few faint pink streamers on the 7th at 7.30 P. M. It was also seen on the 8th at Cape Otway and Port Albert, and on the evening of the 10th at Mount Macedon.—*Monthly Record during March*, 1874, *under the Superintendence of R. L. J. Ellery, Government Astronomer, Melbourne.*

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Royal Society.*—The Copley medal of the Royal Society has been awarded to Prof. LOUIS PASTEUR, "for his researches on Fermentation and Pebrine;" and the Rumford medal to J. NORMAN LOCKYER, "for his spectroscopic researches on the Sun and on the Chemical elements."

2. *Proceedings of the Centennial of Chemistry.*—Numbers 2, 3 (for August and September) of volume v of the American Chemist are occupied with the proceedings of the Centennial meeting at Northumberland. They contain the address of Prof. H. H. Croft, on the life and labors of Priestley; of Dr. H. Coppée at the grave; of T. Sterry Hunt, on the century's progress in chemical theory; of J. Lawrence Smith, on the century's progress in industrial chemistry, and of B. Silliman, on American contributions to chemistry. To the last the author has added, with much labor, a list of papers published by American authors, bearing on chemistry, physics, and the chemical composition of minerals, and including also references to many papers on other topics.

Report of the Commissioners of Agriculture for 1873. 496 pp. 8vo. Washington, 1874.

Half-hour Recreations in Popular Science. No. 11. The Transmission of Sound by the Atmosphere, by John Tyndall. Gigantic Cuttle-fish, by W. S. Kent. 32 pp. 8vo. Boston. (Estes & Laureat.)

Half-hours with Insects. Part 5. Insects of the Pond and Stream, by A. S. Packard, Jr. pp. 129–160, 12mo. Boston. (Estes & Laureat.)

Tidal Researches, by William Ferrel, A.M., Assistant U. S. Coast Survey. Appendix to the U. S. Coast Survey Report for 1874. A profound work, the result of many years labor.

Topographical Survey of the Adirondack Wilderness of New York for the year 1873; by Verplanck Colvin. Transmitted to the New York Legislature, April 21, 1874. 306 pp. 8vo, with many valuable maps. Albany, 1874.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XVIII.—*On some Phenomena of Binocular Vision;* by JOSEPH LECONTE, Professor of Geol. and Nat. Hist., University of California.*

VII. *Position of the eyes in sleepiness.*

IT is usually taught, and I think universally believed by physiologists, that in sleep and also in extreme drowsiness, when control over the ocular muscles is lost, the optic axes turn upward and *inward*. Müller says:† "During sleep the eyes have a peculiar position. At that time, as well as in a *state of mere sleepiness*, both eyes are turned *inward* and upward." I find similar statements by other writers on this subject; but I cannot find any experiments or observations upon which this conclusion is based. I think it probable that it has been reached indirectly thus: It is known that in a state of sleepiness the images of objects double. This double vision may arise either from convergence on divergence of the optic axes. It is very naturally attributed to the former, because in sleep the iris is contracted, and contraction of the irides is usually consensual with convergence of the optic axes. Thus Müller says (p. 535): "The contracted state of the irides in sleep is *a consensual motion dependent on the position of the eyes*, which are turned upward and inward by the inferior oblique muscles." The experiments I am about to describe, however, prove conclusively that, if not in sleep, at least *in sleepiness* or the act of falling to sleep, *the optic axes diverge.*

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* For preceding articles on this subject, see this Jour., Ser. II, vol. xlvii, pp. 68 and 153; and Ser. III, vol. i, p. 33, vol. ii, p. 1, vol. ii, pp. 315 and 417.

† Müller's Physiology, Baly's Translation, Am. ed., p. 810.

AM. JOUR. SCI.—THIRD SERIES, VOL. IX, NO. 51.—MARCH, 1875.

There are few persons, I suppose, who have not experienced an unconquerable drowsiness while listening to a dull speaker on a warm summer afternoon. Every one at such times must have observed that as the control over the ocular muscles is lost, the head of the speaker, whom he is vainly attempting to regard with attention, becomes double; the two heads separating more and more, until, at the distance of 30 feet, they may seem 10 to 15 feet apart. In my own case the control over the eyes is lost even while the consciousness is perfect, and the mind in a condition to make a scientific experiment. Often, although, by an effort, the control over the eyes could be retained, I have chosen to abandon it, in order to make the experiments related below.

In accordance with the usual doctrine, I had long supposed that this doubling of images in sleepiness was due to optic convergence. On testing it, however, I found I was mistaken. I tested it as follows:

It is well known that the images of all objects nearer or farther off than the point of optic convergence (point of sight) are double. But there is this difference in the two cases: if the object be beyond the point of sight, i. e. if the optic convergence be *too great*, the images *are homonymous*; but if the object be nearer than the point of sight, i. e., if the optic convergence be *too small* (and *a fortiori*, *if there be optic divergence*) *the images are heteronymous.* Now I have found it perfectly easy, even in the state of drowsiness already mentioned, to perform an experiment to test the position of the optic axes. As soon as the head of the speaker doubles and the two images are well separated, I wink the *right* eye. Invariably the *left* image disappears. The images are therefore heteronymous, and *heteronymous images in this case prove optic divergence.* For even with the point of sight at infinite distance, i. e., the optic axes parallel, the doubling of an object at the distance of 30 or 40 feet would be almost imperceptible (the distance between the centers of the two images being only equal to the interocular distance, or 2½ inches), while in the experiments the images were widely separated, in some cases 10 to 15 feet, indicating therefore an optic divergence of 15° to 20°.

I am sure, in the course of 15 or 20 years I have performed this experiment many hundred times, and always with the same result. But thinking it possible that my own case might be exceptional, I asked Professor E. P. Alexander, one of my colleagues in the University of South Carolina, who had been troubled with similar drowsiness, to make the same experiments. He did so, and his results were identical with my own.

In every case, of course, the experiment rouses the mind and quickly re-unites the images, but not so quickly but that the result is perfectly obvious. But lest some may regard this speedy re-union of the images as an objection to the reliableness of this mode of experimenting, I devised another method, which is not open to this objection.

While gazing on vacancy, objects near at hand are very perceptibly double, the images being heteronymous. Now if while thus gazing, perhaps in profound thought, the observer should happen to be overcome with drowsiness and lose control over the eyes, *the already heteronymous images will separate more and more widely.* I have made this experiment many times while lying abed in the morning and always with the same result. But this result is impossible except by optic *divergence*, for by convergence the images would approach each other, unite and then cross over and become homonymous.

Thus, then, it is certain, that *in the act of falling to sleep the eyes diverge.* Whether this position is retained in profound sleep, I have tried in vain to determine. The attempt to make observations on the eyes of sleepers, for this purpose, invariably introduces disturbing influences which vitiate the result. I have also attempted observations on sick infants, who often, in a weak condition, drowse with the eyes half open; but I have always found, under these circumstances, the action of the ocular muscles irregular and the position of the optic axes, therefore, unsteady. Nevertheless it seems highly probable that in profound sleep, also, the position of the optic axes is divergent.

I will now bring forward several facts which point to the conclusion reached by these experiments, viz: that the *optic axes diverge when completely relaxed:*

1. Double vision is a well-known phenomenon of the state of drunkenness. Accompanying the double vision there is always observable an unnatural appearance of the eyes, resulting from a want of the perfect parallelism or the very slight convergence of the optic axes, which is a necessary condition of single vision. Double vision may result from too much convergence (squinting) or from divergence; but I think every one who has observed persons in this state will agree with me that the eyes do not squint but *diverge* as in sleepiness, and from the same cause, viz: a loss of control and consequent relaxation of the normal tonic contraction of the muscles of the eyes.

2. The axes of the conical eye-sockets, if produced, would meet each other about the occiput. This gives a divergence, in passing forward, of about 25°. It is probable that in a state of absolutely perfect relaxation, the optic axes coincide with the axes of the eye-sockets, and it requires, therefore, some contraction to bring the optic axes to a condition of parallelism and still more to a condition of convergence, as in every volun-

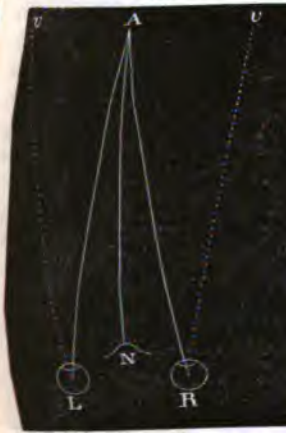
tary act of sight. In the human eye, therefore, and also in that of the highest animals, there are three conditions of the optic axes: 1st, convergence, when we look at a near object; 2d, parallelism, when we look at a distant object or gaze on vacancy; 3d, divergence, when we lose control of the eyes, as in sleep, in drunkenness and probably in death. The 1st requires a distinct voluntary effort—a distinct voluntary contraction of the ocular muscles; in the 2d there is no *voluntary* contraction, but only that tonic contraction characteristic of the waking state; in the 3d the relaxation is complete. The 1st is the *active* state of the eye; the 2d the *waking* passive state; the 3d the *absolutely* passive state.

3. In most normal eyes, in the waking passive state, the optic axes are perfectly parallel, and it is impossible to carry the relaxation so far as to produce divergence. Hence it is easy to double a near object either by looking at a *nearer* or at a *more distant* point—either by *too great* or too small convergence; but it is impossible to double an infinitely distant object, like a star, except in one way, viz: by convergence—by crossing the eyes. For the same reason it is impossible for most normal eyes, *without the use of instruments*, to combine stereoscopically two similar objects or two similar pictures, *beyond the plane of the object or the pictures*, unless the distance between identical points of the objects or pictures be, at most, not greater than the interocular distance. Yet I have known, at least, one man, a gentleman of rare intelligence and much interested in binocular experiments, and whose eyes were to all appearance perfectly normal, in whom the waking passive state, as in gazing on vacancy, was one of slight optic *divergence* —in whom therefore the relaxation was more complete than in most eyes. This gentleman could double a star by gazing vacantly, and as it were beyond it. I thought at first that the doubling was the result of optic convergence, but by placing a screen alternately before the one and the other eye and asking which image disappeared, I completely satisfied myself that his eyes, while gazing on vacancy, were really slightly divergent instead of parallel.

I proved this, however, still more satisfactorily in another way. I have stated that it is impossible for most persons of normal eyes to combine two similar pictures *with the naked eyes*, so as to form a stereoscopic image *beyond the plane of the pictures*, unless the distance between identical points in the pictures be, at most, not greater than the interocular distance. But this gentleman could thus combine ordinary stereoscopic pictures with the naked eyes beyond the plane of the pictures, even when the distance between identical points was greater than the distance between the centers of his pupils. His ocular divergence was very small, and therefore, as might have been expected, in proportion as the distance between identical points of the pictures was greater, the distance from the eyes at which the pictures must be placed must also be greater. For instance: when the distance between identical points was three inches the pictures were held at arm's length; when the distance was six inches the pictures were placed on the other side of the room. It would be curious to enquire, at what *distance* and of what *size*, according to the laws of vision, the stereoscopic image ought to seem in this case. For while one condition of single vision, the absolutely necessary one, viz: that the retinal images shall occupy corresponding points on the two retinæ, is satisfied; another condition. which if not absolutely necessary, is present in every act of single vision except this one—a condition which determines the apparent place and size of the object or stereoscopic image—viz: the *meeting of the two visual lines, is not satisfied.* In all cases of single vision, whether of natural objects or by stereoscopic combinations, the object or stereoscopic image is seen at the intersection of the visual lines, which is therefore called the *point of sight.* But in this case there is no point of sight at all—the visual lines do not meet at all except behind the head.

In one of my papers on binocular vision* I gave a new method of representing the position of double images; a method which, however, represents equally well the position of images seen single by stereoscopic combination. The facts which I have just presented may be perfectly represented by this method, but cannot by any other. Let R and L (fig. 1) represent the position of the eyes in sleepiness, and A the object

1.



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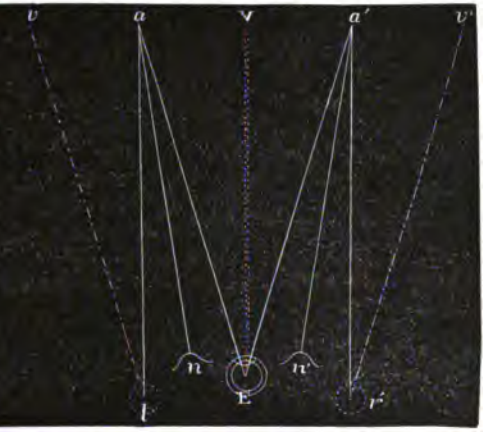

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* This Jour., Ser. III, vol. i, p. 33.

contemplated; then the lines A N, Lv Rv, will represent the position of the median line and the two visual lines and R A, L A of the ray-lines from the object A to the eyes. The visual result, if all these lines were visible, is represented by fig. 2. The object A, fig. 1, is seen double at a and a', fig. 2. If, instead of one object at A, there be two similar objects, e. g., stereoscopic pictures at v and v, fig. 1, then these will combine and be seen single at V somewhere along the combined visual lines E V, fig. 2. Of course there will be seen also, unless cut off by a septum, two other images, v and v', to the extreme right and left, as already explained in my previous paper. I cannot even conceive how these phenomena can be represented by the usual method.

VIII. *In binocular vision the law of corresponding points may be opposed to the law of direction. In such cases the law of corresponding points prevails.*

There are two well known fundamental laws of vision, viz: the *law of visible direction* and the *law of corresponding points.** The one is the fundamental law of *monocular*, as the other is of *binocular* vision. The one gives the true position of all objects and radiants, and therefore completely explains erect vision with *inverted* retinal image; the other combines the double external images of the same object at the point of sight, and therefore completely explains *single* vision with *double* retinal images. The former is usually regarded as the more fundamental—as underlying and explaining the other law, and as therefore itself capable of explaining all the phenomena of vision both monocular and binocular. The law of visible direction does indeed explain all the *ordinary* phenomena of single vision with two eyes; for in all ordinary cases of single vision—in all ordinary voluntary acts of binocular sight—in all cases of vision of objects lying in the horopter (whether this be a line or a surface), since ray lines from an object or radiant come to each eye from the same spot in space, by the law of direction the two images must be referred back along these ray lines to the same spot and therefore *superposed* and *seen single.* Thus single vision becomes in these cases a necessary result of the law of direction, and the law of corresponding points becomes only a particular case of the more general law of direction.

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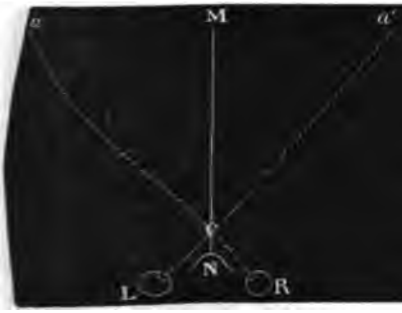
* The law of *visible direction* may be thus expressed: Every impression on the retina reaching it by a ray line passing through the optic center, is *referred back along the same ray line* to its true place in space. Thus for every *radiant* point in the object there is a corresponding *focal* point in the retinal image, and every focal point is referred back along its ray line to its corresponding radiant, and the *external* image (object) is thus re-constructed in its proper position. Or it may be otherwise expressed thus: Space in front of us is, under all circumstances, an *outward projection of retinal states.* With the eyes *open*, the field of *view* is the outward projection of the *active* state; with the eyes *shut*, the field of *darkness* is an outward projection of the *passive* state of the retina. Thus the internal retinal concave with all its states is projected outward and becomes the external spatial concave, and the two correspond point for point. Now the lines connecting the corresponding points internal and external intersect each other at the optic center, and impressions reach the retina and are referred back into space along these lines. This would give the true position of all objects and radiants, and therefore completely explains erect vision with inverted retinal image.

The law of corresponding points may be thus briefly expressed: Imagine the two eye-balls placed together in such wise that they geometrically coincide throughout, then the coincident points of the two retinæ are what are called corresponding points. Now the law of corresponding points declares that images or impressions of any kind falling upon corresponding points of the two retinæ are referred back to the same place in space and therefore seen single, while impressions falling on non-corresponding points are seen double. The determination of the horopter, as a geometric problem, is the determination of the surface or line, the ray lines from every point of which would impress corresponding points of the retinæ.

But in all cases of *double* images, the apparent position of these to the binocular observer is always different, and in some cases very different from the true position of the object which they represent. The difference may amount even to 45°. For example: The binocular field of view in my own case is about 100° in a horizontal direction. By strong convergence I can almost wholly obliterate this common field. In such case the images of objects lying near the extreme margins of the common field, and therefore at least 90° apart, are brought together and united in the middle, and seen apparently directly in *front;* while objects really in front are doubled and their images are separated 90° from each other, and each 45° from the true position of the object which they represent.

3.



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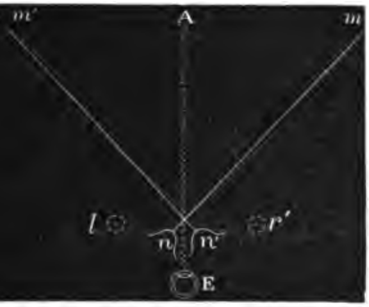

Fig. 3 represents the actual position of parts in this experiment, R and L being the right and left eye respectively, N the root of the nose, *a a'* objects in the visual line 45° to the right and left, M N the median line and M an object in front; and figure 4 is the visual result represented by my method. It will be seen by comparing the two figures that the two eyes R and L are combined and rectified to form the single binocular eye E, the visual lines R*a* and L*a'* become the common visual

line A E, the objects *a* and *a′* combine at A, while the median line and the object M are doubled and the images of the latter are seen at *m* and *m′*.*

It is true that in the case of the objects *a* and *a′* seen directly in front, it may be argued that this visual result is not in violation of the law of visible direction, but is in fact easily explained by that law. For if the combined images be referred to the point of ocular convergence (*c*, fig. 3) as in fact they often are, then each eye sees its own object in the true *direction* but only mistakes their *distance*. To this I answer, that *each eye* does indeed give the true direction, as is quickly shown by shutting one eye, but the *two eyes* do not. Each eye sees its own object in the true direction, but the *binocular observer* sees their combination in a wrong direction. The law of direction is true in all cases in *monocular* vision, but is not always true in *binocular* vision. In the case of the double images *m* and *m′* of the object M, it is still more difficult to explain their apparent position by the law of direction.

I suppose, however, that in all cases of ocular *convergence* it may be possible by an ingenious device to save the law of direction; but in the case of ocular *divergence* it is impossible by any device to explain the position of double images by that law. The position of images in this case is in direct violation of that law; the laws of direction and of corresponding points are in contradiction to each other and the *law of corresponding points prevails.* This we now proceed to show.

We have seen, p. 160 and figs. 1 and 2, that in drowsiness the optic axes may diverge 20°, and that in such cases the double images of an object in *front* are seen right and left 10° from the real position of the object which they represent; and on the other hand, that objects really in the direction of the visual lines, and therefore 20° apart, are combined and seen directly in front. Unfortunately, we cannot diverge the optic axes voluntarily and therefore cannot make experiments of this kind at will; but by pressing the eye-balls on their outer side the axes may be made to diverge considerably. By strong, almost painful pressure on the outer portion of both eye-balls, I have succeeded in making the double images of an object directly in front separate 50°, and those of objects 50° apart combine directly in front. There can be no doubt that if we could turn our eyes directly outward, or if our eyes, retaining their present organization, were transferred to the sides of our heads, with their axes directed straight right and left, and therefore making an angle of 180° with each other, *images of objects in the direction of these axes, and therefore directly right and left, would be moved round 90° each and combined and seen directly in front.* This seems an extraordinary result, but is a necessary consequence of the law of corresponding points. The retinal images of the two objects are on corresponding points; by the law of corresponding points, therefore, they must be seen as *one.* But where else can this take place but in front.

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* By comparing these figures with those of my previous paper, it will be seen that I have modified slightly the mode of representation by including the parts of the face itself in the rotation, as well as in the *shifting*, of the field of view of each eye. In *convergence* this brings the two images of the nose nearer together in front and thus narrows the common field, which is exactly what actually happens. In divergence, on the contrary, the double images of the nose separate and the common field is widened.

The foregoing results, as extraordinary as they may seem, are completely explained by the principles brought out in my previous papers, especially that "On the mode of representing the position of double images," and that "On so-called images of illusion."* Those who have read these papers will remember that I have there shown that there are two apparent movements of the field of view accomplished by the eyes in binocular vision. The first is a bodily *shifting* of the field of the right eye one-half interocular distance to the left, and of the field of the left eye the same distance to the right; so that the two eyes are brought together in the middle and combined to form a *single binocular eye,* and the two visual lines combined to form a common visual line. This movement is habitual and involuntary, but it produces little change in the apparent position of objects unless they be very near. The second is a *rotation* of the whole field of view of both eyes about the optic center, whenever the two eyes move in opposite directions, as in *convergence* or *divergence.* This rotation is always in a direction opposite to the motion of the eyes, and therefore homonymous in convergence and heteronymous in divergence. It is this movement which changes so greatly the apparent position of all objects in the field of view under the conditions mentioned, and it is this, therefore, which concerns us in the explanation of the phenomena related above. In monocular vision, as also in binocular, when the eyes move together right or left, up or down, external objects seem stationary, and the visual lines seem to move across them. But in binocular vision, when the two eyes move in opposite directions, the visual line seems to remain stationary (i. e., we seem to look in the same direction), while external *objects* or their images seem to move. In converging or diverging the optic axes, the external images belonging to each eye, like trooping shadows, seem to sweep in opposite directions, toward each other in convergence, away from each other in divergence. As the visual lines *actually* approach and sweep across objects successively, the images of these objects *seem* to advance and take their places successively in front of the observer. And this is true whatever the degree of convergence or divergence. In all cases, whatever be the position of the eyes, objects in the visual lines, and whose retinal images therefore fall on the central spots of the retinæ, are seen in front. If therefore the two eyes could be turned directly outward, or if the eyes were placed on the sides of the head so that the optic axes were directed straight right and left, objects directly right and left would be seen in front, and therefore 90° from their true position.

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* This Jour., III, vol. i, p. 33, and vol. ii, pp. 314 and 417.

IX. *Comparative Physiology of Binocular Vision.*

For many years past I have reflected much, and attempted to make some observations, on the phenomena of vision of lower animals, for the purpose of testing the existence or non-existence of binocular vision in them. I must frankly confess I have accomplished but little. The phenomena of binocular vision are so essentially subjective, that it seems almost impossible to reach any satisfactory results by observations or experiment. As the subject, however, as far as I know, has never been touched, I will bring forward my somewhat crude thoughts, in the hope that others may follow them up with more success.

In man the axes of the conical eye-sockets diverge about 25°, or each makes an angle with the median line of about 12°. In these slightly diverging conical sockets, the eye-balls are so placed, and the muscles so adjusted, that in the waking passive state their axes are parallel; and from this passive parallel condition they may easily be converged even upon very near objects. In man, then, though the eye-sockets still diverge considerably, the eyes are set in front with axes naturally parallel. This is evidently the position most suitable for binocular vision.

In monkeys the position of the eyes is much the same as in man. They are placed well in front, near together, their axes apparently parallel, and therefore well adapted to binocular convergence. But as we go down the vertebrate scale, the eyes are placed wider and wider apart; the axes of the eye-sockets diverge more and more, the difficulty of convergence upon a near point becomes greater and greater, until in some mammals, such as the Cetaceæ, in most birds and in all fishes, the eyes are placed no longer in front, but on the sides of the head, with their optic axes inclined nearly or quite 180° to each other. It seems quite evident that animals with eyes so placed cannot converge the optic axes on a single point, especially a near point. In fact, it is well known that most birds, when viewing an object very attentively, turn the head on one side and *look with one eye.* It seems impossible that the law of corresponding points can exist for these animals; for if it did, as we have already seen (p. 167), it would only lead to constant and fatal mistakes as to the position of external objects. In a word, it is impossible that these animals can enjoy a true binocular vision, with its double and its combined images, its stereoscopic effects, and the complex but accurate visual judgments founded on these effects. They see indeed with two eyes; but these do not act together as one instrument—as a single binocular eye; they are independent and see each for itself. This is the case with the other senses even in man. However much their organs may be multiplied, each organ perceives for itself. The property of corresponding points, from which arise all the phenomena of binocular vision, is something peculiar to the eye. Nothing analogous exists in the other senses. Binocular vision in its perfection, as it exists in man, is the last result of the gradual improvement of that most refined and wonderful instrument, the eye; specially adapting it to meet the wants of the higher faculties of the mind.

Below the vertebrates, of course, binocular vision does not and cannot exist.

There is another peculiarity of the human eye, probably closely connected with binocular vision, which still more quickly disappears as we go down the vertebrate scale. I refer to the existence of the so-called yellow spot or *central spot* of the retina.

It is well known that in the very center of the retina, just where the optic axis or continuation of the visual line pierces the retina, and therefore just where the images of objects in the visual line fall, there is a small depression about $\frac{1}{10}$ inch in diameter, bordered by a yellow margin. This depression is called the central spot. It is the most highly organized and most sensitive spot of the retina. It differs from other portions, 1st, in the fact that here the layer of rods and cones, the true receptive organ of the retina, is not covered by the fibrous layer, but directly exposed to the impression of light; and 2d, in the fact that here the cones are much smaller and more numerous than elsewhere.

Now it is a familiar fact that while gazing steadily at a certain point we see very clearly only a very small area about the point of sight. This small area corresponds, point for point, to the central spot of the retina. If now, while gazing steadily, we observe the relative distinctness of vision in other portions of the field of view, we shall find that it becomes less and less distinct in proportion as the point observed is more distant from the line of sight. In other words, there is a perfect graduation of distinctness from the point of sight, where it is greatest, to the margins of the visible field, where it is least. Now as the retina corresponds with the visible field point for point, it follows that there is a corresponding graduation, certainly in sensitiveness, probably in fineness of organization of the receptive layer of the retina, from the central spot even to the extreme anterior margin. Except in the central spot this increasing fineness of organization has not been demonstrated, but it doubtless exists, for the increasing distinctness of vision toward the point of sight is its effect and its representative in the field of view.

As we go down the vertebrate scale, the central spot is found only in some monkeys. After a total absence in other mammals and in all birds, it is said to reappear in some lizzards; but whether it there has the same structure, and therefore the same significance, as in the human eye is not certain. It seems fair to conclude, therefore, that the graduation of distinctness toward the point of sight and the limitation of the greatest distinctness to that point, do not exist in most of the lower animals.

The importance of the central spot in the highest animals, and especially in man, is very evident. The limitation of the greatest distinctness to the point of sight is absolutely necessary to the *concentration and limitation of the most thoughtful attention* to that point. If all portions of the retina were similarly organized, and therefore all points in the field of view equally distinct, it would be impossible to fix the attention steadily and thoughtfully on any one point to the exclusion of others. We might *see* equally well and over a wider field, but we could not *look* attentively. We could not *observe* thoughtfully. But in the lower animals, especially those which are preyed upon by others, it is far more important to see well in every direction than to fix the attention too exclusively on one point; the advantages of exquisite microscopic distinctness of the center of the field is sacrificed for the much greater advantages of moderate distinctness over a very wide field. The most important thing for them is a very wide field, and the equal distribution of attention over every part. Hence their eyes are prominent, set wide apart on the margins of the front or on the sides of the head, and destitute of central spot; so that they sweep the whole horizon and see all parts with nearly equal distinctness.

The connection of the central spot with binocular vision is also quite evident. These central spots are, more than all others, endowed with the properties of corresponding points, and the somewhat complex binocular judgments expressed by the words "stereoscopic perspective" are accurate and reliable only in the vicinity of the point of sight. This is the great difficulty in the way of experimental determination of the horopter, as already explained in one of my early papers.*

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* This Jour., II, vol. xlvii, p. 153.

The following, then, are the general changes in the vertebrate eye, as we go up the scale: 1. A gradual change of the *position* of the eyes from the *sides* to the *front* of the head, and a consequent change of the angle of inclination of the optic axes from 180° to parallelism. 2. A gradually increasing graduation of the fineness of organization, and therefore the sensitiveness of the retina, from the anterior margins toward the central parts, so as finally to form in monkeys and in man a central spot. 3. A gradually increasing power of converging the optic axes upon a single near point, so that the images of that point may fall upon the central spots of both eyes. 4. The gradual evolution of the properties of corresponding points, and therefore of all the distinctive phenomena of binocular vision.

These changes seem all intimately connected with each other and with the development of the higher faculties of the mind.

Oakland, Cal., Oct. 26, 1874.

ART. XIX.—*Jeffries Wyman. Address of Professor* A. GRAY at the Memorial Meeting of the Boston Society of Natural History, Oct. 7, 1874.*

[Concluded from page 93.]

IN attempting these analyses, I am drifting into a fault which Prof. Wyman never committed, that of being too long. So I must leave many of his papers unmentioned, and barely refer to two or three others which cannot be passed over. The most noteworthy of the shorter papers, however, are upon less technical or more generally interesting topics, so that we have need only to be reminded of them. Among them are his "Observations on the Development of the Surinam Toad," and the same of "*Anableps Gronovii*," and the paper "On some unusual Modes of Gestation." The importance of these papers lies, not in being accounts of some of the most striking curiosities of the animal world, but in the sagacity and quickness with which he discerned, and the clearness with which he taught, the lessons to be learned from them. Any good zoologist, with the same excellent opportunities, would have worked out all the details of the development of the Surinam toads in the skin of the back of their mother, and would equally have noted the morphological significance of the branchiæ and tail, that are never to know any thing of the element they are adapted for; but Dr. Wyman remarks upon the development of the limbs independently of the vertebral axis, as showing that, whatever view be taken of their homology, they are something superadded to it, and not evolved from it; he notes how the *whole* yelk-mass is moulded into a spiral intestine; and that the embryo at the end of incubation forms a larger and heavier mass than existed in the egg when it commenced, —showing that there was an absorption of material furnished by the dermal sac of the mother,—"a solitary instance among Batrachians, if not among Reptiles generally, in which the embryo is nourished at the expense of materials derived from the parent." From this he is led (in the later paper above mentioned) to infer the probability that the developed larvæ of *Hylodes lineatus*,—carried about inland upon the back of their mother, and destitute of limbs adapted to terrestrial locomotion,—may depend upon a secretion from the body for needful sustenance—an interesting and rudimentary foreshadowing of mammalian life, of which he discerned the bearings.

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* From the "Proceedings" of the Society.

His "Description of a Double Fœtus" (in the Boston Medical and Surgical Journal, March, 1866) gives him the opportunity of briefly recording some of the results of his studies of the development of double monsters, and to bring out his view, that "the force, whatever it be, which regulates the symmetrical distribution of matter in a normal or abnormal embryo, has its analogy, if anywhere, in those known as polar forces;" that "studying the subject in the most general manner, there are striking resemblances between the distribution of matter capable of assuming a polar condition and free to move around a magnet, and the distribution of matter around the nervous axis of an embryo." That this is not one of those vague conceptions by which many speculators set about to explain that of which they know little by means of that of which they know less, but that he had striking parallelisms to adduce, the close of this striking paper shows.

The subject of fore and hind symmetry, thus brought directly under notice, had been broached by Dr. Wyman several years before. He returned to it the year following, in his very important morphological paper, "On Symmetry and Homology in Limbs," read to this society in June, 1867, and published in the Proceedings of that date. It is interesting to observe with what caution and restraint he handled this doctrine of "reversed repetitions," which has since been freely developed by one of his pupils, who has a special predilection for speculative morphology, Prof. Burt Wilder.

Prof. Wyman's "Notes on the Cells of the Bee," in the Proceedings of the American Academy for January, 1866, is a characteristic specimen of his way of coming directly down to the facts, and making them tell their own story. I could not recapitulate his results much more briefly than he records them in his paper. I need not recall to you how neatly he made this investigation, and represented some of the results, filling the comb with plaster-of-paris and then cutting it across midway, so that the observations might be made and the cells measured just where they are most nearly perfect; and then printing impressions of the comb upon the wood-block, he reproduces on the pages of his article the exact outlines of the cells, with all their irregularities and imperfections. But I cannot refrain from citing a portion of his remarks at the close:—

"Here, as is so often the case elsewhere in nature, the type-form is an ideal one; and with this real forms seldom or never coincide. . . . . An assertion, like that of Lord Brougham, that there is in the cell of the bee 'perfect agreement' between theory and observation, in view of the analogies of nature is more likely to be wrong than right; and his assertion in the case before us is certainly wrong. Much error would have been avoided if those who have discussed the structure of the bee's cell had adopted the plan followed by Mr. Darwin, and studied the habits of the cell-making insects comparatively, beginning with the cells of the humble-bee, following with those of wasps and hornets, then with those of the Mexican bees (*Melipona*), and finally with those of the common hive-bee. In this way, while they would have found that there is a constant approach to the perfect form, they would at the same time have been prepared for the fact, that even in the cell of the hive-bee perfection is not reached. The isolated study of anything in natural history is a fruitful source of error."

Let me add to this important aphorism its fellow, which I have from him, but know not if he ever printed it. "*No single experiment in physiology is worth anything.*"

The spirit of these aphorisms directed all his work. It is well exemplified in his experimental researches—the last which I can here refer to,—upon "The formation of Infusoria in boiled solutions of organic matter, enclosed in hermetically sealed vessels and supplied with pure air," and its supplement, "Observations and Experiments on living organisms in heated water," published in the American Journal of Science and Arts, the first in the year 1862, the other in 1867. Milne-Edwards could not have known the man, when he questioned the accuracy of the first series because they do not agree with those of Pasteur, and thought the difference in the results depended upon a defective mode of conducting the experiments. As Dr. Wyman remarks in a note to the second series, "the recent experiments of Dr. Child of Oxford, and those reported in this communication, are sufficient answer to the criticisms of M. Edwards." Then as to his thoroughness: most persons would have rested on the results of his thirty-three well-devised experiments, proving "that the boiled solutions of organic matter made use of, exposed only to air which has passed through tubes heated to redness, became the seat of infusorial life;" but all would not have concluded that, after all, they "throw but little light on the immediate source from which the organisms have been derived," nor would many have closed an impartial summary of the opposing views in this judicial way:—

"If, on the one hand, it is urged that all organisms, in so far as the early history of them is known, are derived from ova, and therefore from analogy we must ascribe a similar origin to these minute beings the early history of which we do not know, it may be urged with equal force, on the other hand, that all ova and spores, in so far as we know anything about them, are destroyed by prolonged boiling; therefore from analogy we are equally bound to infer that Vibrios, Bacterians, etc., could not have been derived from ova, since these would all have been destroyed by the conditions to which they have been subjected. The argument from analogy is as strong in the one case as in the other."

Returning to the subject again a few years later, with a critical series of twenty experiments, each of three, five, ten, fifteen, or even twenty flasks, used by way of checks and comparisons, —a rigorous experimenter would have been satisfied when he had proved that sealed solutions, subjected to a heat of at least 212° for from *one* to *four* hours, became the seat of infusorial life, at least of such as Vibrios, Bacterians and Monads, while all infusoria having the faculty of locomotion were shown by a special series of experiments to lose this at a temperature of 120°, or at most 134° Fahr. But Prof. Wyman carried the boiling up to *five* hours, and in these flasks no infusoria of any kind appeared. The question of abiogenesis stands to-day very much where Prof. Wyman left it seven years ago.

I must omit all notice of the ethnological work which has occupied his later years, merely referring to the seven Annual Reports of the Trustees of the Peabody Museum of American Archæology and Ethnology, of which he was curator. The last of these, issued just before the writer's death, contains the principal results of his investigation of the human remains he collected in the shell-heaps of East Florida, and convincing evidence of the cannibalism of those who made them. A fuller memoir, embodying all his observations of the last six winters upon the Florida shell-mounds, was sent to the printer just before he died.

The thought that fills our minds upon a survey even so incomplete as this is: How much he did, how well he did it all, and how simply and quietly! We knew that our associate, though never hurried, was never idle, and that his great repose of manner covered a sustained energy; but I suspect that none of us, without searching out and collecting his published papers, had adequately estimated their number and their value. There is nothing forth-putting about them, nothing adventitious, never even a phrase to herald a matter which he deemed important.

His work as a teacher was of the same quality. He was one of the best lecturers I ever heard, although, and partly because, he was the most unpretending. You never thought of the speaker, nor of the gifts and acquisitions which such clear exposition were calling forth,—only of what he was simply telling and showing you. Then to those who, like his pupils and friends, were in personal contact with him, there was the added charm of a most serene and sweet temper. He was truthful and conscientious to the core. His perfect freedom, in lectures as well as in writing, and no less so in daily conversation, from all exaggeration, false perspective, and fictitious adornment, was the natural expression of his innate modesty and refined taste, and also of his reverence for the exact truth.

It has been a pleasure to learn, from former college students, who hardly ever saw him except in the lecture-room, that he gave to them much the same impression of his gifts and graces, and sterling worth, that he gave us who knew him intimately —so transparent was he, and natural.

With all his quick sense of justice, and no lack of occasion for controversy, it seemed to cost him no effort to avoid it altogether. He made no enemies, and was surrounded by troops of life-long friends. When he first went abroad, in 1841, he was told by some near friends, who recognized his promise, that a chair of Natural History in his alma mater would soon have to be filled, and that he should be presented as a candidate. In the winter following the present incumbent, responding to an invitation to visit Boston, which he had never seen, and to consider if he would be a candidate, then first heard of Wyman's name and of his friends' expectations or hopes; whereupon he dismissed the subject from his mind. Probably he felt more surprise than did Dr. Wyman, when notified, a few months afterward, of the choice of the Corporation. The exigencies of the Botanic Garden probably overbore other considerations. I doubt if Dr. Wyman ever had an envious feeling. Certain it is that no one welcomed the new professor with truer cordiality, or proved himself a more constant friend.

In these days it is sure to be asked how an anatomist, physiologist, and morphologist like Prof. Wyman regarded the most remarkable scientific movement of his time, the revival and apparent prevalence of doctrines of evolution. As might be expected, he was neither an advocate nor an opponent. He was not one of those persons who quickly make up their minds, and announce their opinions, with a confidence inversely proportionate to their knowledge. He could consider long, and hold his judgment in suspense. How well he could do this appears from an early, and so far as I know, his only published presentation of the topic, in a short review of Owen's "Monograph of the Aye-Aye" (in Am. Journ. Science, Sept., 1863)—the paper in which Prof. Owen's acceptance of evolution, but not of natural selection, was promulgated. Dr. Wyman compares Owen's view with that of Darwin (to whom he had already communicated interesting and novel illustrations of the play of natural selection); and he adds some acute remarks upon a rather earlier speculation of Mr. Agassiz, in which the latter suggests that the species of animals might have been created as eggs rather than as adults. He states the case between the two general views with perfect impartiality, and the bent of his own mind is barely discernible. In due time he satisfied himself as to which of them was the more probable, or, in any case, the more fertile hypothesis. As to this, I may venture to take the liberty to repeat the substance of a conversation which I had with him some time after the death of the lamented Agassiz, and not long before his own. I report the substance only, not the words.

Agassiz repeated to me, he said, a remark made to him by Humboldt, to the effect that Cuvier made a great mistake, and missed a great opportunity, when he took the side he did in the famous controversy with Geoffroy St. Hilaire; he should have accepted the doctrines of morphology, and brought his vast knowledge of comparative anatomy and zoology, and his unequalled powers, to their illustration. Had he done so, instead of gaining by his superior knowledge some temporary and doubtful victories in a lost cause, his preëminence for all our time would have been assured and complete. I thought, continued Wyman, that there was a parallel case before me,—that if Agassiz had brought his vast stores of knowledge in zoology, embryology, and palæontology, his genius for morphology, and all his quickness of apprehension and fertility in illustration, to the elucidation and support of the doctrine of the progressive development of species, science in our day would have gained much, some grave misunderstandings been earlier rectified, and the permanent fame of Agassiz been placed on a broader and higher basis even than it is now.

Upon one point Wyman was clear from the beginning. He did not wait until evolutionary doctrines were about to prevail, before he judged them to be essentially philosophical and healthful, "in accordance with the order of Nature, as commonly manifested in her works," and that they need not disturb the foundations of natural theology.

Perhaps none of us can be trusted to judge of such a question impartially, upon the bare merits of the case; but Wyman's judgment was as free from bias as that of any one I ever knew. Not at all, however, in this case from indifference or unconcern. He was not only, philosophically, a convinced theist, in all hours, and under all "variations of mood and tense," but personally a devout man, an habitual and reverent attendant upon Christian worship and ministrations.

Those of us who attended his funeral must have felt the appropriateness for the occasion of the words which were there read from the Psalmist:—

"The Heavens declare the glory of God, and the firmament showeth his handy-work. . . . . O Lord, how manifold are thy works! In wisdom hast thou made them all; the earth is full of thy riches; so is this great and wide sea, wherein are things creeping innumerable, both great and small beasts. Thou sendest forth thy spirit, they are created, and thou renewest the face of the earth."

These are the works which our associate loved to investigate, and this the spirit in which he contemplated them. Not less apposite were the Beatitudes that followed:—

Blessed are the meek; blessed are the peace-makers; blessed are the merciful; blessed are the pure in heart.

Those who knew him best, best knew how well he exemplified them.

ART. XX.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXXI.—*The Gigantic Cephalopods of the North Atlantic;* by A. E. VERRILL.

(Continued from page 130.)

ACCOUNTS of an attack made upon two men by another gigantic cephalopod, in Conception Bay, Oct. 27, 1873, have been published in this Journal,* and in many other magazines, as well as in the newspapers. In the encounter the monster lost two of his arms by amputation with a hatchet. A portion of one of these arms, measuring nineteen feet in length, was preserved by Rev. M. Harvey and Mr. Alexander Murray for the museum at St. John's, Newfoundland. It has been photographed, and cuts copied from the photograph have been published in some of the English magazines.* Before it was secured for preservation it had been considerably injured, many of the larger suckers having been torn off or mutilated. Owing to this fact they were originally described by Mr. Harvey as destitute of marginal denticulations, but he has recently re-examined the specimen, at my request, and now informs me that he is satisfied that they were all originally denticulated. Of this specimen I have seen only the photograph and some of the smaller suckers.

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* See vol. vii, p. 158, 1874; and American Naturalist, vol. viii, No. 2, p. 120, February, 1874, in a letter from Mr. Alexander Murray.

It is stated that six feet of this arm had been destroyed before it was preserved, and the captors estimated that they left from six to ten feet attached to the creature, which would make the total length between 31 and 35 feet. According to Mr. Murray, the portion preserved measured but 17 feet in length, when he examined it, Oct. 31, 1873, after it had been a few days in strong brine; the circumference of the slender portion was 3·5 to 4 inches; of the enlarged sucker-bearing part, 6 inches; length of the part bearing suckers, 30 inches; diameter of largest sucker, 1·25 inches. Calculating from the photograph, the portion bearing the larger suckers was about 18 inches in length, and about 2·4 inches broad, across the face; distance between attachments of large suckers, 1·68; outside diameter of larger suckers, 1·16 to 1·28; inside diameter, ·74 to 1 inch; diameter of small suckers of the outside rows, ·40 to ·48 of an inch. Mr. Harvey has recently sent to me a full series of measurements of this arm, as now preserved. It has contracted excessively in the alcohol, and is now only 13 feet and one inch in length (instead of 19 feet, its original length), the enlarged sucker-bearing portion being 27 inches; the large suckers occupy 12 inches; the terminal part bearing small suckers, 9 inches; circumference of slender portion 3·5 to 4·25 inches; of largest part 6 inches; breadth of face, among large suckers, 2·5 inches; from face to back, 1·62 inches; diameter of largest suckers outside, ·75 of an inch; inside, ·63 of an inch. It will be evident from these measurements, when compared with those made while fresh and from the photograph, that the shrinkage has been chiefly in length, the thickness remaining about the same, but the suckers are considerably smaller than the dimensions previously given. Comparing all these dimensions with those of the Logie Bay specimen, and calculating the proportions as nearly as possible, it follows that this specimen was very nearly one-third larger than the latter, but the large suckers appear to have been relatively smaller, for they were hardly one-twelfth larger than in the Logie Bay specimen. As the relative size of the large suckers is a good sexual character among squids, it is probable that this individual was a *female*. In form, proportions and structure, it agrees very closely with the specimen first described, and therefore I do not hesitate to refer it to the same species. The fishermen estimated the body of this individual to have been about 60 feet in length and 5 feet in diameter; but if the above proportions be correct, as I believe, then the body could not have been more than about 10 feet long, and 2·5 in diameter, and the long arms should have been about 32 feet in length. Allowing two feet for the head, the total length would, therefore, be about 44 feet.

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* See Annals and Magazine of Natural History, IV, xiii, p. 68; and "The Field," Dec. 13, 1873. The central line of this photograph is reduced four and a quarter times, while the front part is reduced about four times.

Another specimen (No. 3), probably of the same species, and similar in size to the last, was captured at Coombs' Cove, Newfoundland. The following account has been taken from a newspaper article of which I do not know the precise date, forwarded to me by Professor Baird, together with a letter, dated June 15, 1873, from T. R. Bennett, Esq., of English Harbor, N. F., who states that he wrote the article, and that the measurements were made by him, and are perfectly reliable.

"Three days ago, there was quite a large squid run almost ashore at Coombs' Cove, and some of the inhabitants secured it. The body measured 10 feet in length and was nearly as large round as a hogshead. One arm was about the size of a man's wrist, and measured 42 feet in length; the other arms were only 6 feet in length, but about 9 inches in diameter, very stout and strong. The skin and flesh were 2·25 inches thick, and reddish inside as well as out. The suction cups were all clustered together, near the extremity of the long arm, and each cup was surrounded by a serrated edge, almost like the teeth of a hand-saw. I presume it made use of this arm for a cable, and the cups for anchors, when it wanted to come to, as well as to secure its prey, for this individual, finding a heavy sea was driving it ashore, tail first, seized hold of a rock and moored itself quite safely until the men pulled it on shore."

It would appear from this description that one of the long arms had been lost before the capture. The large diameter of the short arms, compared with their length, and with the size of the long arms, is the only point in which this specimen apparently differed essentially from those described above. Possibly the *circumference* was intended,* which would make the proportions agree well with those of the other specimens.

In a letter from Mr. Harvey, dated Dec. 10, 1873, he says that the Speaker of the House of Assembly stated to him that he had measured a specimen cast ashore in Fortune Bay, which was between 42 and 43 feet in length, the body and head together being between 12 and 13 feet, and the two long arms each 30 feet. This we may designate as No. 6.

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* A similar mistake actually occurred in the description of the long arms, in the letter from Mr. Murray, published in the American Naturalist for February, 1873, p. 122, referred to above, but in that instance the error was very obvious.

Dr. Honeyman, geologist of Nova Scotia, has published, in a Halifax paper, a statement made to him by a gentleman who claims to have been present at the capture of another specimen (No. 7) in the Straits of Belle Isle, at West St. Modent, on the Labrador side. "It was lying peacefully in the water when it was provoked by the push of an oar. It looked fierce and ejected much water from its funnel; it did not seem to consider it necessary to discharge its sepia, as mollusca of this kind generally do, in order to cover their escape." * * * * "The length of its longest arm was 37 feet; the length of the body 15 feet; whole length 52 feet. The bill was very large. The suckers of its arms or feet, by which it lays hold, about 2 inches in diameter. The monster was cut up, salted, and barrelled for dog's meat." In this account the length given for the "body" evidently includes the head also. This creature was probably disabled, and perhaps nearly dead, when discovered at the surface, and this seems to have been the case with most of the specimens hitherto seen living. Animals of this sort probably never float or lie quietly at the surface when in good health.

Mr. Harvey also refers to a statement made to him by a clergyman, Rev. M. Gabriel, that two specimens (Nos. 8 and 9), measuring respectively 40 and 45 feet in total length, were cast ashore at Lamaline, on the southern coast of Newfoundland, in the winter of 1870–71. These may also have been of the same species as those described above, all of which I now refer to *Architeuthis monachus* of Steenstrup.

Mr. Harvey also mentions, in a recent letter, that a specimen was cast ashore at Bonavista Bay, December, 1872, and his informant says that the long arms measured 32 feet in length, and the short arms about ten feet in length, and were "thicker than a man's thigh." The body was not measured, but he thinks it was about fourteen feet long, and very stout, and that the largest suckers were 2·5 inches in diameter. The size of the suckers is probably exaggerated, and most likely the length of the body also. It is even possible that this was the same specimen from which the beak and suckers described in my last article, as No. 4, from Bonavista Bay, were derived, for the date of capture of that specimen is unknown to me. The latter, however, was much smaller than the above measurements, and it will, therefore, be desirable to give a special number (11) to the former.

Another specimen, which we may designate as No. 12, was cast ashore this winter, near Harbor Grace, but was destroyed before its value became known, and no measurements are given.

Architeuthis princeps Verrill. Plate v, figures 14, 15, 16.—This species is based on the lower jaw mentioned as No. 1 in my former papers, and on the upper and lower jaws designated as No. 10, in the first part of this article; besides these jaws we only have the rough measurements of the body of No. 1, and an estimate of the diameter of the sessile arms. The jaws of No. 10 were obtained from the stomach of a sperm whale taken in the N. Atlantic, and were presented to the Essex Institute by Capt. N. E. Atwood, of Provincetown, Mass., but the date and precise locality of the capture are unknown. The form of these jaws is well shown in figures 14 and 15. The total length of the upper jaw (fig. 14) is 5 inches; greatest breadth, 1·45; front to back, 3·5 inches; width of palatine lamina, 2·32. The frontal portion is considerably broken, but the dorsal portion appears to extend nearly to the posterior end, the length from the point of the beak to the posterior edge being 3·4 inches. The texture is firmer and the lamina are relatively thicker than in *A. monachus*. The rostrum and most of the frontal regions are black and polished, gradually becoming orange-brown and translucent toward the posterior border, and marked with faint striæ radiating from the tip of the beak, and by faint ridges or lines of growth parallel with the posterior margin; a slight but sharp ridge extends backward from the notch at the base of the cutting edge, and other less marked ones from the anterior border of the alæ. The tip of the beak is quite strongly curved forward, and acute, with a slight shallow groove, commencing just below the tip, on each side, and extending backward only a short distance and gradually fading out. The cutting edge is nearly smooth and well curved, the curvature being greatest toward the tip; at its base there is a broad angular notch, deepest externally. The inner face of the rostrum is convex in the middle and concave or excavated toward the margins, which are, therefore, rather sharp. The anterior borders of the alæ are convex, or rise into a broad, but low, lobe or tooth beyond the notch, but beyond this they are nearly straight, but with slight, irregular lobes, which do not correspond on the two sides. The anterior edges of the alæ make nearly a right angle with the cutting edges of the rostrum. The palatine lamina is broad, thin, and dark brown, becoming reddish brown and translucent posteriorly, with a thin, whitish border. The surface is marked with unequal divergent striæ and ridges, some of which, especially near the dorsal part, are quite prominent and irregular; the posterior border has a broad emargination in the middle, but the two sides do not exactly correspond. The lower jaw (plate v, fig. 15) was badly broken, and many of the pieces, especially of the alæ, are lost, but all that remain have been fitted together. The extreme length is 3·63 inches; the total breadth, and the distance from front to back, cannot be ascertained, owing to the absence of the more prominent parts of the alæ; from tip of beak to posterior dorsal border of mentum, 1·68; from tip of beak to posterior lateral border of alæ, 2·20; from tip of beak to posterior dorsal border of gular lamina, 2·37; from tip of beak to bottom of notch at its base, ·80; tip of beak to inner angle of gular lamina, 1·85; height of tooth from bottom of notch, ·25; breadth between teeth of opposite sides, ·60; from front to back of gular lamina, in middle, 1·75. The rostrum is black, with faint radiating striæ, and with slight undulations parallel with the posterior border; the beak is acute, slightly incurved, with a notch near the tip, from which a very evident groove runs back for a short distance, while a well marked, angular ridge starts from just below the notch, and descends in a curve to the ala, opposite the large tooth, defining a roughened or slightly corrugated and decidedly excavated area between it and the cutting edges; the cutting edge below this ridge is nearly straight, or slightly convex; the notch at its base is rounded and deep and strongly excavated at bottom; the tooth is broad, stout, obtusely rounded at summit, sloping abruptly on the side of the notch, and gradually to the alar edge. The anterior edge of the alæ, beyond the tooth, is rounded and strongly obliquely striated; it makes, with the cutting edge, an angle of about 110°. The inner surfaces of the two sides of the internal plate of the rostrum form an angle of about 45°.

The lower jaw of No. 1 (plate v, fig. 16) is represented only by its anterior part, the alæ and gular laminæ having been cut away by the person who removed it. It agrees very well in form and color with the corresponding parts of the one just described, but is somewhat smaller. The lateral ridges of the rostrum are rather more prominent, and the area within it is narrower and more deeply excavated, especially at the base of the notch, where the excavation goes considerably lower than the inner margin. The notch is narrower and not so much rounded at its bottom. The tooth is about the same in size as that of No. 10, and appears to be even more prominent, because the edge of the alæ is more concave at its outer base; it is also more compressed and less regularly rounded at summit. This jaw measures 1·30 inches from the tip to the posterior dorsal border of mentum; ·65 from tip to the bottom of the notch; ·16 from bottom of notch to tip of the tooth.

Both these lower jaws agree in having a very prominent tooth on the alar edge, with a large and deeply excavated notch between it and the cutting edge, and in this respect differ from the two lower jaws of *A. monachus* in my possession, for in the latter the tooth or lobe is low and broad, and scarcely prominent, while the notch is narrow and shallow. This seems to be the best character for distinguishing the jaws of the two species. But they also differ in the angle between the alar edge and the cutting edge of the rostrum, especially of the lower jaw, for while in *A. monachus* this is hardly more than a right angle, in *A. princeps* it is about 110°. Moreover, the darker color and firmer texture of the jaws of the latter seem to be characteristic.

The proportions of the body seem to be quite different, if we can judge by the measurements given of the specimen (No. 1) which was found dead and floating at the surface of the water, at the Banks of Newfoundland, by Capt. Campbell, of the schooner B. D. Haskins, from Gloucester, Mass., in October, 1871.* It is stated that this specimen was measured, and that the body was 15 feet long and 4 feet and 8 inches in circumference. The arms were badly mutilated, but the portions remaining were estimated to be 9 or 10 feet long and about 22 inches in circumference, two being shorter than the others. This would indicate a much more elongated form of body than that of *A. monachus.* If these proportions be correct, the body of No. 10 must have been about 19 feet in length and 5 feet 9 inches in circumference.

This specimen is probably the largest invertebrate hitherto actually examined by any naturalist.

Notes on specimens described by other writers.—We are mainly indebted to Professor Steenstrup and to Dr. Harting for our knowledge of the specimens preserved in European museums, or cast ashore on the European coasts. Professor Steenstrup has given interesting accounts, compiled from contemporary documents, of a specimen taken in 1546, and of two specimens of huge cephalopods cast ashore at Iceland in 1639 and 1790, and has also described and figured† the jaws of another specimen of *A. monachus*, obtained at Jutland in 1853. In the same memoir, of which I have seen only the first part, there are references to a description and figures of *A. Titan*, obtained in 1855, by Capt. Hygom, in N. lat. 31°, W. long. 76°. The latter specimen appears to be the same that Harting‡ mentioned, under the name of "*Architeuthis dux* Steenstrup," as collected at the same time and place, and of which he pub-

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[*] See the American Naturalist, vol. vii, p. 91, Feb., 1873.

[†] In a paper of which I have only seen some proof-sheets, given by him to Dr. Packard, entitled "Spolia Atlantica." Whether this memoir has been published I do not know. The plate (I) that I have seen is marked "Vid. Selsk. Skrifter, V. Række, naturv. og mathem. Afd. iv Bind;" and there are references to three other plates illustrating *A. Titan*, etc.

[‡] Description de quelques fragments de deux Céphalopodes gigantesques. Publiées par l'Académie Royale des Sciences à Amsterdam. 1860. 4to, with three plates.

lished an outline figure of the lower jaw, copied from a drawing furnished to him by Steenstrup. Harting states that the pen or "gladius" of this specimen is six feet long. Many important parts of this specimen were secured, and I regret that I have been unable to see the figures and description of it, referred to by Harting as forming part of Prof. Steenstrup's memoir, then unpublished. But to judge by the outline figure given by Harting, it is a species quite distinct from those described above. The lower jaw resembles that of *A. monachus* more than *A. princeps*, and is a little larger than that of our No. 5. The beak is more rounded dorsally, less acute, and scarcely incurved, the notch is narrow, and the alar tooth is not prominent.

Harting, in the important memoir referred to, describes specimens of two species, both of which are evidently quite distinct from all those enumerated above.

The first of these (his plate I) is represented by the jaws and buccal mass, with the lingual dentition, and some detached suckers, preserved in the museum of the University of Utrecht, but from an unknown locality. These parts are well figured and described, and were referred to *Architeuthis dux* by Harting. But the character of the dentition (plate IV, fig. 8) is so totally different from what I have found in *A. monachus* that it will be necessary to refer this species to a different genus, if not to a distinct family. The form of the lower jaw is quite unlike that of *A. dux*, for the beak is very acute, the cutting edge is concave, the notch shallow and broad, and the alar tooth is somewhat prominent. The size is about the same as our No. 5. The suckers figured are from the sessile arms, and agree pretty nearly with those of *A. monachus*. The edge is strengthened by an oblique, strongly denticulated ring. The internal diameter of the largest of these suckers is ·75 of an inch; the external, 1·05 inches. They were furnished with slender pedicels, attached obliquely on one side. The lingual teeth (see fig. 8, copied from Harting) are in seven regular rows, and resemble closely those of *Loligo* (fig. 7). In fact, I cannot find, in the figures and description, any character by which this species can be separated from *Loligo*, and at the same time it is evident that it is a species distinct from all others known. I would, therefore, propose to designate it by the name of *Loligo Hartingii*.

The other species described by Harting was from the Indian Ocean, and belongs to the genus *Enoploteuthis*.

Mr. Kent, in the article already referred to,* mentions a sessile arm of a giant cephalopod, which has been long preserved in the British Museum, but of which the origin is unknown. He states that it is 9 feet long; 11 inches in circumference at the base, tapering off to a fine point. There are from 145 to 150 suckers, in two alternating rows, those at the base being half an inch in diameter. The relatively small size of the suckers and great length of the arms show that this arm cannot belong to the same species as our *Architeuthis monachus*, which Mr. Kent thought probable. But as the arms of *A. princeps* and *Loligo Hartingii* are still unknown, it might belong to one of those species; or it may belong to the species observed, but not captured, by the officers of the "Alecton," in 1861, near Teneriffe, and named *Loligo Bouyeri* by Crosse and Fischer, but known only from the imperfect descriptions of it given by the officers, and a sketch of it prepared while the crew were making unsuccessful attempts to get it on board.

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* Proceedings Zoological Society of London for 1874, p. 178.

The body of this one was estimated at 15 to 18 feet in length, with the arms somewhat shorter.

EXPLANATION OF PLATE.

Plate v.—Figure 14. Upper jaw of *Architeuthis princeps* V. (No. 10); natural size.
Figure 15. Lower jaw of the same. The dotted line shows the parts that are present on the opposite side.
Figure 16. Part of lower jaw of *Architeuthis princeps* (No. 1); natural size.

ART. XXI.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXXII.—*The Trap Rocks of the Connecticut Valley;* by GEORGE W. HAWES.

IT has been already stated in this Journal* that Mr. E. S. Dana and myself have been engaged in the study of the eruptive rocks of the Connecticut Valley. Some of the results obtained by me through chemical analysis are here presented.

The dikes which intersect the Mesozoic sandstone and the adjacent strata are very numerous and oftentimes very different both as to the physical appearance and composition of the rocks. The analyses of some of these traps show them to be nearly anhydrous and of the same composition as the rock which is called dolerite. They have, moreover, a bright and apparently unaltered appearance, and under the microscope a clear crystalline character. Others are hydrous and also contain carbonic acid; their luster is dull and they are more or less green, and possess the composition and the chloritic character of the rock called diabase. Every grade of variation between the extreme cases can be found; but the attempt will be made to show, by the following analyses, the uniform character of the ejected material, and what were the changes which took place to alter it into its present varieties, and what were the causes of the same.

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* See Abstract of a paper on the Trap Rocks of the Connecticut Valley, by E. S. Dana, this Journal, III, viii, 390. (Read before the American Association at the Hartford meeting, Aug., 1874.)

The specimens of which analyses are here given have in general been taken from railroad cuts or working quarries, in order to avoid material which had been altered by surface action.

1. DOLERITE.

Lying to the west of New Haven there is a long ridge of trap, ending in a high bluff which is called West Rock. The trap is very firm, and analyses show it to be dolerite, the typical rock of this region.

WEST ROCK. Sp. gr. = 3·03.

	I.	II.	Mean.
Silica	51·80	51·76	51·78
Alumina	14·21	14·19	14·20
Ferrous oxide	8·26	8·23	8·25
Ferric oxide	3·55	3·62	3·59
Manganous oxide	·42	·45	·44
Lime	10·68	10·73	10·70
Magnesia	7·63	7·64	7·63
Soda	2·15	2·13	2·14
Potash	·39	·38	·39
Phosphoric acid	·14	·14	·14
Ignition	·63	·64	·63
	99·86	99·91	99·89

West Rock is one of the southernmost dikes of the old Connecticut Valley. The red sandstone region extends northward through Connecticut to the northern boundary of Massachusetts, and in the latter State are Mount Tom and Mount Holyoke. A fresh specimen from the latter mountain had the following composition:

MOUNT HOLYOKE. Sp. gr. = 2·97.

	I.	II.	Mean.
Silica	52·70	52·65	52·68
Alumina	14·11	14·17	14·14
Ferrous oxide	9·78	9·80	9·79
Ferric oxide	1·87	2·03	1·95
Manganous oxide	·45	·44	·44
Lime	9·36	9·39	9·38
Magnesia	6·42	6·35	6·38
Soda	2·54	2·57	2·56
Potash	·89	·87	·87
Ignition	1·61	1·58	1·60
	99·73	99·85	99·79

These analyses show it to be dolerite, differing from the trap of West Rock in the proportion of its ingredients.

A specimen from the Mesozoic sandstone region of New Jersey, taken from a deep railroad cut in Jersey City, afforded the following results:

JERSEY CITY. Sp. gr. = 2·96.

	I.	II.	Mean.
Silica	53·16	53·09	53·13
Alumina	13·87	13·62	13·74
Ferrous oxide	9·09	9·10	9·10
Ferric oxide	1·01	1·14	1·08
Manganous oxide	·44	·43	·43
Lime	9·44	9·50	9·47
Magnesia	8·56	8·59	8·58
Soda	2·28	2·32	2·30
Potash	1·03	1·04	1·03
Ignition	·89	·91	·90
	99·77	99·74	99·76

Another specimen taken from a trap hill lying to the eastward of New Haven, called East Rock, was also analyzed. But enough has been given to show the uniform character of the unaltered rocks of this era; no two analyses differing from one another more than would those of samples which could be selected from the same dike.

The principal mineral ingredients are pyroxene and labradorite, with a small amount of magnetite, and often, as Mr. Dana has detected by his microscopic examinations, a little chrysolite and apatite. The *pyroxene* can be recognized by its cleavage. Spots are sometimes found in the rock where the crystals of pyroxene have considerable size, and often the prismatic angle as well as the basal cleavage can be distinctly seen. From one piece it was possible to extract a sufficient number of crystalline fragments of apparent purity for an analysis.

PYROXENE FROM WEST ROCK.

Silica	50·71
Alumina	3·55
Ferrous oxide	15·30
Manganous oxide	·81
Lime	13·35
Magnesia	13·63
Ignition	1·17
Alkalies and loss (by difference)	1·48
	100·00

On comparing this analysis with those of the dolerites, it leaves little doubt but that the feldspar is labradorite, as the low percentage of silica and the presence of so much lime proves. The presence in one case of a feldspar with even a lower percentage of silica (see beyond) renders it improbable that the chief feldspar constituent has a higher percentage than labradorite. Moreover, in the analyses, making due allowance for the magnetite, the oxygen ratio of the bases and silica is even less than one to two, which would not be the case if the feldspar were oligoclase.

The *magnetite*, which is always present, is quite variable in amount. There is much more in the trap of West Rock than in any of the other specimens analyzed. This is evident from the amount of sesquioxide of iron present, which, if it be all contained in magnetite, would represent five per cent of that ingredient. In the Mt. Holyoke and New Jersey specimens there is less magnetite, as is shown by the lower percentage of sesquioxide of iron, the higher percentage of silica and the inferior specific gravities. In some cases the amount is very large. In a part of one of the dikes, that form the "Hanging Hills" of Meriden, the rock is quite black and the amount of iron has been the cause of rapid disintegration, so that now the fine earthy debris reaches to the top of the ridge. A determination of the iron in this rock gave

	I.	II.
FeO	8·52	8·55
Fe_2O_3	5·30	5·35
	13·82	13·90

A few feet distant, however, the rock becomes as firm and undecomposed as usual, showing that the gathering of the magnetite was of no great extent. Often octahedral crystals of magnetite can be seen in the rock with the unaided eye.

The analysis of the pyroxene shows it to be ordinary augite containing equal amounts of lime and magnesia; and as magnesia does not enter into the composition of the feldspar, the evident excess of magnesia above that which would form such a mixture of labradorite and pyroxene belongs to the chrysolite. The minute crystals of apatite detected by Mr. Dana account for the presence of phosphoric acid.

The feldspar *anorthite* is sometimes a constituent of these dolerites. Passing up the West Rock ridge, at about a mile from its southern face, a smaller dike is seen with an eastern and western strike, while West Rock runs nearly north and south. This dike upon one side of West Rock forms the dam of Wintergreen Lake, then cuts directly through the main dike, and projecting out several yards upon the other side of West Rock ridge, forms what is called the buttress. Its position, intersecting the main dike, shows it to be of later origin. The direction of the columns of this buttress are horizontal, while the columns of the main ridge are nearly vertical, which shows that the main ridge must have cooled sufficiently to determine the position of the columns of the buttress, since the columns are always at right angles to the cooling surfaces. This dike is characterized by large grains (¼ to ⅓ inch across) of a clear whitish cleavable mineral, which render it sparsely porphyritic. Even at the extremity, where the dike becomes very small, and where in consequence the rock is very fine in texture owing to the more rapid cooling, this porphyritic character is retained. A sufficient quantity of the pure material was extracted for an analysis; its composition was

ANORTHITE.

		O.
Silica	45·95	24·50
Alumina	34·70	16·31
Ferrous oxide	·64	
Lime	15·82	5·06
Magnesia	*trace*	
Potash	·45	
Soda	1·80	
Ignition	·96	
	100·32	

It is therefore a lime feldspar; and the oxygen ratio for RO, R_2O_3 and SiO_2 is 1 : 3·2 : 4·8, which shows the feldspar to be anorthite. Mr. Dana informs me that under the microscope it has a different appearance from the other feldspar, and although triclinic it shows less tendency toward twinning. An analysis of a sample of the trap of the dike in which the anorthite occurs shows it to be a dolerite of the same composition as the others; which is proof of the uniformity of the ejections at different times in this period.

DOLERITE FROM WINTERGREEN LAKE. Sp. gr. = 3·00.

	I.	II.	Mean.
Silica	52·38	52·45	52·42
Alumina	14·59	14·50	14·54
Ferrous oxide	9·89	9·79	9·84
Ferric oxide	1·27	1·22	1·25
Manganous oxide	·50	·53	·51
Lime	10·63	10·54	10·59
Magnesia	7·36	7·31	7·33
Soda	2·20	2·27	2·23
Potash	·51	·48	·49
Ignition	·55	·55	·55
	99·88	99·64	99·75

The rock is therefore a mixture of pyroxene and labradorite, from which the anorthite crystallized out on account of its different composition; for a very slight change in the composition of the whole mass would account for the formation of the small amount of anorthite which the rock contains, and the difference in its fusibility, that of labradorite being 3, and that of anorthite 5, would favor the separation of the anorthite.

2. DIABASE, OR CHLORITIC DOLERITE.

In the examination of the various dikes we find the rocks in all stages of alteration, from the almost anhydrous rocks, like the preceding, to those in which the original ingredients seem to have undergone an almost complete change. This difference we conclude to be connected with geographical location and not with geological age; and the following analysis, as well as the study of the positions of the different varieties, prove this.

The rocks which were selected for analysis were taken from the eastern side of the sandstone region and are typical specimens of very large and long dikes. The Lake Saltonstall ridge, which is hydrous through its whole extent, is cut through near its southern extremity by the Shore Line Railroad, and it was from this spot, at some distance from the surface and from any amygdaloidal cavities, that the sample was selected.

LAKE SALTONSTALL. Sp. gr. = 2·86.

	I.	II.	Mean.
Silica	49·27	49·29	49 28
Alumina	15·87	15·97	15·92
Ferrous oxide	10·17	10·23	10·20
Ferric oxide	1·93	1·88	1·91
Manganous oxide	·35	·40	·37
Lime	7·46	7·42	7·44
Magnesia	5·90	6·07	5·99
Soda	3·45	3·36	3·40
Potash	·74	·69	·72
Water	3·92	3·88	3·90
Carbonic acid	1·12	1·17	1·14
	100·18	100·36	100·27

A specimen from the southern dike of a high ridge called the Durham Mountains exhibits still greater alteration, for the amount of water shows that the larger part of the pyroxene has been changed to chlorite.

It will be seen that the alteration of these rocks has not been attended by further oxidation of the iron, and therefore it could not have been accomplished by any surface action, since the oxidation of protoxide of iron is one of the chief causes of surface alteration; while in this case one mineral containing protoxide has been changed into another protoxide mineral. It would, therefore, seem certain that the alteration took place at

SOUTH DURHAM MOUNTAIN. Sp. gr. = 2·83.

	I.	II.	Mean.
Silica	46·56	46·51	46·54
Alumina	14·75	15·05	14·90
Ferrous oxide	9·89	9·78	9·83
Ferric oxide	3·58	3·50	3·54
Manganous oxide	·33	·35	·34
Lime	7·99	7·90	7·94
Magnesia	4·83	4·89	4·86
Soda	2·47	2·38	2·43
Potash	·59	·60	·60
Water	4·50	4·54	4·52
Carbonic acid	4·38	4·32	4·35
	99·87	99·82	99·85

the time of ejection, as has been urged by Prof. Dana.* If the trap-rocks of this eastern region, when coming up melted through the red sandstone strata, encountered subterranean waters, and if it would be impossible for the vapors produced by the heat to be pressed back, owing to the hydrostatic pressure above, then these vapors, together with other vaporizable material, as carbonic acid, set free from its combinations in the strata by the heat, would pass into the mass of molten matter. In this way we have a sufficient explanation of the change that made this diabase out of the material that formed the dolerite.

The pyroxene was the mineral that was most attacked, the result of the alteration being chlorite. As chlorite is a magnesian mineral, lime was set free by this change. Part or all of this lime united with carbonic acid; for on touching any of these hydrated rocks with a drop of dilute acid, effervescence takes place. In many of the amygdaloidal traps this carbonate of lime fills the cavities. The pyroxene, however, does not furnish sufficient alumina for chlorite; this was furnished by the feldspar, which also participated in the change, and which, under the microscope, appears more or less dimmed by decomposition. As a result, silica was liberated, which also fills many of the amygdaloid cavities; and often both silica and carbonate of lime are found in the same cavity. In other cases it has united with the lime and alkalies to form zeolites, which are common in the cavities. The exact chemical reactions that took place will be considered at another time, when more of the products of decomposition have been analyzed. That there was such a passage of vapors through the molten mass is evident; for the rock of some dikes contains long pipe-stem-like cavities, which were made by the ascending vapors, and which are generally filled with calcite.

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* See this Jour., III, vi, 104. "J. D. Dana on Igneous Ejections and Volcanoes."

On excluding the water and carbonic acid from the two preceding analyses, (columns 3 and 4 below,) it will be seen how nearly the original unaltered material resembles that of the dolerites (columns 1 and 2) in composition. As usual, there is a small difference in the amount of magnetite and in the proportion between the pyroxene and feldspar.

	1. West Rock.	2. Mt. Holyoke.	3. L. Saltonstall.	4. Durham Mt.
Silica	52·11	53·54	51·90	51·06
Alumina	14·29	14·37	16·77	16·35
Ferrous oxide	8·30	9·95	10·74	10·79
Ferric oxide	3·61	1·98	2·00	3·88
Manganous oxide	·44	·45	·39	·39
Lime	10·77	9·53	7·83	8·72
Magnesia	7·67	6·49	6·31	5·33
Soda	2·15	2·60	3·59	2·66
Potash	·39	·88	·75	·66
	99·73	99·79	100·28	99·84

There is a remarkable uniformity at all points in the ejected material, which seems to prove that, whether now anhydrous or chloritic, it must have had a common source, and this a deep-seated one; for so great uniformity would be well nigh impossible if the source were nearer the surface among the metamorphic rocks of the crust, as has sometimes been supposed.

ART. XXII.—*On the Comparison of Certain Theories of Solar Structure with Observation;* by S. P. LANGLEY. With a plate.

IN memoirs already published,[*] allusion has been made to the interest which would attend studies of the almost unknown interior of the umbra of sun-spots, and of forms there which, owing to the relative darkness, are hitherto nearly undescribed, and reference has also been made to certain so-called "crystalline" shapes seen at times, and which are especially associated with large spots and periods of great disturbance.

Doubtless owing to the difficulty of seeing appearances so delicate, these "crystalline" types have never been minutely delineated, and it has been naturally assumed that their existence lent some confirmation to the views of those who regard the photosphere as the luminous covering of an incandescent fluid, and consider spots as deposits of cooling matter, more or less analogous to the scoria deposits of terrestrial volcanoes. M. E. Gautier has, in a very interesting communication to M. Faye,[†] referred to these forms, and to my description of them. They are indeed so remarkable, and at first sight so apparently confirmatory of the views alluded to, that only after long study I have been led to think them not so much assimilable to the products of cooling upon a liquid surface as to certain cloud forms of our own atmosphere.

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[*] American Journal of Science, February, 1874. Royal Astronomical Society's Notices, March, 1874.

[†] Comptes Rendus, May 18, 1874.

To furnish material for a public examination of these details, whose study is so eminently instructive, it is necessary, as has been already remarked, (since photography cannot yet seize them,) to make drawings in which the single aim of the designer is to set down with a minute fidelity specific forms; aiming, in short, much more to produce a piece of accurate topography than a picture; but while it is on studies made of this minute exactness that discussion will be most profitable, their reproduction for the press is a work of so much labor that this kind of illustration will probably remain unusual.

The steel engraving, plate VI, from studies made at the Allegheny Observatory chiefly with the full aperture (13 English inches) of its equatorial, has been prepared by the kind furtherance of Prof. George F. Barker, of Philadelphia, its execution being secured at the hands of an engraver who has done his work with peculiar fidelity and skill. I trust it will be accepted as a means of putting the reader in a certain sense in the place of the observer, and enabling him himself to directly compare theory with the facts of observation. This plate is made from sketches taken on the 23d, 24th and 25th of December, 1873, of the eastern extremity of the great spot then nearing the center of the sun, and about 12° south of the solar equator. It is called a "typical spot" because (since the details could not be completed at a sitting) it is less an accurate outline of what could be seen at any one moment, than an assemblage of the different types presented, in their proper connection. The whole, then, is taken from observation; but while the details of the adjacent photosphere have been supplemented from other studies, everything in the main body of the spot is the most literal transcript I could make of specific penumbral and umbral forms.

The sun had been hidden here for some days before the 23d of December, when the sky cleared, disclosing a spot of more than usual size. Although a daily record of the solar surface is maintained at the Allegheny Observatory, the weather for some weeks before had interrupted it so capriciously that I am unable to say with certainty what the age of the spot was when it suddenly presented itself, but unquestionably it had at this time already passed through the initiatory stage of its formation, and had entered upon that in which the forms seen earlier have commenced to become segmented or distorted, while still retaining characteristics which show the type from which they have sprung.

Attention was first directed to that dark interior, in which Dawes discerned still darker shades, which he called *nuclei*, as the unusual size of the spot and the irregularity of shade in the umbra seemed to favor their investigation. Aided by special optical devices, there became visible to close attention, forms which appeared to be affiliated to the better known ones of the penumbra, which were studied also, and a description of a part of whose characteristics, interesting perhaps in their bearing upon solar theories, follows.

It was observed: (1.) That the now well-known filaments of the penumbra, and those (still to be described) of the umbra, were all disposed in curves. These curves might be described in general as portions of rude spirals, since while there was such a variety as to make classification difficult, the spiral type was, as a whole, beyond any question the dominant one. This and the characteristic forms of the outer penumbral edge, elsewhere partly described, bear witness to the existence of a force, or perhaps I should say the component of a force, directed in a general sense to the center of the spot, while at the same time the absence of a common direction of rotation, and the existence even of distinctly marked opposite flexures in the same filaments, show the complexity of the action which had been at work.

(2.) An appearance which deserves remark is this. It has long since been observed that the interior border of the penumbra is commonly brighter than its exterior; but the hitherto unrecognised cause is here shown, in a general tendency of these singular objects, the filaments, to grow progressively brighter toward their extremities. It should be noticed that it is not only here meant that these grow brighter at the inner edge of the penumbra, but that the many filaments, not long enough to reach wholly across the penumbra, and whose ends in this case lie partly down its slope, in every case show the same tendency, so that it is difficult to resist the impression that these extremities have a general disposition to turn upward and to appear as though lifting their points above some obscuring medium.

(3.) In this connection we may best study the umbral forms previously referred to, about which so little has hitherto been known, owing to the darkness in which they are involved. That this darkness is only relative has been long surmised, and I have found by direct experiment that when all extraneous light is excluded, except that from the "blackest" part of the umbra, this proved to be not only intrinsically bright, but insupportably intense to the naked eye. By the optical device referred to, then, I have been able to look within the sun to some limited extent, or farther at least below the surface than is commonly seen. Thus armed, we find that the reddish-brown masses within the umbra are resolved into filaments, analogous to the penumbral ones; like them disposed in curves, and like them apparently in planes, whose direction is usually approximately horizontal. Here also we see that these umbral filaments grow brighter toward their extremities, which *appear* as if curling upward, their ends thus occasionally furnishing that appearance of isolated bright points in the umbra which has been already observed.

Leaving these for the moment, let us consider what was, on the whole, the most remarkable feature of the spot: a plume-like appearance in its lower portion,* which, in connection with adjacent peculiar curves, presented forms of what has been called the "crystalline" type. The impression that agencies like those which mould the delicate crystallizations of water have been here engaged, is a natural one, and has been expressed before, the term "photospheric crystal" having apparently been used by M. Chacornac as long ago as 1853. This part of the spot, if any, would seem to justify the remark of M. Gautier, that the modifications of certain spot forms recall rather the effects of mineral or saline deposits than that of the action of whirlwinds. They may certainly be said to remind us of such deposits, but is it by a true analogy or by a superficial resemblance? If we look closer; if we increase our telescopic power, we find that filaments which elsewhere possess a scarcely sensible magnitude become here of immeasurable fineness, and lie, not so much at the sharp angles of a crystalline deposit, (as they with lower powers seem to do,) as like finely carded wool. We may be in doubt whether to treat these "plumes" as part of the penumbral or umbral structures, their brightness seeming to affiliate them to the former, while, on minute examination, their fibers may yet be discerned prolonged through the umbral shade and rising again (apparently) above it, in the luminous points just referred to. The resemblance seen here, then, is rather to the filamentary types found in the chromosphere, and which no one questions are purely gaseous. There is frequently remarkable symmetry displayed in these forms, but very rarely as much as in the present example, which was the most regular I have ever seen. The balance of its parts around a central axis was almost as exact as in a sculptured ornament, and here again the regularity of certain crystalline forms is suggested. The "plume," however, whatever it may be, is evidently an integral structure, not due to the casual union of heterogeneous elements, and it was found on measurement to be approximately 20″ in length and 10″ to 12″ in width. Are we prepared to admit the existence of a body properly analogous to a "crystal," covering over ten times the area of Europe? Even on the sun, where everything is enormous, this taxes belief. But once more, the extremely attenuated filaments of the "plume" do not appear to be in any single plane. The great length of nearly 10,000 English miles, through which they apparently extend, is a curtate distance, or that in which we see them as projected on the apparent plane of the solar disc. If I do not misinterpret the indications given by the brightening ends, they can hardly be spread upon the surface of a liquid, or upon any single surface whatever,—they bend *down* and *up*. All through the umbra are to be noted similar appearances; we seem to look down through increasing depths, but as far as vision extends, without coming to any liquid or solid floor,—always down through volumes of whirling vapor, (whirling, if we judge from their forms, which are disposed as if by vortical action,) and growing fainter till lost to sight at an unknown depth below the surface. Speaking, then, without reference to any hypothesis, it seems to me that the resemblance to crystalline structure (though I agree that it is striking) does not appear to be more than superficial. We have at certain rare intervals remarkable cirrous clouds in our own atmosphere, whose resemblance to these forms is equally close, and in which, I think, we may see not only a resemblance but an analogy. Some of these rarer cirrous types of our own sky, which I have studied in connection with solar forms, might, so far as external appearance went, certainly be fancied to display crystalline action as clearly as any frost-figure on a window, yet we have no difficulty in seeing that in this case the eddies of our own atmosphere have been in some way a principal cause. While recognizing the danger of pushing too far, conclusions drawn from terrestrial analogy, I should then (pending a more complete study of these appearances), regard them as most nearly typified by certain cloud-forms of our own atmosphere.

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* Seen in the lower portion of the drawing.

This spot presented other interesting types not here referred to. But I may mention, in support of previous observations, that in the upper portion of the penumbra, just below where a considerable part of the photosphere was islanded, the sudden and abrupt change of direction of the filaments marked the apparently unmistakable passage of one cloud stratum over another. This disposition was also marked elsewhere on the spot, whole banks of clouds moving one over the other, so as to form a *terraced* appearance. In very many other spots, this movement of one stratum over and frequently at right angles to another has been recognized, so that after long study, I have felt justified in elsewhere formally announcing it as an ascertained fact, observed not in an isolated instance, but as a general characteristic of the solar surface, whose features are thus again assimilated to atmospheric ones in some degree like our own.

From the preceding facts of observation, it appears that the following conclusions may be drawn:

(1.) Without prejudice to the important considerations on which its distinguished author has framed the theory of the existence of a liquid or viscous solar shell, it seems to me, speaking simply as an observer, that, from what has been stated of the appearance of umbral forms, such a shell or crust must, if it exist, be at a distance, below the surface of the photosphere, considerable even with reference to the dimensions we here deal with.

(2.) It seems difficult to reconcile the bright, sharply-defined inner penumbral edge, and the regular structure discerned in the umbra, with another view, in which this umbra is a sort of stagnant pool, formed by cold vapors, or clouds, which have settled there after depressing the general surface by their weight till the penumbral slope is determined. So much of the early hypothesis of Herschel as regards the umbra as an opening (however made) *through* which we look into a non-luminous interior of the sun, extending everywhere beneath the photosphere, seems common to views which, differing widely elsewhere, agree on this point more nearly with these observations than with the views peculiar to Father Secchi.*

(3.) Finally, it seems to be little more than a summary of the facts of observation already rehearsed, to say that traces of a vortical action are found throughout the spot, and especially in the umbra. The theory which regards cyclonic or vortical action as a prominent agent in determining the forms we have studied, appears then to be in closer accordance with observation than the former.

As the substance of the present article was written before Father Secchi's remarks appeared in the August *Memorie*, it was originally prepared without reference to the questions raised there, and without any special reference to the cyclonic theory; and it is in no sense meant as a complete expression of opinion on those several points, in connection with which Father Secchi has done me the honor to cite my name. As one of the few who have used an instrument of adequate power in the particular field of research in which a large part of his labors have lain, I am more able than most, perhaps, to appreciate his eminent qualities as an observer. When, however, he states that longer study of the sun than I have yet given, will change my views upon the theory of M. Faye, I may remark that those views were not emitted so hastily, or on such light grounds, as to be readily altered; for they were the result of several years of observation, with an instrument of greater power than that Father Secchi employs. Doubtless, before adopting conclusions in any way differing from those reached by one whose ability as an observer deserves such respect, I was bound in every way to verify the grounds on which these conclusions rested. If what seem to me the facts of observation conducted, and still continue to conduct me, to views in many ways differing from those which he maintains, in opposition to distinguished Italian and French astronomers, I have less hesitation in trusting to observations which agree more nearly with theirs, from the belief that no personally cherished hypothesis has subjected me to that unconscious bias in the collection and interpretation of facts, against which common experience shows that even eminent ability is not a certain protection.

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* . . . "Il solo errore dell 'illustre osservatore (i. e., Herschel) fu di estendere la massa oscura su tutto il globo sotto la fotosfera, *mentre in realtà essa non forma che pezze o chiazze assai limitate.—P. Secchi, Memorie, Agosto*, 1874.

Allegheny Observatory, Allegheny, Pennsylvania, December 29, 1874.

ART. XXIII.—*Notes on Costa Rica Geology;* by W. M. GABB.

SINCE the beginning of 1873, I have been engaged in an exploration of the southeastern corner of the republic of Costa Rica, a region of about 3,000 square miles, known politically as the District of Talamanca. Previous to my going there, the country was probably the least known of any part of Central or Isthmian America. It comprises the Atlantic slope of nearly one half of the length of the republic, and is inhabited by tribes of Indians who, with good reason, have such a hatred to everything called Spanish, that the people of the country have never dared to penetrate it. Strange to say, the hatred is not against the white race; only against "Spaniards," i. e., persons of that race, or speaking that language. A few English-speaking traders have had dealings with these people for half a century, and have invariably treated them well; so that an Englishman or American can go among them with impunity; while one speaking only Spanish is looked upon with distrust, and either treated with insolence, or at best left severely alone.

I was at first engaged by a company of the leading persons in Costa Rica, natives and foreigners, but afterward the Government took charge and assumed the responsibility of the work. The prime object of the exploration was the re-discovery of some mines, reported by tradition to exist in the region, and of which the most fabulous stories were told. Suffice it here to say that such mines do not, and for sufficient geological reasons cannot, exist there. This, however, is not surprising to one who has been in California or in the West Indies, where such traditions exist everywhere. Having now completed the field-work of this little isolated region, I consider it advisable to put on permanent record a bare *resumé* of the leading facts and observations, leaving all theories and deductions for a future occasion. It is difficult, and perhaps unadvisable, to attempt to generalize where one's observations have been so entirely circumscribed as mine have been in this district. I have not been able to carry my observations to the Pacific; and the conglomerate rocks, of which I shall speak, point to older sedimentary formations which I have not seen, and which may have played an important part in the history of Isthmian America.

The central Cordillera of the lower part of Costa Rica is not much less than 6,000 feet high at its lowest point. Along this crest rise several prominent peaks, that of U-jum, at the head of the Coen River, and Mt. Lyon, at the head of the Lari, being probably over 8,000 feet each, while Pico Blanco, or Kamuk, the culminating point, is 9,652 feet, by careful barometrical measurement. Farther down the isthmus the reputed volcanoes of Chiriqui and Robalo are said to be of corresponding heights; but to the northwest a decided depression of the range occurs, before we reach the high region of Central Costa Rica. The direct distance from the summits of the range to the coast is hardly more than thirty miles; but the country is so cut up by sharp ridges and deep cañons, so covered with dense forests and impassable swamps, and so poorly supplied even with Indian trails, that the distance travelled is fully three times as great. In an open country, with good routes of travel, I could easily have accomplished in three or four months all the geological explorations that necessitated seventeen months of the hardest labor I ever did in my life. This is not the place to speak of toil and suffering, of exposure for weeks to continued rains, of fording swollen rivers at the risk of our lives, of fevers; in short, of all the pleasant episodes inevitably connected with work in primeval tropical forests. We have been through them all, and without the loss of a single life in my corps; albeit, several Indians paid that penalty, and, although I escaped with health uninjured, some of my assistants have been less fortunate in this respect.

The chain slopes rapidly to the northeast, and in a dozen miles from the summit is barely more than a thousand feet high. The next dozen miles is a still more gentle slope; and the coast is bordered by a flat region of swamp, broken by only a few low spurs.

Two large rivers drain the region. These are the Tiliri (sometimes called the Sicsola) and the Tilorio (likewise known at its mouth as the Changinola). These names in parenthesis were given by the Mosquito negroes to the mouths of the rivers, and, although they occur on the maps, are not the real names of the streams. The Tiliri is the more important of the two, draining by far the larger area, by means of five great branches, viz., the Tiliri proper, the Coen, the Lari, the Uren, and the Zhorquin. All of these branches, except the last, unite, in the space of a mile, in a broad valley of over 100 square miles of area. The Zhorquin is the smallest branch, does not descend from the main range, as the others do, and enters the Tiliri a few miles below the junction of the other branches.

The Tilorio drains the region east of Pico Blanco, by one great arm and its branches, and the country back of Pico Robalo, by another arm. This latter is entirely unknown, and is likely to remain so, until some traveller more adventurous than any yet found dares to penetrate the region. The Indians reported the country to be almost impassable, and say it is inhabited by a small band of Indians who refuse unconditionally to hold any communication with civilized people, or to permit them to enter their district. Several such refugee bands of savages are known to exist in this vicinity, and I have little doubt but that there is good foundation for the story.

The high mountains of Talamanca are in great part made up of a mass of granitic rocks, bearing in some respects a marked resemblance to those of the island of Santo Domingo. There is one important point of difference, however, between the manner of occurrence of this rock here and in Santo Domingo. In the latter country, granitic dikes often cut through or extend into the sedimentary rocks for great distances. They are of all sizes, from a thread to hundreds of yards in thickness, and often contain fragments of the jaspery slates entangled in them, and perfectly soldered by fusion. In Costa Rica I have never seen a granitic dike. The material seems to have been forced up from below in a plastic state, sufficiently heated to have changed the character of the overlying rock wherever it came in contact with it, but never sufficiently fluid to penetrate any possible fissures that may have existed. True granite rarely occurs, while syenites are much more common. The rock is almost always rather fine-grained, and while hornblende abounds, mica is rather the exception than the rule. The rock is lighter in color, and of a slightly coarser grain at the more eastern exposures than farther west. I saw nowhere the slightest approach to a gneissoid structure, or any other sign that would indicate a metamorphic origin for the mass, but several facts point to its having been quite recently in a heated condition. On the upper part of the Coen River, on one of its largest western branches, I found a single small boulder of mica slate, but I could not trace it to its source.

Lying on the flanks of the granite, in the region of the Tiliri, and extending to the coast, there is a deposit, only a few hundred feet thick, of Tertiary rocks, where, in contact with the granite, they are highly tilted, dipping seaward; a large fold or two follows this uplift in the higher mountains, and more gentle undulations extend to the coast. There is not a regular system of plications, however, since I have observed local variations of strike, even as great as at right angles, within a mile, accompanied by high dips. This is markedly the case on the Zhorquin, near its mouth, where, perhaps, the underlying granite is not far below the surface. Close to the granites the sedimentary rocks are highly metamorphosed, and in most cases their bedding is entirely destroyed; but I was fortunately able to collect fossils at a number of localities scattered not only over the greater part of Talamanca, but also farther north and northwest, in the adjoining parts of Costa Rica. These prove, beyond a reasonable doubt, that the rocks are an extension of the great Miocene deposit found not only on the isthmus proper, but over so many of the West Indian islands. I recognized at sight many familiar species, and have no doubt that others only await comparison to identify them with known species.

These Miocene rocks are made up principally of conglomerates and fine shales, with occasional beds of sandstone, and a very little limestone. The limestone occurs most abundantly in the central regions of Costa Rica, in the Candelaria Mountains, and on the Reventazon River near Sapote, and are almost entirely wanting in Talamanca. The conglomerates and shales are also found as far as the latter locality; while the last mentioned rock is the most important constituent of the Candelaria range. Wherever the shales are found unaltered, they carry small beds of very inferior coal. It is to this member that the coal mines of Chiriqui belong; and, half a dozen miles south of San José, the capital of Costa Rica, there is a small mine of coal now being worked experimentally, with results far from encouraging.

The conglomerates are made up entirely of pebbles of a metamorphosed claystone. I have studied them with great care at almost every locality where I found exposures, and have never met with a single boulder or pebble of crystalline material. Even where close to the granitic rocks, I failed completely to find the slightest trace of granite. Nor have I been able to trace to their source these claystone pebbles. The formation must have been extensive to have furnished such a bulk of coarse material, to say nothing of the shales probably derived from the same source. The absence of granite pebbles proves conclusively that this rock was not exposed at the time of the deposition of the Miocene. Another peculiarity of the conglomerate is that, while from the region of the Tiliri west and north, as far as the line of the railroad now being constructed, the pebbles are hard and well rounded by attrition, and cemented by a matrix of different character, on the Tilorio the rock is rather a mottled claystone than a true conglomerate. The pebbles seem to have been but little altered, to have been imbedded in a matrix formed from their own material, disintegrated, and the whole, after deposition, to have undergone metamorphism together for the first time.

The trend of the granitic intrusion is not coincident nor coextensive with the mountain range. It bears more to the east, Pico Blanco being its culminating point; beyond this to the east it extends almost to the Tilorio River, ending in a narrow tongue. I was unable to ascend the Tilorio, back to the granite belt; but although there are large streams penetrating westward from the Tilorio toward Pico Blanco, there are no granite boulders in that river on its head waters.

Both the granite and the Miocene rocks are cut by numerous porphyritic dikes of a pretty uniform character, but entirely different from the more modern volcanic rocks of Central Costa Rica. These seem to have penetrated the two groups equally, and vary from a few yards to a mile in length. One of these dikes forms the apex of Pico Blanco, and has been exposed by the decomposition and denudation of the surrounding softer granite. This is nearly 300 feet high, and forms the extreme tip of what is otherwise an ordinary granite mountain. Space does not permit me to enter into a detailed discussion of this question here. I was three hours on the summit of the mountain. It is a simple, straight ridge, of which the western end rises in a sharp cone, perhaps fifty feet higher than the rest. There is no sign whatever of a crater, and 300 feet below the summit there is no volcanic rock. It must, therefore, be struck from the list of volcanoes.

The two neighboring peaks, Mt. Lyon, between the Lari and its branch, the Dipari, and U-jum, at the head of the Coen, are said by the Indians to be volcanoes. But after my experience with Blanco, I feel great hesitation in accepting the statement, although it is fortified by many very plausible stories of fires and smoke seen on their summits. The latter, especially, with its sharp, regular cone, certainly looks volcanic, and I had hoped to reach it; but after my excursion to Pico Blanco, all hands of my party, white as well as Indians, decided that for the time, at least, it was impracticable. It would have cost a month of hard work, and I had not so much time at my disposal.

Before closing my account of the mountain region, there is one other item deserving mention. My instructions required me to look for mines, and I examined carefully for traces of the precious metals wherever I went. I found in a few places quartz veins; one of considerable size, the others practically valueless. I also found deposits of placer gold in a few localities. But the existence of gold here is rather of scientific than of economic interest. The gold occurs in the metamorphosed Miocene rocks, and always at a greater or less distance from the granites. The point at which I found gold nearest to the granite was at least a mile distant, although the rock was highly metamorphosed. The spot where I found the largest quantity of gold was miles away from the granitic intrusion.

There is little more to be said about the geology of Talamanca. The coast is low and swampy, and is bordered by a few patches of Post-pliocene rocks of the character which I have designated as Antillite in the West Indies. Near Moen, back of the border reef, there was a little bay, where a dark gray claystone was deposited, and this bed, nearly 100 feet thick in one place, abounds in beautiful fossils, all of recent species of shells and Radiates. Many of the Mollusca retain their colors quite bright. On the beach, at some points, are large deposits of iron sand of great purity, which might be utilized.

To my great regret, I have not been able to carry my section completely across Terraba to the Pacific. On the summit of Pico Blanco, after robbing Irazu of its boast that it was the only point whence both oceans can be seen at the same time, we could only look over Terraba and wish. The granite was pushed up after the deposition of the Miocene. This I have demonstrated by the absence of granitic pebbles in the conglomerate. It therefore seems probable to me, *à priori* (I admit a not perfectly safe argument), that the same formation extended across; and standing on the peak, and looking at the hills and plains on both sides, I could not but feel convinced that on the south base of the mountain I must find the same rock as on the north. Add to this, that but a few miles farther east the rock does actually run around the end of the granite, there seems good reason for expecting to find the plains of Terraba to be Tertiary. Further, as far north and northwest as I have traced sedimentary rocks, they are of the same age, as proven by fossils. The whole Atlantic slope of Costa Rica may be safely predicted to be Miocene.

Above Moen are broad, flat plains, which sweep around to the bases of the volcanic peaks of Turrialba and Irazu, and the farther north I followed the Miocene the less disturbed I found it. East of the volcano of Turrialba I found Miocene, with abundance of characteristic fossils, but little tilted, and in one place actually horizontal. Sweeping round to the westward, the Candelaria range is made up of the same rock, here somewhat disturbed, but with a general strike corresponding to the trend of the range. Beyond this everything is covered with great beds of volcanic ash and cut up by innumerable dikes of volcanic rock. North of the valley the magnificent line of volcanoes, Turrialba, Irazu, Barba, and Poos covers all the other rocks; but on the west face of the Aguacate Mountains, where the coating of ash is in places absent, we find highly altered claystones. These are rich in mineral veins, and have been thoroughly explored for mines of gold and silver. Whether the rock is the ancient clay slate from which the conglomerates were formed, or whether it is highly metamorphosed Tertiary, we have, as yet, no means of knowing, and the problem will require careful study for its solution.

Costa Rica, Nov. 29, 1874.

Art. XXIV.—*Note on the Discovery of a new locality of Primordial Fossils in Rensselaer County, N. Y.*; by S. W. Ford.

Several weeks ago, in company with my friend Mr. James C. Bell, Jr., of this city, I visited the hills a short distance to the east of the village of Lansingburgh, for the purpose of examining more minutely than I had previously done the interesting series of rocks there exposed. The strata first met with in going eastward from the village consist, in ascending order, as follows: (1) a series of thin-bedded gray sandstones with slight shaly partings; (2) a series of fine black slates and shales alternating with thin sandstone layers of a pinkish color, the surfaces of which are frequently profusely covered with ripple-marks and worm-burrows; and (3) a deposit of heavy-bedded gray sandstone with fractures running in every direction, and with now and then thin shaly seams which serve to indicate the bedding: the surfaces of the fractures, with few exceptions, thickly studded with small crystals of quartz. The thickness of the above enumerated beds cannot here be given with exactness; but that of the thin sandstones is not far from thirty feet, and that of the superposed slates and shales about twenty. The heavy-bedded sandstones are of much greater extent At one point they form a sharp, bold elevation, locally known as "Diamond Rock." All of these beds are arranged in a series of close synclinal and anticlinal folds, leaning to the northwest; and, as a result of this arrangement, the rocks of this region have a nearly uniform southeasterly dip. I was unable to obtain any fossils in these rocks, but at a locality about half a mile eastward of the first exposures examined I was more successful. At this point an apparently higher series of slates is met with, and these are, in turn, succeeded by a considerable deposit of conglomerate limestone. This latter rock is a most singular formation, appearing as if composed of innumerable blocks and wedges of limestone confusedly thrown down and afterward firmly cemented together. It appears almost incredible, at first sight, that fossils should occur in such a looking mass. But in a detached block of limestone lying only a short distance from the deposit in question, and to which I am satisfied it belongs, I found upward of half a dozen imperfect heads, and three perfect specimens of what I consider to be the pygidium of *Conocephalites* (*Atops*) *trilineatus* Emm. In addition to these I obtained from the main mass an unequivocal specimen of *Hyolithes Americanus* Billings; and on examining, on my return home, a piece also broken from the rock *in situ* by Mr. Bell, I found a specimen of the species described by me under the name of *Obolella nitida*. All of these species likewise occur in the Lower Potsdam limestones at Troy. No perfect specimens of the pygidium of *C. trilineatus* have, to my knowledge, hitherto been found; and while reserving the complete description of it till another time, I will refer to its leading characters here. It is nearly semicircular in form, the axis strong, composed of seven segments, with a row of obtuse spines along the middle; lateral lobes composed of five segments each, with distinct intervening grooves; marginal-rim nearly flat, widest toward the front, and distinctly and regularly notched or denticulated all around.* The surface of the border is finely granular. On page 405 of his "Palæozoic Fossils," vol. i, Mr. Billings has figured the pygidium of a trilobite from the Quebec group, which has the marginal-rim notched in precisely the same manner as that of *C. trilineatus*. Mr. Billings does not refer it positively to any genus. On comparing the characters furnished by the figure with those of the pygidium of *C. trilineatus*, I strongly suspect that the form in question belongs to the genus *Conocephalites*. At present, it appears to me quite probable that it is the tail-piece of *Conocephalites Zenkeri*, described on page 398 of Mr. Billings' work cited.

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* In the configuration of the border above noticed, this species differs widely from any species of the genus hitherto described. Barrande, in his beautiful work on the Trilobites, places the genus *Conocephalites* in his group of genera in which the border of the pygidium is unbroken or entire (Systême Silurian de la Bohême, vol. i, p. 220). The species known as *Conocephalites Iowensis*, from the Wisconsin Primordial, furnishes another exception to the rule. In that species the border of the pygidium is prolonged backward into two long diverging spines. (16th Reg. Rep.)

I regard the Lansingburgh limerock as the stratigraphical equivalent of my limestone band No. 1, at Troy. (See this Journal for August, 1873.) They are lithologically similar; and, moreover, have furnished two species in common, namely, *C. trilineatus* and *Hyolithes Americanus.* At Troy this deposit is similarly underlaid by a heavy slate formation, while apparently below this, and in the same line of outcrop, there are exposed at one point a few layers of thick-bedded sandstone similar to that constituting "Diamond Rock." The inferior slates and thin-bedded sandstones of the Lansingburgh section are, so far as direct observation goes, here wanting; but, if I am right in my interpretations, they are most probably present, but are not brought up to view. The equivalency of the Troy and Lansingburgh beds cannot be proved by actual inspection of the rocks between these two places, since, for the greater portion of the distance, there are almost no exposures. In my first regular paper relating to the rocks of this district (this Journal, July, 1871), I briefly referred to the Lansingburgh slates and sandstones described at the opening of the present article, and spoke of them as most probably an inferior division of the Lower Potsdam. This provisional reference was based alone upon the stratigraphical evidence I then possessed, as the accompanying limerock had at that time yielded no positive testimony bearing upon the subject. The evidence now in hand seems to sustain the correctness of that reference, but it is quite possible that these lower sandstones may yet be found to contain a still more ancient fauna. I hope in the coming spring, if my opportunities permit, to give this field a more careful and extended study.

Troy, N. Y., Dec. 26th, 1874.

ART. XXV.—*Abstract of a Memoir on the Origin, and Mechanism of Production, of the Prismatic or Columnar Structure of Basalt;* by ROBERT MALLET, F.R.S. (Read before the Royal Society, January 21st, 1875.)*

THE author, having briefly traced the history of geological opinion on this subject from before the period when the controversy in which the igneous or aqueous origin of basalt may be viewed as settled, and having stated the views of some of the more prominent British authors of recent date on this subject, points out that, up to the present, no clear and definite theoretic views have been enunciated to account for the prismatic structure in basalt, and that it is impossible to gather, with any distinctness, from systematic writers, whether prismatic structure be due to contraction by cooling alone, or whether the structure is due to preëxisting concretionary or crystalline arrangement of the integral particles of the mass, or to this coacting with enormous external pressures, the origin of which is left perfectly vague, or to some play of successive and joint actions of all these various forces.

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* We are indebted to the author for this abstract.—EDS.

Professor James Thomson, in a paper read some years ago, and since repeated at the Belfast Meeting of the British Association, has proposed views in some respects new, tracing prismatic structure to contraction by cooling only, and has expressed entire doubt as to the part supposed to be played by concretionary spheroids pressed together. Professor Thomson's views, however, are still far from complete, and the mode assigned by him to the production of cup-shaped cross-joints in the prisms fails to account for the phenomena.

The aim of the author is to point out in this paper that all the salient phenomena of the prismatic and jointed structure of basalt, as observable in nature, can be accounted for upon the admitted laws of cooling, and contraction thereby, of melted rock possessing the known properties of basalt, the essential conditions being a very general homogeneity in the mass cooling, and that the cooling shall take place slowly, principally from one or more of its surfaces.

Thus, taking the simple case of a tabular mass of molten basalt, whose top surface is level, the depth being great, and the two other dimensions indefinitely greater than that, and assuming the material at one temperature initially, and homogeneous and isotropic, and that cooling takes place from the top surface only, he, on these data, proceeds to consider the phenomena that will successively result by contraction in cooling.

While the mass remains at its upper part still plastic by heat, contraction will be met by internal movements and subsidence of the top surface, and no cracking or splitting can take place until the material there has become rigid enough to break under tensile strain. He points out that this degree of rigidity, or "splitting temperature," is not reached until the top surface has fallen to between 900° and 600° Fahr.

At this temperature the cooling surface begins to separate by fracture, penetrating perpendicularly to it into smaller surfaces. These must be similar and equal in area, and such that their edges in contact can make up a continuous superficies. To relieve the orthogonal strains in the cooling surface, and to meet the above conditions, only three geometric figures for the separating surfaces are possible, namely, the equilateral triangle, the square, and the regular hexagon.

The author then inquires why the last of these is normally the force found in nature. He traces this to the law of least action which governs the play of all natural forces whose final result is produced by the least possible expenditure of force. He shows that, in a contracting surface splitting up into equal areas, the expenditure of work will, for the equilateral triangle, the square, and the regular hexagon, be approximately as the numbers 1·000, 0·680, and 0·519. This economy of force decides the hexagon as the form found in nature. The diameter of the hexagon, which is the upper surface of the inceptive hexagonal prism, is shown to be fixed by the relation that subsists between the coefficient of contraction and that of its extension at rupture by a tensile force at the splitting temperature. This decides the diameters of the separate prisms. The splitting by contraction proceeds into the mass always in a direction perpendicular to the cooling surface, and at any instant the splitting is limited in its progress by the isothermal *couche*, which is at the splitting temperature within the mass; for within that *couche* the mass is still plastic. In the case assumed the prisms formed are straight and vertical. When the splitting has proceeded to some distance within the mass, the further cooling of each prism takes place not only from the top, but from the sides, and the more important conditions influencing the latter in nature are pointed out.

Any one prism is coldest at its extremity, and its temperature increases along its length to the other end, where the splitting is still proceeding. The prism is hotter, also, for any transverse section, as we approach its axis, than about the exterior; differential strains in the longitudinal direction thus take place, by cooling and contraction, between the successive imaginary *couches*, taken from the exterior to the axis of the prism, which tend to cause the outer portions of the prisms to tear asunder at intervals in length dependent, like the diameter of the prisms themselves, upon the relation subsisting between the coefficient of contraction and of extensibility at rupture of the material.

The prism contracts not only in its length, but in its diameter; transverse fracture at its surface, when it occurs, is therefore due to the resultant of two orthogonal forces, the one parallel to the axis of the prism, as already referred to, and the other in a plane transverse to the axis. These two forces are proportionate, the first to the length of the prism from a preceding joint, or from its extremity, the second approximately to the semi-diameter of the hexagon; and the resultant of these two, at any point taken round the prism, is oblique to the axis, and tending toward it in direction. As fracture in a homogeneous solid always takes place transverse to the line of strain, so the fracture producing a transverse joint takes place oblique to the sides of the prism; the obliquity becoming less as the fracture advances toward the axis of the prism, so that when complete it is cup-shaped, the convex surface of the fracture always pointing in the same direction as that in which the splitting of the prism itself is proceeding.

This solution, which is believed to be the first ever presented which, resting upon admitted laws, completely accounts for the production of the very remarkable cupped-shaped joints, is verified and illustrated by several diagrams, showing the mode of production of these joints, and the modifications of their curvature produced by varied conditions in the cooling.

It is further shown that the partial or complete detachment of certain fragments, frequently observed to be partially or wholly detached from the cusps of the concave side of these joints at and near the solid angles of the hexagon, is a consequence necessarily resulting from the mode of production of the joints themselves. The author then points out that in the case of very slender prisms other (and mechanical) conditions besides those of differential cooling enter into the production of the cross joints, which are at more considerable and irregular distances apart, and in planes of fracture often nearly transverse to the axis of the prism.

He also discusses the modifications produced in the prisms themselves, and in their cross joints, by heterogeneity in the mass of basalt itself—as, for example, by a more or less previously developed cleavage in the basalt in planes transverse to the axis of the prism, or by the presence of heterogeneous substances imbedded in the mass. To these latter, and to differences in conductivity, or in the cooling energy at different points of the cooling surface, are chiefly to be ascribed the divergencies from the normal hexagonal form of the prisms, as occasionally observed, the author remarking that where such divergencies occur they disappear, and the normal hexagonal form is returned to in such a manner as to require only the minimum expenditure of work.

The conditions producing greater or less interspaces between the prisms, which may vary from point to point of the same mass, are pointed out, as also those which cause the spaces between successive joints in adjacent prisms to coincide in successive planes, transverse to their axes, or the contrary.

The author then proceeds to discuss the various positions in space, and relatively to each other, which the axes of the prisms must assume, dependent on the general law, as already stated, that the axes of the prisms, however produced, are always normal to successive isothermal couches or planes at the splitting temperature, taken in succession within the mass.

If the mass be tabular, as already assumed, and cooling take place only from the top surface, the prisms will be straight and vertical, extending from top to bottom nearly of the tabular mass, and being separated from the bottom on which it rests by a more or less thick layer of irregular angular fragments, or of badly conducting material, tufa, scoriæ, &c., the convex surface of the joints all pointing downward. If the mass cool, both from the bottom and the top, the prisms, vertical and straight, will split upward and downward, and meet in an irregular intermediate stratum of angular fragments, the convex surfaces of the joints of the lower prisms pointing upward, and the respective lengths of the upper and lower ranges depending on their relative rates of cooling. If the tabular mass cool also from one or more of its sides, as by an abutting wall of rock, prisms will be produced with their axes perpendicular to that wall, and will be separated from the vertical ranges of prisms by an inclined stratum of angular fragments. Also if the basalt fill a crevasse producing a dyke, the prisms formed by cooling will be generally transverse to the plane of its walls, and meet somewhere toward the center in a stratum of more or less irregular fragments, due in all cases to the irregular contractions at the extremities of the prisms breaking up their mass there into wholly irregular forms. If the upper and cooling surface have a curved convex contour, the prisms shall be taper and convergent from the surface of the mass; and on the contrary, if the cooling surface have a concave contour, or rest upon a concave bottom, the prisms shall be divergent from the interior of the mass, the natural law of economy of work limiting the length or amount of taper in either case, limiting the length of the prisms, and a new range of larger diameter partially or wholly then commencing. The convergence or divergence are simple consequences of the general law, that the splitting takes place always normal to the isothermal *couches*, which are at the splitting temperature.

The author then proceeds to develop and illustrate by diagrams some of the varied and curious combinations which are observable in nature, and due to the more or less combined play of these conditions. He then proceeds to develop, as a consequence of the general law, the production of curved prisms, or those with apparently bent axes, which are observed in almost all basaltic countries. If the cooling mass of basalt be in one of its vertical sections of such a form that successive isothermal *couches*, taken in descending order, are not parallel to the original cooling surface, as they are in all cases of straight and parallel prisms, but divergent gradually from the cooling surface and from each other, then the lines of splitting of the prisms, always normal to these *couches*, must be curved in one direction. This will be true whether the isothermal *couches* be plane surfaces divergent from a thinner to a thicker part of the mass, or whether they be curved surfaces arising from the mass reposing on a curved bottom, and diverging in like manner. This explanation of the production of curved prisms, without the necessary intervention of external mechanical forces, having bent into curves prisms originally formed straight, is, the author believes, here for the first time presented. He shows that great difficulties exist to the supposition that curved prisms are ever the result of the bending of prisms originally straight by extraneous mechanical effort. The author having thus shown that all the salient phenomena presented in nature by the forms, jointings, positions of the prisms, &c., of columnar basalt are accounted for as consequences of contraction in cooling, submits that this solution given by him must be the true one. He, however, proceeds to examine at some length the different views of those who have imagined that prismatic and jointed basalt has resulted from the squeezing together by some wholly imaginary external force of spheroidal masses, more or less resembling those known as "onion stones," or concretionary spheroids, such as those imagined by Mr. Gregory Watt. The author submits all points of the subject to a searching examination, and points out that, upon the only probable suppositions that can be made as to the pre-arrangement of such spheroids, no extraneous force of compression could produce prisms at all, but must squeeze the spheroids instead into rhombic dodecahedrons.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Atmospheric Hydrogen Peroxide.*—Between the first of July and the first of December, 1874, SCHÖNE examined one hundred and thirty specimens of rain, and twenty-nine specimens of snow, for hydrogen peroxide. These experiments were made in the vicinity of Moscow. Of the whole number of specimens of rain, only four failed to respond to the test, though out of the twenty-nine specimens of snow, twelve gave no reaction. Having established the fact, the author continued his investigations with reference to the following points: (1) Form of occurrence of hydrogen peroxide in the atmosphere; whether gaseous or dissolved in the fluid, or solid rain or hail; (2) Relation to other meteoric phenomena, to time of day and to season of the year; (3) Relation to the ozone of the atmosphere; (4) How produced in the air; (5) Part played by it geologically and botanically; (6) Action upon the animal economy when breathed; and (7) Hygienic

importance. For this purpose, all the rain, hail, snow, dew, and frost were collected and tested for hydrogen peroxide, the analysis being quantitative when possible. Further, at various times, especially in clear weather, artificial dew and frost were prepared and examined. Careful meteorological records were kept during the entire interval at the adjoining observatory. The ozone was determined by a Schonbein's ozonometer. The results show: that the quantity of hydrogen peroxide in rain varies from 0·04 to 1·00 milligram per liter; that the larger the drops, the greater the amount; that the first rain after dry weather is poorer in peroxide than that which falls later; that the peroxide is greatest when the wind is south and southwest, that in the rain brought by the equatorial current being greater than that which falls in the rain produced by the conflict of this with the polar current, or brought by the latter current itself; that the relative quantity of peroxide in rain increases from the summer solstice to the autumnal equinox, and then diminishes; that the quantity is not greater in rain which falls during a thunder-shower; and that during the four months, the absolute quantity of hydrogen peroxide contained in the 221 liters of rain which fell upon each square meter was only 62·9 milligrams. In snow there was only 0·05 mgr. peroxide to the liter, the amount diminishing toward the winter solstice. Natural dew and frost contain no peroxide, or at least, less than one twenty-five millionth of this substance. In artificial dew and frost, the amount of peroxide varied from 0·04 to 0·05 mgr. per liter, reaching on a bright moonlight night in summer 0·09 mgr. The amount increased with the altitude of the sun. The daily maximum was reached between 12 and 4 o'clock P. M., and the annual in the month of August. The amount is greater the lighter the temperature, the clearer the sky, the higher the absolute and the lower the relative humidity of the air. The author concludes that the peroxide is contained in the air both free and in solution, to the extent, as a maximum, of 0·000000268 c.c. in a liter. He also believes that sunlight plays an important part in its production. The experiments are still in progress.—*Ber. Berl. Chem. Ges.*, vii, 1693, Dec., 1874.

G. F. B.

2. *On the Absorption Spectra of Potassium and Sodium.*—ROSCOE and SCHUSTER have examined the absorption produced in the spectrum by the vapors of potassium and sodium. Fragments of the former metal, placed in tubes filled with hydrogen, were heated until the green vapors appeared. A continuous spectrum from a calcium light placed behind the tube, suffered absorption within it, and was examined by means of a Steinheil spectroscope of two prisms. A complicated absorption spectrum appeared, consisting of three groups of bands; the first, group α, appeared first and was in the red; the second, β, was on the one side, and the third, γ, on the other of the D lines. These bands shaded off toward the red and resembled in general the bands of iodine. The position of the bands was approximately measured. Group

α consisted of nineteen bands, varying in wave-length from 6844 to 6275 ten-millionths of a meter; group β of eight lines, from 6059 to 5901; group γ of thirteen lines, from 5860 to 5667. The ordinary spectrum lines of the metal were not seen reversed owing to the too feeble intensity of the calcium light. Similar experiments with sodium led to similar results. The blue vapor gave an absorption spectrum showing in the blue a group of bands (γ) eleven in number, followed soon after by a group in the red and yellow (α) consisting of twelve bands, and then by group (β) in the orange made up of seven bands. Sodium vapor in an iron tube heated to redness shows an absorption spectrum in which the red, the green and a part of the blue are removed. The D absorption lines widen considerably and a strong absorption band appears in the green. Hence only a part of the orange, the green and the ultra-blue are transmitted.—*Proc. Roy. Soc.*, xxii, 362; *J. Phys.*, iii, 344, Nov., 1874. G. F. B.

3. *Preparation of Glacial Formic Acid.*—In the ordinary preparation of strong formic acid, by passing hydrogen sulphide gas over lead formate, gently heated over a free fire, the product is always contaminated with sulphur products which communicate an unpleasant odor to the acid, and which cannot be removed. M. BERTHELOT finds that this may easily be avoided by conducting the decomposition at a low temperature. The well-dried lead formate is placed in a U tube which is immersed in an oil bath, the temperature of which must not surpass 130° C. Hydrogen sulphide is then slowly passed over it and the product is collected and purified by fractioning. Placed in a freezing mixture and solidified, it may be concentrated. The pure acid solidifies at a temperature of 8·6, a temperature considerably higher than that usually stated in the text-books.—*Bull. Soc. Ch.*, II, xxii, 440, Nov., 1874. G. F. B.

4. *On Phosphenylous Acid.*—Precisely as phosphorous chloride reacts with water to produce phosphorous acid, so phosphenylous chloride reacts to yield phosphenylous acid, according to the equation: $C_6H_5PCl_2+(H_2O)_2=C_6H_5PH_2O_2+(HCl)_2$. MICHAELIS and ANANOFF have made use of this acid, discovered by them, for the purpose of getting some light upon the constitution of phosphorous acid itself. The phosphenylous acid is obtained in white crystalline plates, easily soluble in water and alcohol, and fusing at 70°. It is monobasic. The author describes the potassium, ammonium, calcium, barium, lead and iron salts. Between the two views held on the constitution of phosphorous acid, one that it is a trihydroxyl derivative of phosphorus, $P(OH)_3$, the other that it is derived from phosphoric acid by replacing hydroxyl by hydrogen and is $PO(OH)_2H$, the authors sought to decide from the reactions of phosphenylous acid. This may be considered either as $P(OH)_2C_6H_5$, or as $C_6H_5PO(OH)H$. Which of these formulas is correct, may be determined by the action of phosphoric chloride. In the first case the reaction is:

$$P(OH)_2C_6H_5+(PCl_5)_2=PCl_2C_6H_5+(POCl_3)_2+(HCl)_2,$$

the products being phosphenylous chloride and phosphoryl chloride. In the second case:

$$C_6H_5PO(OH)H+(PCl_5)_2=C_6H_5POCl_2+POCl_3+PCl_3+(HCl)$$

the products now being phosphenylous oxychloride, phosphoryl chloride, and phosphorous chloride. Experiment entirely confirmed the latter view. In the case now of phosphorous acid itself, the products given when it is acted on by phosphenyl tetrachloride must be according to one of two equations In the first case: $P(OH)_3+(C_6H_5PCl_4)_3=PCl_3+(C_6H_5POCl_2)_3+(HCl)_3$. In the second case, the reaction is:

$$OPH(OH)_2+(C_6H_5PCl_4)_3=POCl_3+(POCl_2C_6H_5)_2+$$
$$C_6H_5PCl_2+(HCl)_3.$$

The phosphorous acid was prepared by acting on the chloride with water, and was treated with the tetrachloride in a flask. Phosphoryl chloride, but no phosphorous chloride, was formed. The fraction of the distillate boiling above 220° gave, when treated with water, phosphenylous and phosphenylic acids, thus proving the presence of the corresponding chlorides, $POCl_2C_6H_5$ and $PCl_2C_6H_5$. The authors believe, therefore, that the formula $OPH(OH)_2$ represents the true constitution of phosphorous acid. —*Ber. Berl. Chem. Ges.*, vii, 1688, Dec., 1874. G. F. B.

5. *On the Production of Sarcolactic Acid by Fermentation.*—MALY some time ago showed that, under the influence of the mucous coat of the stomach, many of the carbohydrates, as cane-sugar, grape-sugar, milk-sugar, dextrin, etc., were converted into lactic acid. The ferment in this case is a product of the dead tissue, since the living mucous membrane has no power to effect this conversion. Since that time, the author has observed that often, but not always, there is simultaneously formed some sarcolactic acid, proved to be such by the composition of the zinc salt and its much greater solubility. The precise conditions under which this formation takes place have not been determined; though in one case sarcolactic acid was the sole product. This acid has been alike produced from grape and from milk-sugar; the separation of the two zinc salts being effected by fractional crystallization. Maly also mentions the discovery of sarcolactic acid in the fluid of an ovarian cyst, far from muscular tissue.—*Ber. Berl. Chem. Ges.*, vii, 1567, Dec., 1874. G. F. B.

6. *On the Antiseptic Action of Salicylic Acid.*—KNOP, at Kolbe's suggestion, has made some experiments to ascertain the action of salicylic acid upon vegetation. The results prove that this acid has a marked action upon the vegetative activity of cells, whether these be the chlorophyll cells of the higher or the non-chlorophyll cells of the lower orders of plants, provided only the acid remain free in the liquid. Two vigorous maize plants, 4 or 5 decimeters high, grown in his well-known solution and well rooted, were immersed in 500 c.c. of this solution, to which had been added 100 c.c. of a solution of salicylic acid containing 0.4 per cent of the acid. A third, fourth and fifth plant was simi-

larly treated, using tartaric, citric, and lactic acids respectively in place of salicylic. In the salicylic solution, the roots were killed. The new roots put out during the duration of the experiment became dark upon their ends, and under the microscope their cells were seen to be markedly affected, the protoplasm separating from the cell-walls. A second crop of roots fared similarly; but the third was more successful, and the plant continued to live in the solution for a long time. No mould, however, formed on the surface of the liquid even after three weeks. The other acid solutions killed the maize plants in three weeks, but the surfaces were covered with a thick layer of mould. Neither maize nor buckwheat grains germinate after soaking in water to which an equal volume of a saturated solution of salicylic acid has been added, not even by planting in earth. Fifteen grains of corn were soaked in water containing $\frac{1}{10000}$ of salicylic acid, and then placed in a germinating fluid consisting of 50 c.c. gypsum solution, 50 c.c. solution salicylic acid of 0·1 per cent, in a liter of water; 14 failed to germinate. The same fact appeared with buckwheat, wheat, rye, and oats. A similar depression of vegetative activity was observed in the case of mould; the difficulty of germinating seeds in solutions in June and July, owing to the rapid production of mould, being entirely obviated by the use of a highly dilute solution of salicylic acid.—*J. pr. Ch.*, II, x, 351, Nov., 1874.

G. F. B.

7. *Outlines of Proximate Organic Analysis;* by ALBERT B. PRESCOTT, Professor of Organic and Applied Chemistry in the University of Michigan. 192 pp. 12mo. New York, 1875 (Van Nostrand).—This work is a useful and much needed laboratory manual, for the identification, separation, and qualitative determination of the more commonly occurring organic compounds. It is well arranged for the use of the student in the laboratory, and will be found valuable also to the teacher. It is to be followed by a manual of the "Chemical Examination of Alcoholic Liquors," by the same author.

8. *Passage of Gases through Liquid Films.*—Dr. F. EXNER has determined quantitatively the penetration of various gases through the film of a soap bubble, an effect shown qualitatively by Draper and Marianini. Calling C the co-efficient of absorption of the gas for the liquid of which the film consists, and δ the density of the gas, he has deduced the law that the velocities of diffusion of gases are proportional to C divided by $\sqrt{\delta}$. For the diffusion velocities of the gases examined, putting that of air $=1$, the following numbers were found by observation: $N=0·86$, $O=1·95$. Illuminating gas $=2·27$, $H=3·77$, $CO^2=47·1$, $H_2S=651$, $NH_3=46,000$. Within these extraordinarily wide limits, between 0·86 and 46,000, the observations are in perfect accord with the formula $\frac{C}{\sqrt{\delta}}$. Experiments were also made for the purpose of ascertaining the absolute velocity of diffusion. These gave for the diffusion of H in air through a soap film, that in one minute 1·88

cubic centimeters H and ·50 cub. centim. of air simultaneously pass through one square centim. of the film.—*Roy. Acad. Sciences, Vienna*, Nov. 5th, 1874; *Phil. Mag.*, xlviii, 547. E. C. P.

9. *Late improvements in Magneto-Electric Machines.*—M. L. T. Gramme describes the improvements effected in the machine which bears his name, by which its power is greatly increased. In 1872 one machine was made for producing the light, two for plating, and several small machines. The latter could redden 10 cms. of platinum wire ·3 mms. in diameter. Those now made redden 60 cms. of the same wire, though there is no increase in the cost or weight. This large increase is principally due to the employment of the new magnets in sheets of Jamin. In 1872 and 1873 twelve large machines for plating were constructed weighing 750 kgs. and containing 175 kgs. of copper apiece. Their height was 130 cms., their length 80 cms., they deposited 600 grams of silver per hour and required for this work a force of 75 kilogrammeters. The new plating machine has but one central ring instead of two, weighs 177 kgs. and contains 47 kgs. of copper. Its dimensions are 55 cms. on a side and 60 cms. high. Like the other, it deposits 600 grs. per hour, but requires only 50 kgmtrs. to run it. It therefore occupies but one-half the space of the old form, the weight is reduced three-quarters and the motive force one-third. These improvements are effected by suppressing the exciting bobbin, putting the electro-magnet directly into the circuit, by a better arrangement of the copper, and by slightly increasing the speed. The coils, instead of being formed of wire, are now constructed of a band of thin copper, of width equal to one-half the length of the bars of the magnet, so that four large ribbons are used for each machine.

The plan of putting the electro-magnet into the main circuit gives rise sometimes to a curious change in polarity. When the machines are in motion, and the circuit closed through metallic baths, the direction of the current remains the same indefinitely. But if suddenly stopped, the current induced by the electro-magnet reverses the polarity of the latter, so that if the machine is again started the current passes in the opposite direction and removes the silver it has just deposited. To avoid this trouble, he inserts in the circuit an electro-magnet, which ordinarily hold up an armature, and keeps the circuit closed, but as soon as the speed slackens below a certain point, the armature drops and breaks the circuit, thus preventing the induced current. By applying these improvements to the old form of machine, its capacity was at once raised from 600 to 2,100 grams per hour.

The first machine for generating the light weighed 1,000 kgs., the copper of the magnets weighing 250 kgs., that of the ring 75 kgs. The space occupied was 80 cms. on a side and 125 cms. high. This was used a long time in the tower of Westminster, and for two years has not given much trouble, though it heated a little and gave sparks around the commutator. The new form weighs 183 kgs. and contains only 47 kgs. of copper. Its length and breadth are 55 cms., and its height 60 cms. The capacity is however much less, being only 200 Carcel burners, instead of 1,000. A greater light may nevertheless be obtained by an increased speed, as is shown by the following results of ten series of experiments: 650 turns gave a light of 77 burners; 850 gave 125; 880, 150; and 900, 200 without heating or sparks. 935 gave 250 with slight heating, and 1,025 gave 290, but produced heating and sparks. Comparing with the machine of the company of the Alliance, in use at the lighthouse of La Hève, we find that this also gave a light of 200 burners, but weighs 2,000 kgs. and occupies a space of 170×135×150 cms. Its weight is therefore twelve times as great, the surface covered seventeen times as great, and the volume eighteen times as great.

These machines having neither cranks, connecting shafts or dead-points, are well adapted to the transformation of electricity into work. One of the small machines gave the results shown in the accompanying table, when run with Bunsen cells of 0·20. The first column gives the number of cells, the second the number of turns, and the third the work done in kilogrammeters. This suggests a convenient method of transmitting power, placing one machine near the source, and transmitting the current to any distance by a wire or cable. A conclusive experiment was made as follows: a machine was driven by a steam engine, by a power of 75 kgmtrs., determined by a friction brake. The current moved a second machine with a second brake, which gave 39 kgmtrs., or more than half. But as the power was first changed into electricity and then the electricity into power, the coefficient of efficiency of each machine must have been over 70 per cent.

Cell.	Turns.	Power.	Cell.	Turns.	Power.
2	760	·32	7	1100	4·14
3	810	1·02	8	1100	5·00
4	1000	1·02	8	900	4·81
4	900	1·80	9	1500	5·11
5	1100	2·50	10	1700	5·52
6	1000	3·36	10	1300	6·16

A curious form of machine is made by winding with two wires, one coarse, the other fine. The current from two Bunsen cells is then passed through the first, running the machine, and producing a current of high tension in the fine coil. A telegraph may thus be run by two Bunsen cells.—*Compt. Rend.*, lxxix, 1178. Articles fully illustrated on the same subject may also be found in *Engineering*, Nov. 27th, 1874, and in the *Annales Industrielles*. E. C. P.

10. *Expansion of Phosphorus.*—MMS. PISATI and DEFRANCHIS have measured the expansion of both solid and liquid phosphorus at temperatures from 0° to 270° C. They employed a cylindrical dilatometer with a graduated neck. This was immersed in a bath of oil kept stirred continuously, and the greatest errors were estimated at a tenth of a degree. The readings were all reduced to degrees of the air-thermometer by a direct comparison. From a

large number of experiments they deduced the following results. The specific volume of solid phosphorus is given by the equation:

$$w_t = w_0 + 2000 \times 10^{-7} t + 1150 \times 10^{-10} t^2$$

and of liquid phosphorus by:

$$w_t = w_{50} + 2969 \times 10^{-7} (t - 50) + 2115 \times 10^{-10} (-50)^2$$

The mean co-efficient of dilatation per degree for solid phosphorus from 0° to v° is given by the formula:

$$R = 3674 \times 10^{-7} + 211 \times 10^{-9} t,$$

and for liquid phosphorus:

$$R = 5167 \times 10^{-7} + 370 \times 10^{-9} (t - 50).$$

—*Gazz. Chim. Ital.*, iv, 1874. E. C. P.

11. *Velocity of Light.*—M. Cornu gives the results of some measurements he has made on the velocity of light under the direction of the Council of the Observatory, at the suggestion of LeVerrier and Fizeau. The method employed was that of the toothed-wheel, first employed by Fizeau in 1849. The result then obtained was 298,500 meters from measurements between the Polytechnic School and Mt. Valérian. The stations now selected were the Observatory and the tower of Montlhery, distant about 23 kilometers, a distance accurately determined, and famous for its use in the determination of the meter, and more recently for the velocity of sound. The telescope at the Observatory had a focal length of 885 cms. and an aperture of 37 cms., the other a focal length of 200 cms. and an aperture of 15 cms. A simple reflector was placed at its focus, and the whole was enclosed in a large iron tube and built into the wall. The toothed wheel could be turned with any velocity up to 1600 turns per second, and by a chronograph and electric register the time measured to thousandths of a second. The method of measurement is well known, and the precise time of disappearance was marked and the velocity measured electrically. This was necessary, since it was impossible to preserve a perfectly uniform motion. 504 observations were thus taken, and the results are given in the following table. The column headed n gives the number of teeth that passed during the passage of the light; the column headed m gives the number of times the observation was made, and the column V gives the corresponding velocity. The mean of the whole, giving proper weights to each, is 300,330 in air, or 300,400 meters in vacuo, with a probable error of only one-tenth of one per cent.

n	m	V
4	15	300,130
5	33	300,530
6	20	300,750
7	10	300,820
8	7	299,940
9	94	300,550
10	69	300,640
11	72	300,350
12	3	300,500
13	4	300,340
14	9	300,350
15	65	300,290
16	4	300,620
17	22	300,000
18	35	300,150
19	6	299,550
20	—	—
21	36	300,060

The methods of measuring the parallax of the sun may be divided into three classes. 1st. The physical methods dependent on optical phenomena. They include the eclipses of the satellites of Jupiter and aberration of the fixed stars, together with the value of the velocity of light. Employing the results here obtained give 8″·88, 8″·88 and 8″·80, or a mean 8″·85 as the solar parallax. 2d. The analytical methods of astronomical observations with theoretical laws based on the theory of gravitation. They give the value 8″·86. 3d. The purely geometrical methods by the parallax of the planets near the earth. The opposition of Mars in 1862 gave 8″·84. But the greatest accuracy is attained by the observations of the transit of Venus.—*Comptes Rendus*, lxxix, 1361. E. C. P.

12. *On a new way of illustrating the Vibrations of the Air in Organ Pipes;* by Prof. JOSEPH LOVERING, of Cambridge, Mass. (From the Proceedings of the American Association for the Advancement of Science, vol. xxiii.)—I shall begin with a brief description of the methods adopted by W. B. Rogers and Kœnig for making visible the vibrations of the invisible air in organ pipes. By the first method two gas burners were fed from a common reservoir; a glass tube was placed over one burner and the length of the flame altered until it was brought into unison with the fundamental note of the tube, and an energetic vibration of the flame and the column of air began. The flame on the other burner also vibrated from sympathy. The individual vibrations of this naked flame were unraveled by making the burner revolve in a small circle of about one inch in diameter. A cylindrical sheet of light resulted from the motion, which was channelled by dark spaces as soon as the sound was heard. The method has the advantage of giving an object which can be seen in all directions, with the disadvantage, however, on account of the small circle in which the flame travels, of crowding upon each other the alternate dark and bright spaces of the fluted surface.

The description just given has reference to the apparatus constructed by Kœnig under the name of the *Appareil à flammes chantantes de M. le Comte Schaffgotsch.* But I have not been able to find in Schaffgotsch's account of his apparatus and experiments any allusion to this method of exhibiting to the eye the individual vibrations by the revolution of the flame itself. Probably, it was an appendage to Schaffgotsch's apparatus suggested to Kœnig by the experiment of Prof. W. B. Rogers.*

In this experiment only a single flame was employed; a tube being placed over it large enough to allow the interior flame to revolve in a small orbit.

The second method for accomplishing the same object is a device by Kœnig, and is known under the name of the *Manometric Flames.*† An opening is made in the side of an organ pipe at the place where the greatest changes of density occur when the pipe is sounding: to this hole be applied a small box, the end which is placed upon the hole being covered with a delicate membrane.

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* Amer. Journ. Science, xxvi, 1858. † Ann. Chem. und Phys., cxxii.

Gas is introduced into this box by one tube and then conducted from the box to the burner by another tube. The vibrating air in the pipe plays upon the membrane, and communicates vibration to the flame. The individual vibrations of the flame are revealed to the eye, by reflecting it in a revolving mirror after the manner of Wheatstone. Four mirrors placed upon the four vertical sides of a revolving cube produce the same effect with a smaller velocity. This method has the disadvantage of substituting for the flame itself a faint and virtual image, not easily seen in daylight or in all directions. It has the advantage of admitting of a large orbit of revolution and thus separating widely the individual vibrations of even high notes.

I have endeavored to combine the advantages of both methods without incurring their disadvantages. Imagine eight arms of gas pipe, each one foot in length, arranged as the spokes of a horizontal wheel upon a hollow hub, which is lengthened out below to the extent of six inches. This hub is closed at the upper end by steel and is balanced upon a steel point at the top of a hollow upright through which the gas is delivered. Two holes are made in the sides near the top, through which the gas flows out into the hollow hub and the attached arms. The upright comes up through the bottom of a vessel, into which water is poured in sufficient quantity to immerse the bottom of the hub and prevent leakage of the gas below. In this way great freedom of motion is secured on the steel point, with a perfectly tight joint. The arms are rotated in a large circle by clock-work, and their weight suffices to make them serve as a fly-wheel and acquire a uniform motion. The arms are closed at the ends, and the gas issues in eight vertical streams from as many holes in the tops of the arms. When these streams are lighted, the rotation produces a large cylindrical sheet of light. The greater the number of arms the smaller is the velocity required to produce persistency of impression in the eye, with the additional benefit of having a well-balanced instrument. Vibration of the flames is produced by attachments to the organ pipes similar to those used by Kœnig. When the full pressure of the gas is on, the cylindrical sheet of light is three or four inches in height, and, when the organ pipe speaks, it is broken up as neatly as when the virtual image is used in Kœnig's apparatus. The effect can be seen readily in full daylight and from every part of a large room. The best effect is with organ pipes at least three feet in length and open at both ends. Of course, the serration is finer when shorter pipes are used. The experiment is very beautiful, even with short pipes, if the flame is brought down to the size of a pea. In this case the illuminated circle, as soon as the sound begins, is broken up into bright beads loosely strung upon a dark cord. On one side of each flame a screen is placed, which moves on a hinge and can be set at any angle with the plane of rotation. These screens serve the double purpose of checking the velocity, if the energy of the clock movement is too great, and of preventing the flame from flaring if the motion is too rapid. For purposes of research, Kœnig's apparatus may be all that is desired. But for illustration in class rooms, and especially to large numbers, the new arrangement just described will be found to possess great advantages.

II. Geology and Natural History.

1. *New Order of Eocene Mammals.*—At the last meeting of the Connecticut Academy, Feb. 17th, Professor O. C. Marsh made a communication on a new order of Eocene Mammals, for which he proposed the name *Tillodontia.* These animals are among the most remarkable yet discovered in American strata, and seem to combine characters of several distinct groups, viz: Carnivores, Ungulates, and Rodents. In *Tillotherium* Marsh, the type of the order, the skull has the same general form as in the Bears, but in its structure resembles that of Ungulates. The molar teeth are of the ungulate type, the canines are small, and in each jaw there is a pair of large scalpriform incisors faced with enamel, and growing from persistent pulps, as in Rodents. The adult dentition is as follows:—Incisors $\frac{2}{2}$; canines $\frac{1}{1}$; premolars $\frac{3}{2}$; molars $\frac{3}{3}$. The articulation of the lower jaw with the skull corresponds to that in Ungulates. The posterior nares open behind the last upper molars. The brain was small, and somewhat convoluted. The skeleton most resembles that of Carnivores, especially the *Ursidæ*, but the scaphoid and lunar bones are not united, and there is a third trochanter on the femur. The radius and ulna, and the tibia and fibula are distinct. The feet are plantigrade, and each had five digits, all terminated with long, compressed and pointed, ungual phalanges, somewhat similar to those in the Bears. The other genera of this order are less known, but all apparently had the same general characters. There are two distinct families, *Tillotheridæ*,* in which the large incisors grew from persistent pulps, while the molars have roots; and the *Stylinodontidæ*, in which all the teeth are rootless. Some of the animals of this group were as large as a Tapir. With *Hyrax* or the *Toxodontia* the present order appears to have no near affinities.

2. *On the alleged parallelism of Coal beds;* by J. J. Stevenson. (Proc. Amer. Phil. Soc., xiv, 283.)—Mr. Stevenson in this paper gives examples, from this country and England, of the subdivision of coal beds, and of variations in the thickness of the intervening rock strata. The interval between the Upper Freeport and Kittaning coal beds along Yellow Creek in Ohio varies from 80 to 160 feet in five miles. The same interval on Wells Creek varies in one mile from 8 feet to 28, and from this it enlarges beyond [distance not stated] to 110 feet. In six sections given by Prof. Rogers in his Pennsylvania Report the intervals are 184, 143, 142, 117, 109, 103. The total interval between the Pittsburgh coal bed and the Upper Freeport varies in Ohio from

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* This family may possibly prove to be identical with *Anchippodontidæ.*

420 feet at the west to 505 at Steubenville; in Pennsylvania it is 200 feet at Ligonier, 220 at Elk Lick, and 450 to nearly 600 feet on the Monongahela River; in West Virginia, along the Monongahela and Tygart's Valley River, it varies not much from 420 feet.

Mr. Stevenson, after stating many other facts, observes, in conclusion, that the variation in thickness arises mainly from the fact that the beds were deposited in a great trough having the Cincinnati uplift on one side and the Alleghany region on the other; that the diminution in thickness is quite regular east and west from the middle of the trough; that the subsidence as a whole was regular, approaching uniformity, but that there were "bulgings or other irregularities such as could not fail to accompany any operations so extensive." He further concludes that "all the coals of the Upper Coal group are offshoots from one continuous marsh, which existed from the beginning of the era to its close, and which in its full extent is now known as the Pittsburg coal seam."

3. *Diatoms of the Carboniferous.*—Count F. CASTRACANE of Rome has found, in an examination of the ashes of mineral coal from Liverpool, a large number of species of Diatoms. The most of them are freshwater; but with these occur a number of marine kinds.—*Am. Mag. Nat. Hist. for February, from the Actes de l' Acad. Pontif.*, Feb., 1874.

4. *A new Mastodon.*—Professor COPE has announced his obtaining *Elephas primigenius* var. *Columbi* from Quarternary beds at the base of the Zandia Mountains, and the *Mastodon Ohioticus* from corresponding beds near Taos, and from the valley of the South Platte in Northwestern Colorado. The Pliocene of Santa Fé affords the remains of a Mastodon, referred by Leidy to his *M. obscurus*, of which Cope makes a new species, *M. productus*.

5. *The Geology of New Hampshire. A Report comprising the results of Explorations ordered by the Legislature.* C. H. HITCHCOCK, State Geologist; J. H. HUNTINGTON, Principal Assistant. *Part I, Physical Geography.* 668 pp. roy. 8vo, with many illustrations. Concord, 1874.—This First Part of the Report on the Geology of New Hampshire commences with a history of the Geological Surveys in New Hampshire by Prof. Hitchcock. The chapters on this subject are followed by a history of explorations among the White Mountains by W. Upham; a chapter on the climatology of New Hampshire by J. H. Huntington; another excellent one on the use of the magnetic needle in surveying by E. T. Quimby, illustrated by maps; others on the topography of the State by C. H. Hitchcock, including a long catalogue of altitudes from railroad and other surveys; another on the river-systems by W. Upham; on the distribution of Insects in N. Hampshire, with a map, by S. H. Scudder (noticed on page 232); on the distribution of Plants by W. F. Flint; on the Natural History of the Diatomaceæ by A. Mead Edwards, with a plate; and chapters on the Scenery of the State by C. H. Hitchcock and J. H. Huntington. The illustrations are numerous and a number of them are photographic.

The survey has brought much to light that is new respecting the geology of New Hampshire. The occurrence in metamorphic New England of Upper Silurian fossils within a few miles of the White Mountains, which it has made known, is a fact of the highest geological importance; and that of the labradorite and chrysolite rocks in the White Mountain region is another. These discoveries are enough of themselves to prove the survey a success as far as geological science is concerned; for they are destined to give great aid toward unravelling the knotty points in New England geology.

Professor Hitchcock has reserved the details respecting these and the other geological results for another volume, and has given in the one issued—on the Physical Geography of the State—only his general conclusion with regard to the *ages* of the crystalline rocks, the facts being brought out in a chapter on "the physical history of New Hampshire." .This subject, moreover, is illustrated by a series of six colored maps representing the supposed geography—or areas of dry land and water—of the region of New Hampshire and Eastern Vermont during these several ages, four of which are made *pre-Silurian*, one *Cambrian* or *Primordial*, and one *Helderberg*. We only remark that the author does not seem to have sufficiently appreciated the fact that in a region which is metamorphic throughout, the immense amount of denudation that has taken place in past time throws great doubt over all attempts to mark out over it outlines of the *dry land* of successive pre-Silurian ages by the present superficial distribution and positions of the different kinds of rocks.

Some other opinions in Professor Hitchcock's part of the volume appear to us to need revision.

After speaking of the "extremely abundant Laurentian vegetation" (p. 508), he observes, with regard to the origin of beds of iron pyrites and copper pyrites in his "Huronian rocks," that "For the formation of sulphurets we require originally a sulphate ocean, just as in the case of gold," and then speaks of the deoxidizing agency of vegetation, reducing the sulphates to sulphurets; but he does not explain how "an extremely abundant vegetation" could have existed in "a sulphate ocean."

Mr. Hitchcock recognizes the existence of a long Azoic era, as well as Eozoic era, in pre-Silurian time, and then objects to the term *Archæan* for the pre-Silurian, on the ground that the terms Azoic and Eozoic are sufficient alone. In this we should agree with him *if* the rocks of the Eozoic part of the pre-Silurian time could in all cases be distinguished from those of the Azoic.

The existence of great beds of iron ore in pre-Silurian terranes is regarded as evidence (in this following Prof. T. Sterry Hunt) of the existence during the era of abundant vegetation—on the ground that, in making marsh-beds of iron ore, the oxide of iron, when carried in by the waters, is in the state of iron-salts of organic acids. But the era was a very long one: Helmholtz made the time which elapsed during the cooling from 2,000° C. to 200° C.

350,000,000 of years; and many more millions should be added for the continuation of the cooling down to 100° C.; all of which necessarily antedated the first appearance of the simplest forms of life; and more still to reduce the temperature to 38° C. (100° F.), which limit was probably reached before the close of the Archæan. And in that long era, even to its close, the atmosphere certainly contained a greatly larger proportion of carbonic acid than now, if not also other acids; and also, for this reason, it had much greater density. Hence carbonic acid, which does now some of the work of iron-transportation, may have done far more then. It is surely very unsafe to conclude from the existence of those iron ore beds that the vegetation was extremely abundant, or that any then existed.

Neither does the phosphate of lime (apatite), which is so generally disseminated through the iron ores, afford good evidence of life. The amount of this mineral in Archæan rocks exceeds that in all the later rocks of the globe, and would certainly indicate the existence of an extraordinary amount of life over the Archæan marshes and lands if of any at all. Some of the earliest shells of the Silurian (*Lingulæ* and a few related kinds) consist largely of phosphate of lime, as shown by Hunt, and this appears to prove, as this author has observed, that the waters of the ocean then held in solution more phosphates than they now do. And the grains or crystals of phosphate of lime in the iron ore may be proof only of the same fact with regard to the ocean's waters. The chemistry of the globe during any part, even the last, of the hundreds of millions of years before the opening Silurian is too doubtful for positive speculation as to what chemistry did not do and life did.

On the question of the animal nature of the *Eozoon* Prof. Hitchcock writes judiciously, excepting in a single remark. He observes that "those who disbelieve the organic theory are mostly better skilled in mineralogy than biology." But Messrs. King and Rowney, the chief contestants, are not mineralogists, but zoologists, and Mr. Carter of England, another strong opponent of the "organic theory," is also a zoologist, and one particularly versed in the lower orders of animal life. There are probably mineralogists that doubt, as there certainly are zoologists, but we can recall no articles by any such on the subject, excepting one or two which aim to show that the limestones containing *Eozoon* are sometimes of igneous origin, an observation which, whether sustained or not, cannot be attributed to mineralogical prejudices.

Notwithstanding some exceptions to the volume, we regard it as a repository of much valuable information on the physical geography and productions of the State.

6. *First Annual Report of the Geological and Agricultural Survey of Texas;* by S. B. Buckley, State Geologist. 142 pp. 8vo. Houston, Texas, 1874.—This Report is occupied mainly with observations of economical interest relating to soil, rocks, ores, trees. There are brief notices of the Tertiary, Cretaceous and older

rocks. But these show much ignorance of the subject. In a paragraph headed in capitals DEVONIAN, we find the author saying that "in 1860, when engaged with Dr. Shumard in the survey of San Saba County, some of the limestones and shales in the eastern part of that county were referred by him to the Devonian. The Trenton limestone was the formation recognized: its chief fossils found were of the following genera: *Belerophon*, *Maclurea*, *Orthis*, *Murchisonia*, *Pleurotomaria*, and some other genera of that period." A "State geologist" who refers the Trenton limestone to the Devonian is evidently not a geologist, whatever his State appointment. Such a mistake Dr. Shumard could not have made.

Another specimen of the Report may do its author better justice. In the chapter, a little over a page long, on the Jurassic of the State, three lines are used in stating that half a mile west and northwest of Phantom Hill, Callahan Co., there are "what may be fossils of the Jurassic period;" and three more in saying, "Intending to make a more careful study of them, I placed them in a box and shipped them to Austin, since which I have not seen them." The rest of the page on the Jurassic is occupied with an account of the scenery, buffaloes, etc., of Callahan County, in the course of which he says: "Old buffaloes, especially the old bulls, delight to roll and wallow in the soft dirt. I have seen them roll and kick up their heels, over and over, and then get up and shake themselves apparently with great satisfaction. It will not be many years before Northwest Texas will be resorted to by crowds of Northern invalids for the purpose of breathing its pure air, to behold its charming landscapes, and hunt and fish; hunt the buffalo, the antelope, the deer, wild turkeys, etc., and catch thousands of fish. Such pastimes will kindle the spark of life anew, and give new life to the body." This much for the Jurassic.

The Report contains charges against the late Dr. Shumard and his assistant, Mr. A. R. Roessler, which should have been stricken out by the Governor of Texas, to whom the Report is addressed, before the manuscript was sent to the printers. Dr. Shumard was an excellent geologist, and we believe an honorable man.

7. *Second Geological Survey of Pennsylvania. Report of Progress for* 1874; by PETER LESLEY, Director of the Survey.—The appointments for this new survey were not made until the month of June had far advanced, and consequently the parties were not in the field until July and August. The assistants appointed are: Mr. A. S. McCreath, chemical assistant; Prof. F. Prime, Jr., assistant in charge of the Lehigh district: Prof. P. Frazer, Jr., in charge of the York and Adams district; J. H. Dewees, in charge of the Juniata district; F. Platt, in charge of the Clearfield and Jefferson coal district; J. F. Carll, in charge of the Venango oil district; Dr. F. A. Genth, chemist and mineralogist; H. E. Wrigley, on the petroleum region of Western Pennsylvania; E. B. Harden, draughtsman. Two more assistants are to be appointed, one for Greene and Washington Counties, and

the other for the northern tier of counties, Tioga, Branford, Susquehanna and Wayne. Besides these there are a number of volunteers.

Although the parties were late in the field, valuable results have been obtained, which will soon be published in the Report for the year. Dr. Genth's Mineralogical Report is nearly printed; it will extend to about 150 pages. The geological reports of Professor Frazer, Professor Prime, Mr. Platt and Mr. Dewees, illustrated by several maps and sections, are finished and are either in the printer's hand or will soon be. Besides these, the volume for 1874 will include a special report on petroleum by Mr. H. E. Wrigley, which will contain a large map of the oil regions of West Pennsylvania and West Virginia, a small map of the oil regions of the Middle States and Canada, sections and other illustrations. Mr. Lesley's extensive knowledge of geology, theoretical and practical, and of the range of subjects that will come under investigation in the survey, together with his thoroughness and energy, ensures for the State of Pennsylvania, and for the science of the land, results of the highest value.

8. *Geographical Explorations and Surveys west of the* 100*th Meridian;* First Lieut. G. M. Wheeler, Corps of Engineers, U. S. A., in charge.

(1.) *Preliminary Report upon Invertebrate Fossils collected in* 1871–1873, with descriptions of new species; by C. A. White, M.D.—The species included are from the Primordial of the Cañon of the Colorado, Mohave Co., Arizona, the vicinity of Antelope Spring, House Range, Utah, and Pioche in Nevada; from the Quebec group of Utah and Nevada; the Trenton beds in Nevada, New Mexico and Arizona; the Subcarboniferous of Arizona, Nevada and Utah; the Carboniferous of Arizona, New Mexico and Nevada; the Jurassic period of Utah and Arizona; the Cretaceous of New Mexico, Arizona and Utah.

(2.) *Report upon Ornithological Specimens collected in* 1871–1873. 148 pp. 8vo. 1874.—Contains a Report on the birds collected in 1872, by Dr. H. C. Yarrow and H. W. Henshaw; on those of 1873, by H. W. Henshaw; annotated list of the birds of Utah, by H. W. Henshaw. The papers are full of valuable notes.

9. *Notes on the Natural History of portions of Montana and Dakota*, being the substance of a Report to the Secretary of War, on the collections made by the North Pacific Railroad Expedition of 1873, Gen. D. S. Stanley, Commander; by J. A. Allen, Naturalist of the Expedition. 62 pp. 8vo. 1874. (From the Proc. Bost. Soc. Nat. Hist., vol. xvii, June, 1874.)—Consists of notes on the Mammals, Birds, Reptiles, Amphibians, Plants and Butterflies collected by the Stanley Expedition.

10. *Geological and Geographical Survey of the Territories;* Dr. F. V. Hayden, Geologist in charge. Department of the Interior.

(1.) *Seventh Annual Report*, for 1873. 718 pp. 8vo. Washington, 1874.—This report is noticed from an incomplete copy, extending to page 533, in the December number of this Journal. A complete bound copy reached us on the 11th of February. The closing part includes Reports on Insects by W. L. Carpenter, C. R. Osten Sacken, H. A. Hagen; on Crustacea by S. I. Smith and A. S. Packard; on the methods of Topographical Survey by James T. Gardner; Topographical Report on the Middle Park Division by S. B. Ladd, and on the Gold Hill Mining Region, by A. R. Marvine. A further notice is reserved to another number.

(2.) *Bulletin, Second Series*, No. 1. 48 pp. 8vo. 1875.—Contains memoirs on the Fishes of the Tertiary shale of the South Park, by E. D. Cope; On the Cranial and Dental Characters of Mephitinæ, with description of *M. frontata*, a new species from the bone-caves of Pennsylvania; Ancient ruins in Southwestern Colorado, by W. H. Jackson, with four plates; on fossils from near the eastern base of the Rocky Mountains, west of Greeley and Evans, Colorado, and others from about 200 miles farther east, with descriptions of a few new species, by F. B. Meek.

(3.) *Contributions to the Fossil Flora of the Western Territories. Part* 1, *The Cretaceous Flora;* by LEO LESQUEREUX. 136 pp. 8vo, with 30 lithographic plates. Vol. vi. of the Reports.—Mr. Lesquereux has here brought out the results of his long and careful study of the Cretaceous flora of the Rocky Mountain region. The plants described are from the Dakota group, the lowest of the Cretaceous beds in that region. The facts show that the leaves lie near where they were dropped. The localities are between the parallels of 39° and 47°; and the leaves indicate a general uniformity of climate over these parallels, with perhaps a slightly warmer temperature in Kansas, where alone occur the genera *Credneria*, *Pterospermites* and *Dombeyopsis;* where the species of *Liriodendron*, and of some other genera have large leaves, while those of the Nebraska species are small. The climate indicated, according to Lesquereux, is like that now existing between the parallels of 30° and 45°, as is inferred from the occurrence of the genera *Salix*, *Fagus*, *Platanus*, *Sassafras*, *Aralia*, *Magnolia*, *Liriodendron*, *Menispermum*, *Rhus* and others.

He observes also that Heer finds evidence of the same climate in Greenland during the Upper Cretaceous, 28 species of the genera *Populus*, *Myrica*, *Ficus*, *Sassafras*, *Proteoides*, *Credneria*, *Andromeda*, *Diospyros*, *Panax*, *Magnolia*, *Rhus*, and others having been identified by him from beds in latitude 70°; while from the Lower Cretaceous he has made out a very different range of species, including 9 *Cycads*, 3 *Equisetaceæ*, 17 *Conifers*, 1 *Lycopod*, 5 *Monocotyledons* and 1 *Dicotyledon*, with 38 *Fucoids*.

Besides the genera above enumerated, Lesquereux has the following, exclusive of Cryptogams, in his catalogue of the groups represented. It includes, besides his own, Newberry's results.

Pterophyllum, of the order of Zamiæ?; *Sequoia*, *Araucaria*, *Abietites*, *Glyptostrobus*, *Phyllocladus*, *Geinitzia* of Conifers; *Arundo; Dioscorea; Flabellaria* among Palms; *Liquidambar*, *Populus*, *Populites*, *Myrica*, *Betula*, *Betulites*, *Alnus*, *Quercus*, *Celtis*, *Ficus*, *Nyssa*, *Laurus*, *Laurophyllum*, *Persea*, *Cinnamomum*, *Oreodaphne*, *Proteoides*, *Embothrium*, *Aristolochites*, *An-*

dromeda, *Diospyros*, *Sapotacites*, *Bumelia*, *Hedera*, *Cissites*, *Protophyllum*, *Acerites*, *Negundoides*, *Greviopsis*, *Anisophyllum*, *Celastrophyllum*, *Rhamnus*, *Juglans*, *Pyrus*, *Prunus*. The whole number of species enumerated is 130: of these, eight are of the genus *Quercus*, five, of *Populus*, six of *Platanus*, six of *Sassafras*, five of *Magnolia*, three of *Liriodendron*, and eight of *Protophyllum*. Some of the leaves are referred with doubt to existing genera, as they embrace characters of two or more. The leaves are mostly entire, coarsely veined and coriaceous. No beds in the Rocky Mountain region older than Cretaceous contain any related species, or a single Angiosperm. The author remarks that the facts "seem to prove a collateral development of different primitive types, and therefore, the appearance at certain epochs of those original forms which, at each geological period, have changed the character of the vegetable world, and which have not any connection with antecedent types."

(4.) *Lists of Elevations principally in that portion of the United States west of the Mississippi River.* Collected and arranged by H. Gannett, M.E. 72 pp. 8vo. Washington, 1875.—This pamphlet is the third edition of the List of Elevations issued by the Department of the Interior in connection with the publications of the U. S. Survey of the Territories. The Catalogue has been much enlarged by additions from railway levellings and other sources, and also by including the elevations of many points east of the Mississippi River. It is therefore a very important contribution to the department of North American topography.

(5.) *Meteorological Observations* made in 1873 and 1874 in Colorado and Montana, prepared by G. B. Chittenden. 58 pp. 8vo. 1875.

11. *Carte hydrologique du Department de Seine-et-Marne;* by M. Delesse, Ingenieur-en-Chef des Mines.—This chart represents in colors and by lines of equal height above the level of the sea, the distribution of the superficial and subterranean sheets or streams of water over the Department of Seine-et-Marne, in France, and the altitude of the surface. The facts have been obtained partly by collecting all that is known of the wells of the region. The geological stratum upon which the water rests or flows over the different parts of the department, and the depths or curves of its surface, are indicated. Many interesting conclusions are arrived at, some of them of geological interest, and all of them of great value to the Department. The survey and the charts make an excellent model for the world. The charts are in the very best style of the chromo-lithographic art.

12. *Note on the genus Calamodon;* by E. D. Cope. (Communicated.)—I observe in your last number that you state, on the authority of Professor Marsh, that this genus is identical with the *Stylinodon* of Marsh, already described in your May number of the current year. If Professor Marsh's diagnosis be accurate, the genera are distinct, as the description cited ascribes six molars to *Stylinodon*, while five only exist in *Calamodon*. This I stated in

my description on p. 6 of my report on the Vertebrata of the Eocene of New Mexico.

13. *Shepherd's Pipe of the Reindeer era.*—An instrument made of the bones of birds placed together as in the shepherd's pipe, which it is supposed was used as a musical instrument, is announced by M. Piette as having been found in a deposit of the Reindeer era (Stone Age).—*Les Mondes*, Jan. 28.

14. *Lead vein in Newburyport, Mass.*—The vein of lead ore recently discovered intersecting the gneiss of Newburyport has, according to Professor R. H. Richards, a lead-bearing band against its north wall, which averages a foot in width but widens in one shaft 22 feet deep to six feet. With the galena there is a little chalcopyrite and tetrahedrite. The rest of the vein is a rusty crystalline rock free from mica, whose precise nature is not yet determined. The lead-bearing band is from one-third to one-half galena. A ton (2240 lbs.) of the crude ingot lead obtained from the ore yielded 74 ounces of silver and 341 grains of gold.—*Proc. Boston Soc. Nat. Hist.*, xvii, 200.

15. *Record of Geological Literature.*—An Annual of this title is to be commenced the coming summer by W. Whitaker of the Geological Survey Office (Jermyn st., London, S.W.). The volume will contain short abstracts or notices of Papers, Books, Maps, etc., published during the year 1874, in the departments of Geology, Paleontology and Mineralogy. It will include 200 to 300 pages and be sold for 10s. 6d. Such an Annual Record is greatly needed. Subscriptions are solicited.

16. *Composition of the native alloy of Gold and Silver in the Comstock Lode, Nevada.*—Mr. Melville Attwood, of San Francisco, in a communication to the Microscopical Society in April, 1874, describes the pale yellow alloy from the Comstock lode as occurring in similar form and composition throughout the whole length of the lode and from the croppings down to the bottom of the deepest workings. It is found occasionally in coarse pieces, but in general is finely disseminated and intimately mixed with the silver ore. It exhibits imperfect octahedral crystals and is found also in aborescent filiform masses. Hardness 3; specific gravity 12·5; after melting, 13·5 to 13·7. Color white, with a slightly yellowish tinge when freshly broken, but becomes more yellow on exposure for some time. Analysis yielded Mr. Attwood 55·37 per cent of gold, 42·87 of silver and 1·74 per cent of substances undetermined. W. P. B.

17. *Mineralogical Note;* by Albert R. Leeds, Prof. of Chemistry, Stevens Institute. *A magnesia-iron Tremolite, not asbestiform, from the soapstone quarry above Manayunk, Pa.* (Contributed by the author.)—This mineral is the *anthophyllite*, so called, given in the list of American localities on page 780 of Dana's Mineralogy, 5th edition. It occurs in a dark-colored serpentine rock, in bladed masses sometimes exceeding 1 mm. in thickness and 1 cm. in breadth in their largest part, and then thinning into fine fibers which lose themselves in the rock. These blades, with

their various alterations in size and direction, are frequently many centimeters long. They are fibrous, of vitreous luster, translucent and of slight yellow, greenish or reddish tints, derived from the serpentine and oxide of iron accompanying them. H.=2·5. Fusibility=5·5.

	I.	II.	Mean.	O. ratio.		
SiO_2	59·27	59·228	59·25	31·40	31·40	2·1
Al_2O_3	2·45	2·452	2·45	1·15	14·93	1
FeO	6·50	6·494	6·49	1·43		
MnO	1·09	1·006	1·05	0·23		
CaO	1·72	1·403	1·57	0·46		
MgO	28·30	28·105	28·16	11·26		
H_2O	undet.	undet.	----			
	99·33	98·69	98·97			

This variety of tremolite [bronzite?], it will be seen from the analysis, is distinguished by its large percentage of ferrous oxide and magnesia; and the cause of this interesting peculiarity is to be sought for in the chemical history of the magnetite-bearing serpentine in which the tremolite is imbedded.

18. *Action of light on the development of the young of Frogs.*—M. Thury took the eggs of *Rana temporaria* and placed them all under precisely the same favorable circumstances, except that while part received light through colorless glass, another part received it through green glass. The former developed rapidly, and by the end of May had a length of four centimeters, and well developed hind legs in most of them; while the latter were slowly developed, blackish in color, hardly had a length of two centimeters by the end of May, and were without a trace of the hind legs. By the 10th of June, the former had their fore legs and some were changed to frogs; the latter, still black, had no trace of legs, and breathed almost exclusively by means of their gills. By the 15th of July all the former had become frogs; but those of the latter still had no legs, and by the 2d of August they were all dead without a trace of legs having appeared. Some of the young of the latter lot transferred to the vessel of the former on the 15th of July finished their metamorphosis. At the same time some of the former transferred to the vessel containing the latter continued to develop, showing the influence of the first impulse in their development.—*L'Institut*, Dec. 23, 1874.

19. *Dimorphic Development and Alternation of Generation in the Cladocera.*—Dr. G. O. SARS has discovered a remarkable dimorphism and alternation of generation in *Leptodora hyalina*. (Om en dimorph Udvikling samt Generationsvexel hos *Leptodora*, Forhandlinger Vidensk.-Selsk., Christiania, for 1873, p. 15, and plate.) The development from the ordinary summer-eggs, as already described by E. P. Müller, is without metamorphosis and like that of ordinary Cladocera, the young when excluded from the egg agreeing essentially with the adult; while, according to Sars' observations, the young are excluded from the winter-eggs in a very imperfect condition, quite unlike the known young of

any other Cladocera, and pass through a marked post-embryonal metamorphosis. In the earliest observed stage of the young of this form, the body is obovate, wholly without segmentation, the compound eye wanting, while there is a simple eye between the bases of the antennulæ, the swimming arms (antennæ) well developed, and the six pairs of legs represented only by minute processes projecting scarcely beyond the sides of the body; but the most remarkable feature is the presence of a pair of appendages tipped with cilia and nearly as long as the body, which are evidently homologous with the mandibular palpi of other Crustaceans, although these appendages have always been supposed to be wanting in the species of Cladocera. Two subsequent stages, gradually approaching the adult form, are described. The adults from the winter-eggs have no vestige of the mandibular palpi left, yet the simple eye—which is wholly absent in ordinary individuals developed from summer-eggs—is persistent, and thus marks a distinct generation. Three stages of the young from winter-eggs are beautifully figured upon the plate accompanying the memoir.

This remarkable species has, still more recently, been made the subject of a very elaborate memoir by Prof. Weismann of Freiburg (Uber Bau und Lebenserscheinungen von *Leptodora hyalina*, Zeitschrift für wissensch. Zool., xxiv, Sept., 1874, pp. 349–418, plates 33–38), who, however, had not observed the peculiar development of the winter-eggs. The occurrence of this genus in Lake Superior is noticed in this Journal, vol. vii, p. 161, 1874.

S. I. S.

20. *Development of the European Lobster.*—Dr. Sars has also recently published, in the Proceedings of the same Society for 1874, a paper of 27 pages, illustrated by two autographic plates, on the post-embryonal development of the European lobster (*Homarus vulgaris* Edwards). He describes and figures in detail the three larval stages corresponding precisely with the first three stages which I have described in the American lobster.* Dr. Sars did not receive my papers until after a part of his memoir was printed, so that his investigations were wholly independent. In a short appendix Dr. Sars calls attention to the remarkable agreement in the results at which we had each arrived, and to the excellent opportunity afforded for a careful comparison of the early stages of these two closely allied species. Although the corresponding stages agree so closely in form and structure, they are from the first readily distinguishable by well marked specific differences in the form and armature of the appendages. In fact, the differences appear greater in the larval stages than in the adults. Dr. Sars was not able to trace the development beyond the third stage, which he had at first supposed could not be the last stage of the larva, but after comparison with the later stage of the American lobster he regards it as quite probably the last true larval stage. S. I. S.

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* This Journal, vol. iii, pp. 401–406, plate IX, June, 1872, and Transactions Connecticut Academy, vol. ii, pp. 351–381, plates XIV–XVIII, August, 1873.

21. *Cumacea from the West Indies and the South Atlantic;* by G. O. Sars. 30 pp. 4to, with 6 plates. (From the Svenska Vetenskaps-Akademiens Handlingar, Bandet xi; Stockholm, 1873.)—This memoir, in the same form as the one on the Cumacea of the Josephine Expedition previously noticed, contains minute descriptions and elaborate figures of seven species from the West Indies and from off the mouth of the La Plata. Among them there is a remarkable new genus, *Stephanomma*, in which there is a large central eye upon the front surrounded by a circle of smaller eyes. S. I. S.

22. *Distribution of Insects in New Hampshire;* by Samuel H. Scudder. 50 pp. large 8vo, with 2 maps and a plate. (From vol. i of the Final Report upon the Geology of New Hampshire; Concord, 1874.)—Mr. Scudder first discusses the boundary between the Alleghanian and Canadian faunæ in the State and then the special relations of the alpine and sub-alpine districts of the White Mountains. He makes the Canadian fauna extend to just south of the White Mountains, while the Alleghanian fauna proper occupies only the extreme southern border, the broad intermediate space —about half the area of the State—being regarded as the "common meeting ground" of the two faunæ. These divisions and the alpine and sub-alpine districts upon the mountains are indicated by colored areas on the two maps. This introductory portion is followed by lists of the Butterflies and Orthoptera of the State, with many valuable notes on the distribution of the species, and a full account of two White Mountain butterflies, *Œneis semidea* and *Brenthis Montinus*. S. I. S.

23. *On the Cotton Worm of the Southern States, Aletia argillacea* Hübner, *a moth of the family Noctuidæ;* by A. R. Grote. (Proc. Am. Assoc., 1874.)—After a discussion of the synonymy of the cotton worm family, Mr. Grote discusses as follows the question of the origin of the moth:

"The conclusion to which I have come with regard to the cotton worm is, *that it dies out every year* (*with its food plant*), *that it occurs in the cotton belt of the Southern States, and that its next appearance is the result of immigration.* Testimony is at hand to show that for many years after the cultivation of the cotton plant was introduced into the Southern States, the cotton worm never appeared. The date at which it first appeared in central Alabama has been differently stated to me, but it evidently but little preceded the late war. That the moth is capable of sustaining long and extended flight is readily proven. Professor Packard observed the moth off the coast of the Eastern States, as also Mr. Burgess. I have observed the moth in October in Buffalo, N. Y., as also Dr. Harvey. According to Mr. Riley, the moth has been observed in Chicago, I presume in the fall. It seems that the moth follows the coast-line northward, as also the water courses that empty into the Gulf of Mexico. It is noteworthy here that the watershed of the Ohio and Mississippi extends to within fifty miles of Buffalo. As an example of the prolonged flight of moths, I will state that I have observed in the Gulf

Stream, off the Carolinas and out of sight of land, in the month of August, large numbers of a moth, the *Agrotis annexa* of Treitschke.

Again, I have been struck by the absence of parasitic checks to the cotton worm in the south. I could never discover any, although such may exist. Spreading, as I believe it to do, as a moth, the absence of peculiar parasites to the worm may be reasonably accounted for. I have already and elsewhere pointed out, that in order to make the first brood of the cotton worm the progeny of the so-called "hibernating" individuals (as Professor Riley would suppose), a period of several months has to be accounted for, since these "hibernating" moths could not wait till midsummer to deposit their eggs; and while the cotton is young, and even before it is up, insect life is active, and the weather is warm and other vegetation fully out in the region of the South where I have lived. There is also no reason to believe that the cotton worm ever breeds in the North, and this notwithstanding Professor Riley's suggestions to the contrary, in the Sixth Report before mentioned. The worm never has been noticed on any other plant than the cotton, and in the South perishes by thousands rather than eat any other. The habit of wandering in masses when food fails is a proof of this, as while the worm is supplied with cotton leaf it never quits the plant, transforming to the chrysalis on the stalk which has furnished it nutriment. The wandering habit is not normal but accidental, and the worm is not "gregarious" like the "tent caterpillar." Its "hibernation" with us must also be regarded as accidental, or at least as barren of results. For when spring comes the *Aletia argillacea* has vanished, and is not to be found with the hibernating species of Lepidoptera, renewedly active. And if it *were* found in February and March, it would find no cotton plants upon which to deposit its eggs. If oviposition ever takes place in these months in the cotton belt, the young cotton, free from worms, disproves its efficacy.

It is possible that in the southern portions of Texas, or the Floridian peninsula, the Aletia may sustain itself during the entire year; I have no means of information on this point. My observations are made on its occurrence over the central and principal portions of the cotton belt, and into which I believe it to be imported *de novo* every season that it occurs and from more southern regions.

I conclude, therefore, that while the cotton plant is not indigenous to the Southern States (where it becomes an annual), the cotton-worm moth may be considered not a denizen, but a visitant, brought by various causes to breed in a strange region, and that it naturally dies out with us in the cotton belt, unable to suit itself *as yet* to the altered economy of its food plant and to contend with the changes of the seasons.

When this fact is comprehended, it will simplify the process of artificial extermination by limiting the period during which we can successfully attack the cotton worm, and by doing away with a certain class of proposed remedies.

From the foregoing it will be evident that, 1, The artificial agent employed to destroy the cotton worm must be employed against the first brood as it appears in any given locality during the progression of the moth northward; and, 2, that, in order to be effectual, a concerted action in the application of the remedial agent in any given locality will be found necessary.

I also recommend the introduction of the English sparrow into the Southern States, and additional legal protection to insectivorous birds. Since the war there has been too much ignorant use of the gun on the part of the negroes. *All* the birds should be protected as much as possible, for many species not usually considered insectivorous are yet found, during certain seasons of the year, to live on insects."

III. ASTRONOMY.

1. *The Transit of Venus, Dec.* 8, 1874.—The following additional information respecting the late transit has been received since the publication of the last No. of the Journal. The stations are arranged in the order of latitude:

In the Northern hemisphere.

1. *Vladivostok.* Lat. 43° 7', long. 8h. 47m. E. Prof. Asaph Hall, of the U. S. Naval Observatory, had charge of the American station at Vladivostok. The first contact was pretty well observed, but the haze was so thick as to render it impossible to take photographs at that moment. The time of second contact was noted with accuracy, and some photographs were taken that were tolerably satisfactory. The duration of the transit was 4h. 45m., during which period the haze dispersed three times, allowing the photographers to get some excellent pictures. The time of third contact was pretty accurately noted, but at the moment of fourth contact Venus was entirely invisible. Thirteen photographs were taken and the exact time of each was noted. Some of these are well defined, but others are faint and of uncertain value.
2. *Pekin.* Lat. 39° 54', long. 7h. 46m. E. At the French station the four contacts were observed as follows: first 21h. 32m. 42s.; second 22h.; third 1h. 50m. 15s.; fourth 2h. 17m. 13s. Also sixty good photographs were taken.
3. *Yokohama.* Lat. 35° 36', long. 9h. 19m. E. Russian station. Two contacts were observed, and sixty pictures taken. The Mexican party at Yokohama observed all the contacts and took a large number of pictures.
4. *Beyrout.* Lat. 33° 49', long. 2h. 21m. E. Observations successful.
5. *Nagasaki.* Lat. 32° 45', long. 8h. 39m. E. The American observations at this station were under the direction of George Davidson of the U. S. Coast Survey. The first contact could not be satisfactorily observed on account of clouds. The second contact was well observed. The separation of the limbs was then observed, until Venus had advanced one diameter, and then measures of the diameter of the planet were made. The third contact could not be satisfactorily observed on account of clouds, and at the time of fourth contact the sun was entirely obscured by clouds. The meridian transit of the sun's first limb was observed over nine threads, the first limb of Venus over eight threads, the second limb of Venus over eight threads, and the second limb of the sun over six threads. The difference of declination of the upper limb of the sun and both limbs of Venus was measured by eighteen micrometer readings. The first and second contacts occurred about 1m. 45s. later than the American Almanac computations, and about 3m. 30s. later than the English computations. The third contact occurred near the time of the American computations.

6. *Calcutta.* Lat. 22° 35′, long. 5h. 54m. E. Observations excellent.
7. *Maddapore.* Lat. 12° 48′ (?), long. 5h. 6m. E. Italian station. The four contacts were all observed.
8. *Colombo.* Lat. 7° 0′, long. 5h. 20m. E. All the contacts observed except the first.

In the Southern hemisphere.

1. *Rodrigues.* Lat. 19° 4′, long. 4h. 14m. E. Ingress and egress were well observed, and 58 photographs taken.
2. *Mauritius.* Lat. 20° 20′, long. 3h. 51m. E. Station of Lord Lindsay. All the contacts were observed, except the first, and 110 good photographs were taken.
3. *Reunion.* Lat. 20° 51′, long. 3h. 42m. E. Dutch station. Third contact observed and a few photographs taken.
4. *New Caledonia.* Lat. 21°, long. 11h. E. French station. Second contact observed and 100 good photographs taken.
5. *Cape Town.* Lat. 33° 55′, long 1h. 13m. E. Fourteen photographs taken.
6. *Queenstown, Otago.* Lat. 45° 50′, long. 11h. 20m. E. American station. The observations at this station were under the direction of Prof. C. H. F. Peters of Hamilton College. The sky was overcast until two minutes before the first contact, when the sun shone out and continued bright for nearly two hours. The distance of Venus from the sun's limb was repeatedly measured, as also the apparent diameter of Venus. During the latter part of the transit, floating clouds interfered, but 239 photographs were taken.

E. L.

2. *Observations on the Transit of Venus at Nagasaki;* by Professor Davidson. (Extract from a letter by Mrs. Davidson,* dated Nagasaki, Dec. 10th, 1874.)— . . . The preceding night was clear and beautiful until daybreak, when clouds began rapidly to form, breaking away again about 8½^h and again clouding over by 9½^h. * * I was to record for my husband, who seemed calm and in good spirits notwithstanding the doubtful weather. We were all at our posts of duty by ten o'clock, and as the time drew near you can imagine our suspense. In my husband's observatory (that of the large equatorial), just before the computing time, the sun seemed to be breaking through the clouds, and all was in readiness. George, our eldest boy, holding the chronometers up to his father's eye and ear, and I (seated where I could see my husband's face), with book and pencil in hand, with closed doors and perfect stillness, save the regular beats of the clock and chronometer. It was almost a solemn moment. The sun broke forth with one gleam. I was almost startled to my feet with the shout of "*commence*" given by my husband, as a warning to the photographers that the instant was about to arrive. In a few seconds he gave an exclamation of delight, and the first contact was accomplished and duly recorded. Observations were kept up until the next critical moment, of the second contact—the sun growing less bright but still bright enough for observations. The second contact was seen and, further observations made as the body was passing over the sun. Clouds grew thicker, leaving scarcely a hope for the third contact and also for the fourth, which were not visible, and then the whole thing was over. * * * The exact spot on the sun's limb where the contact should appear was only known by

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* We are indebted for this letter to Mr. Daniel B. Smith of Germantown, Pa.

computation from previous data; and under so large a magnifying power, which took in about 5½ diameters of Venus, an error of one minute of an arc would have been fatal. Mr. Davidson had gone over his calculations several times, and that same morning had revised them again to satisfy himself, then pointed his instruments, and sure enough there came Venus right in the center of his pointing, 3½ minutes later than the English computed time, and 1½ earlier than the American time.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Canadian Scientific Research in* 1874.—Mr. James Richardson (of the Geological Survey) spent the months of May, June and July in a topographical and geological examination of the inlets on the coast of British Columbia, between the 52d and 55th degrees of north latitude. * * *

Mr. George M. Dawson, geologist and botanist to the Boundary Commission, has been engaged in continuing the examination of the region in the vicinity of the 49th parallel. The work of this season extended from Woody Mountain, which is about 400 miles west of the Red River, to the watershed of the continent in the Rocky Mountains. The part of the journey from Red River to Woody Mountain lay over ground already explored in 1873, and but little of geological interest was met with. Woody Mountain, so-called, is situated on the northern edge of the plateau of the Lignite Tertiary, and from the existence of groves of poplars in the valleys, has been chosen by the half-breeds as a base for their hunting and trading expeditions. South of Woody Mountain, on the boundary line, excellent sections illustrating the junction of the Cretaceous and Lignite Tertiary were found, in which the base of the latter formation was well defined. Denudation acting on the little consolidated clays of the Tertiary, has converted a portion of the region where it occurs into *Bad Lands*, which, though perhaps not so rugged as those of Southern Dakota, are characterized by barren clay hills and gorges with scarcely a trace of verdure, and are sufficiently forbidding. Westward from Woody Mountain the Cretaceous rocks cover the greatest area, but those of the base of the Tertiary frequently appear resting on them, and almost always produce a more or less well marked plateau, the general aspect of the country being comparable to that of a cameo, the depressed parts of which are based on the dark Cretaceous clays. The Cretaceous rocks in some places yielded numerous well-preserved fossils enclosed in ferruginous or calcareous nodules, the play of color due to the original pearly matter of the shell being in many cases still apparent. The horizon indicated by most of these is that of the Fort Pierre Group or Cretaceous No. 4 of Meek and Hayden. . . .

The field work of the Boundary Commission is now over, the line having been run and properly marked from the Lake of the

Woods to the Watershed of the Mountains, where it joins that previously surveyed from the Pacific coast. The maps and detailed reports in the various departments are in course of preparation.

Professor Bell has been again engaged during the past summer in the Northwest Territories. The region more particularly explored was the high grounds extending along the western sides of Lakes Manitoba and Winnipegosis and comprising the Riding, Duck and Porcupine Mountains. The valleys of the Swan River and of the Upper Assinniboine were carefully examined, as were also those of some of their tributaries. Mr. Bell and his assistants likewise visited portions of the shore of all the lakes of the Winnipeg basin.—J. F. W. *Montreal Gazette*, Jan. 15.

2. *British Arctic Expedition.*—The scientific part of the forthcoming Polar Expedition will not fail through want of advice and instruction, for while the Geographical Society are preparing their promised Manual of Geography and Physical Geography, the Royal Society are getting ready a Manual of Physical and Natural Sciences. Thus the explorers may prepare themselves for observations on magnetism, on meteorology, on the tides, which in the Arctic zone are peculiar, on geology and botany, on natural history, particularly that part of it which includes the minor forms of marine life; and last, though not least, ethnology is to be attended to, as opportunity may offer. This is a good scheme; almost too good, for opportunities can hardly be other than rare, especially in the ethnic system, in the frozen and desolate regions around the Pole. The Admiralty have relaxed their original intention to appoint none but naval men as scientific observers, and the Council of the Royal Society have recommended for appointment as naturalists to the Expedition, Capt. Feilden, R.A. (at present in Malta) and Mr. Chichester Hart, of Dublin. Hence we may conclude that in the persons of these two gentlemen botany and zoology are provided for. Meanwhile the naval preparations are actively carried on; the two vessels selected for the Expedition, the Alert and the Bloodhound, are being strengthened with all possible dispatch, and the victualling department is busy in cooking and concentrating provisions of the best kind into the smallest possible space.—*Athenæum, Jan.* 30.

3. *Introduction to the Resources of Tennessee;* by J. B. Killebrew, A.M., assisted by J. M. Safford, Ph.D., M.D., and others, being the First and Second Reports of the State Bureau of Agriculture; with five maps, one geologically colored. 1193 pp. 12mo. Nashville, 1874.—The State of Tennessee has an unusually wide range of climates, elevations, geological formations and vegetable productions. The Report now before us is therefore of more than usual interest, and gives the distinctive characters of a much larger area than its title would imply. On the east its border mountains have the same characters as adjoining portions of North Carolina, Virginia and Georgia; its great valley graduates upward into Virginia and downward into Georgia; the coal and iron-ore resources of the slopes of the Cumberland

Mountains and the mild equable climate of the summit plateau are continuous with those of West Virginia, Kentucky and Alabama; its central basin has the same rich "blue-grass" soil as that of Kentucky; and its western border includes a large area of the Mississippi bottoms. The work opens with a brief description of the Topographical Features and Natural Divisions, Climate and Geological Formations, followed by chapters upon Farm-Geology, Soils, Timber, Farm Products, Grasses, Live Stock, Dairy Products, Grapes, Honey, Coal, Iron, Copper, Transportation, Schools, etc., and these by others giving the details of each county separately. The volume makes a grand exhibit of the resources of the State, and will probably many times repay the cost of its publication, through the population and capital which it will be the means of attracting thither.

To make this Report quite complete it should be supplemented by full details as to the geological structure of each county, such as would be obtained in a careful geological survey. It is much to be desired that the survey, begun and so well carried forward by Professor Safford for several years before the war, should be revived on a larger scale and finished.

4. *The Microscope and its Revelations*; by WM. B. CARPENTER. Fifth edition, with 25 plates and 449 wood-cuts. Philadelphia, 1875. (Lindsay & Blakiston.)—This is an enlarged and revised edition of Dr. Carpenter's well known work on the microscope. Considerable additions have been made to the chapters relating to microscopes and the accessory apparatus. But more numerous and perhaps more important additions have been made to the portions relating to the structure of the lower forms of animals and plants, especially the Foraminifera, Bathybius, Coccoliths, Bacteria, Diatoms, etc., including many of the recent discoveries made in connection with the deep-sea dredgings. V.

5. *Mt. Katahdin.*—Through a very careful barometrical determination in August, 1874, Prof. N. C. Fernald has ascertained the height of Mount Katahdin above mean tide at Bangor, Maine, to be 5,215·5 feet, with the probable error not exceeding 4·2 feet.

OBITUARY.

D'OMALIUS D'HALLOY.—This eminent Belgian geologist, an active member of the Geological Society of France, died at Brussels, on the 15th of January, aged nearly ninety-two years.

SIR CHARLES LYELL.—Sir Charles Lyell died on the 22nd of February, aged seventy-eight years.

Annual Report upon the Survey of the Northern and Northeastern States, in charge of C. B. Comstock, Major of Engineers, Bvt. Brig. General, U. S. A., being Appendix CC to the Annual Report of the Chief of Engineers for 1874. 78 pp. 8vo. Washington, 1874.

Report of Progress of the Geological Survey of Canada, for 1873–74, Alfred C. Selwyn, F.R.S. 270 pp. 8vo. Montreal, 1874.

APPENDIX.

ART. XXVI.—*Notice of New Tertiary Mammals*, IV; by O. C. MARSH.

THE remains described in the present communication include some new forms of Quadrumana from the Eocene and Miocene; a species of the new order *Tillodontia*, recently established by the writer; the first horned Rhinoceroses found in this country; a new genus of the *Brontotheridæ;* and a number of other extinct mammals from the Tertiary of the Rocky Mountain region and the Pacific coast. The specimens described are all preserved in the Museum of Yale College.

Lemuravus distans, gen. et sp. nov.

The first announcement of the order *Primates* from the Tertiary of this country was published by the writer, Oct. 8th, 1872, and subsequently appeared in this Journal, (vol. v, p. 405, Nov., 1872). In this paper, three genera of the *Limnotheridæ*, viz: *Limnotherium*, *Thinolestes* and *Telmatolestes*, previously described by the writer, were shown to belong to the lower Quadrumana, the principal parts of the skeleton being very similar to those of Lemurs, while the jaws were somewhat like those of Marmosets. The number of teeth was stated to be greater than in any known forms of the order. Subsequent researches have fully confirmed this determination, and many new facts may now be added in regard to the characters and affinities of this well marked group. From numerous specimens, the writer has ascertained that the *Limnotheridæ* should be placed in the *Prosimiæ*. The brain was nearly smooth, and the cerebellum large, and placed mainly behind the cerebrum. The orbits are open behind, and the lachrymal foramen is outside the orbit.

In addition to the genera mentioned above, an examination of the type specimens of *Notharctos*, *Hipposyus*, *Microsyops*, and *Palæacodon* of Leidy shows that they are true Primates, and probably all belong to the *Limnotheridæ*. To these may be added *Mesacodon*, *Bathrodon*, and *Antiacodon*, described by the writer.*

The genus *Lemuravus*, here described, is nearly related to *Hyopsodus* Leidy. The latter proves on investigation to belong to the *Primates*, and not to the Ungulates. This is shown by the close correspondence of the skeleton with that of the Lemurs, and by the general structure of the skull. *Hyopsodus* and the present genus represent a distinct family, which may be called *Lemuravidæ*. The type genus, *Lemuravus*, has 44 teeth, indicating the most generalized form of the order. *Hyopsodus* has apparently but 42. In the former, the teeth form a continuous series above and below. The canines are small, and the upper incisors are not separated on the median line, as in Lemurs. The molar teeth appear to be essentially the same as those of *Hyopsodus*, but as the latter are only known with certainty from the lower jaw first described there may be important differences. The symphysis of the lower jaw is completely coössified. The brain was nearly smooth, and of moderate size. The skeleton most resembles that of the Lemurs. The humerus has at its distal end a supracondylar foramen, and a supratrochlear perforation. The radius and ulna are distinct. The femur has a small pit in the head for the round ligament. Its distal end is more flattened antero-posteriorly than in the Lemurs. The tibia and fibula are separate. The astragalus is very similar to that of *Lemur*.

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* *Antiacodon nanus* Marsh was redescribed by Cope, several months later, under the name *Anaptomorphus æmulus*. *Limnotherium affine* Marsh was likewise redescribed by the same author as *Tomitherium rostratum* Cope.

Measurements.

Space occupied by entire upper dental series,	30· mm.
Extent of upper molar series,	21·5
Extent of three upper true molars,	11·
Extent of three upper incisors,	5·5
Extent of lower molar series,	23·
Extent of three lower true molars,	12·5
Diameter of head of femur,	5·
Transverse diameter of distal end of femur,	10·
Transverse diameter of proximal end of tibia,	9·
Transverse diameter of distal end,	5·5
Length of astragalus,	7·5

The present species was about the size of the largest squirrels. The type specimen was found in 1871, in the Lower Eocene of Wyoming, by Mr. T. G. Peck of the Yale party.

Laopithecus robustus, gen. et sp. nov.

Among the interesting specimens obtained by the writer during his late expedition to the "Bad Lands" in Nebraska was the lower jaw of a monkey, the first of the order found in that region. The specimen is well preserved, and indicates an animal about as large as a Coati. The crowns of the molar teeth agree essentially with those of some South American monkeys, but still more nearly with those of the Eocene *Limnotheridæ*. From that family the present genus may readily be distinguished by the first true molar, which is the largest of the series, much larger than the penultimate. The last lower molar is smaller than the others. In the first and second true molars, the external cusps are slightly in advance of the corresponding inner ones. The anterior pair are higher and nearer together than those behind. A low ridge extends obliquely from the base of the anterior inner cone to the summit of the outer posterior cusp. The inner posterior cusp is smaller than the others, and separated from them. The crowns are bounded by a distinct basal ridge, except on the inner side. The enamel of the molars is rugose.

Measurements.

Space occupied by three lower true molars,	17·mm·
Antero-posterior diameter of first lower molar,	7·
Transverse diameter,	6·
Height of crown,	5·5
Antero-posterior diameter of penultimate molar,	5·5
Transverse diameter,	5·
Height of crown,	4·2
Depth of jaw below first lower molar,	12·6

This specimen was found in the *Oreodon* horizon of the Miocene "Bad Lands," about thirty miles south of the Black Hills.

Tillotherium fodiens, sp. nov.

Since this genus was proposed by the writer (this Journal, v, p. 485, June, 1873), much light has been thrown upon its affinities by additional remains. It proves to be quite distinct from *Anchippodus* (*Trogosus*) Leidy, although nearly related. The latter genus, unfortunately, is known only from portions of the lower jaw, but this shows marked differences from *Tillotherium*, which lacks the inner pair of small lower incisors, and has an incisor and a canine between the large scalpriform tooth and the first lower premolar. *Tillotherium* has 34 teeth in its permanent dentition (p. 221), and the molar teeth most resemble those in Ungulates. The upper true molars are similar to the premolars of some Eocene Perissodactyls, but are somewhat like the tubercular molars of the *Canidæ*. The lower molar series is of the *Palæotherium* type, and the last lower molar has a well developed posterior lobe.

In the present species, the canines were small, and the superior ones placed somewhat behind the premaxillary suture. The five digits on each foot were all well developed, and of moderate length. The metapodial bones are similar to those in *Ursus Americanus*, but the ungual phalanges preserved are more oblique, and less pointed at the extremity.

Measurements.

Length of skull, from front of incisors to end of occipital condyles,	355· mm.
Extent of entire upper dental series,	186·
Extent of upper molar series,	98·
Extent of three true molars,	59·
Antero-posterior diameter of penultimate upper molar,	20·5
Transverse diameter (greatest),	35·
Antero-posterior diameter of last upper premolar,	12·
Transverse diameter,	24·
Antero-posterior diameter of base of gliriform upper incisor,	22·
Transverse diameter,	16·5
Distance between bases of upper canines,	35·
Extent of lower dental series,	162·
Extent of last three lower molars,	70·
Extent of entire lower molar series,	93·
Antero-posterior diameter of lower gliriform incisor,	21·
Transverse diameter,	15·
Transverse diameter of condyle of lower jaw,	52·
Length of radius,	170·
Transverse diameter of humerus at distal end,	76·
Transverse diameter of tibia at proximal end,	59·
Length of calcaneum,	72·
Length of first metacarpal,	40·
Length of second metatarsal,	46·
Length of ungual phalanx,	35·
Width of articular face,	12·5
Vertical diameter,	14·

The remains here described indicate an animal about two-thirds as large as a Tapir. They are from the *Dinoceras* beds of the Eocene of Wyoming. *Anchippodus minor* Marsh (*Trogosus castoridens* Leidy) is from a lower horizon of the same formation. Both belong to the order *Tillodontia.*

Diceratherium armatum, gen. et sp. nov.

The present genus is of special interest, as it includes the first extinct rhinoceroses with horns found in America. It is an interesting fact, likewise, that these had each a pair of horns placed transversely, as in modern Ruminants, although the discovery of *Dinoceras* and *Brontotherium* has rendered this feature less unexpected. The existence of these horns is clearly indicated by large osseous protuberances on the anterior portion of the nasal bones. The latter are massive and firmly coössified, evidently to support well developed horns. The remainder of the skull, and the teeth, as well as the skeleton, so far as known, resemble the corresponding parts in *Aceratherium.* The dental formula appears to be,

$$\text{Incisors } \frac{1}{2}, \text{ canines } \frac{0}{0}, \text{ premolars } \frac{4}{3}, \text{ molars } \frac{3}{3}.$$

In the present species the skull is of moderate length. The horn-cores are oval in outline, and placed directly opposite each other on the free portion of the nasals, a short distance back from the extremity. They are directed upward and outward, and their surface is rugose. The orbit is small, and there is a prominent postorbital process on the frontal. The premaxillaries are slender, and each supports a single incisor, with a compressed crown. The molar teeth are large, and without cement. The posterior nares terminate in front of the penultimate upper molar. There is a large rounded tubercle on the supraoccipital, just above the foramen magnum. The bones of the limbs preserved indicate that there were four digits in the manus, and three in the pes.

Measurements.

Distance from front of first premolar to end of occipital condyles,	456·mm.
Distance from front of orbit to anterior narial opening,	133·
Extent of upper molar series,	256·
Extent of upper premolar series,	121·
Antero-posterior diameter of last upper molar,	41·
Transverse diameter,	50·
Antero posterior diameter of penultimate upper molar,	49·
Transverse diameter,	55·
Antero-posterior diameter of last upper premolar,	35·
Transverse diameter,	50·
Antero-posterior diameter of first upper premolar,	25·
Transverse diameter,	25·
Width of palate between first upper premolars,	43·
Width between penultimate upper molars,	69·
Length of third metacarpal,	190·
Width of proximal end,	50·
Length of first phalanx of third digit of manus,	31·
Transverse diameter of unciform,	60·
Vertical diameter,	45·

The known remains of the present species indicate an animal about two-thirds the size of the Indian Rhinoceros. They are from the Miocene beds, near the John Day River, in Eastern Oregon.

Diceratherium nanum, sp. nov.

A second species of the above genus is indicated by the greater part of a skull and teeth, and some other remains. These specimens pertained to an animal scarely more than one-half the bulk of that last described. The horn-cores are more compressed, and the extremity of the nasals in front of them is pointed. The anterior narial opening is large. The premaxillaries are slender and compressed. They do not extend so far forward as the nasals.

Measurements.

Distance (approximate) from front of first premolar to postorbital process,	155·mm·
Distance from end of premaxillary to first upper premolar,	91·
Antero-posterior diameter of upper incisor,	25·
Transverse diameter,	11·
Antero-posterior diameter of first lower premolar,	24·
Transverse diameter,	14·
Antero-posterior diameter of second premolar,	26·
Tranverse diameter,	18·

The geological horizon and locality are essentially the same as in the last species.

Diceratherium advenum, sp. nov.

A species clearly belonging to the Rhinoceros family, and possibly to the same genus as the preceding, is indicated by a few fragmentary remains, which are of interest from their geological horizon and locality. The most perfect of these specimens is a last upper molar, and the corresponding lower molar. The former has a wide sigmoid valley between high and nearly parallel transverse crests. There is a strong anterior and posterior basal ridge, but only a faint trace on the inner side near the bottom of the valley. The enamel of the outer and posterior faces especially is marked by delicate vertical striæ. The lower molar has a high narrow crown. The enamel is rugosely striate.

Measurements.

Greatest antero-posterior diameter of last upper molar,	40·mm·
Transverse diameter (approximate),	42·
Depth of transverse valley,	19·
Antero-posterior diameter of last lower molar,	40·
Transverse diameter,	19·
Height of unworn posterior crest,	22·

The known remains of this species pertained to an animal half the bulk of the Indian Rhinoceros. The main interest attached to them is the fact that they were found with upper Eocene fossils, in Utah, and are the first indications of this group in that region. Possibly the strata containing these fossils may in part prove to be lower Miocene.

A comparison of the Lophiodont genus *Hyrachyus* with the Miocene Rhinoceroses, especially *Hyracodon*, seems to point to the former as the Lower Eocene representative or ancestor of the latter group. The skull and teeth of *Hyrachyus* are so similar to those of *Hyracodon* that only slight changes are necessary to transform one into the other. The skeletons, too, are much alike, but *Hyracodon* has only a rudiment of the fifth metacarpal, and hence the line of descent for the four-toed forms was probably a different one.

Brontotheridæ.

During a recent expedition to the "Bad Lands" of Dakota, the writer secured a large number of specimens belonging to the *Brontotheridæ*, most of them in good preservation. From the Miocene of Colorado, explored by the writer in 1870, and subsequently, a large amount of similar material has been obtained, so that at the present time the Yale Museum contains portions of more than 100 different individuals of this family. A study of these specimens, in connection with the types originally discribed, promises to leave but few points in doubt in regard to the structure or affinities of the group. The results will be published at an early day, but a few are given here, which may clear up some of the existing confusion about the different genera.

It may now be regarded as established that all the species of the *Brontotheridæ* had horns, and there is no reasonable doubt that these were common to both sexes. The osseous horn-cores in each species varied much in size and shape with difference of age, and probably of sex. The incisors are small, and in old specimens are frequently lost.

There appear to be four well marked genera in this family now known, which may be distinguished as follows:

1. *Titanotherium* Leidy (*Menodus* Pomel.)

Dentition $=$Incisors $\frac{2}{2}$; canines $\frac{1}{1}$; premolars $\frac{4}{4?}$; molars $\frac{3}{3}$. Diastema behind upper canines. Basal ridge on inner side of upper premolars not continuous. Nasals short. A postorbital process. Third trochanter rudimentary or wanting. Type *T. Proutii* Leidy.

2. *Megacerops* Leidy. (*Megaceratops* Cope), (*Symborodon* Cope in part.)

Dentition $=$Incisors $\frac{2}{0}$; canines $\frac{1}{1}$; premolars $\frac{4}{3}$; molars $\frac{3}{3}$. Diastema behind upper canine. Inner basal ridge on upper premolars not continuous. Nasals more elongated. A postorbital process. Third trochanter rudimentary or wanting. Type *Megacerops coloradensis* Leidy.

3. *Brontotherium* Marsh. (*Symborodon* Cope, in part.) (*Miobasileus* Cope.)

Dentition $=$Incisors $\frac{2}{2}$; canines $\frac{1}{1}$; premolars $\frac{4}{3}$; molars $\frac{3}{3}$. No superior diastema. Strong continuous basal ridge on inner side of upper premolars. No postorbital process. Third trochanter distinct. Type *B. gigas* Marsh.

4. *Anisacodon* Marsh, gen. nov.

Dentition = Incisors $\frac{0}{1}$; canines $\frac{1}{1}$; premolars $\frac{4}{3}$; molars $\frac{3}{3}$. No superior diastema. Strong inner basal ridge on upper premolars. Last upper molar with two inner cones. No postorbital process. Type *A. montanus* Marsh.

Anisacodon montanus, sp. nov.

This species is especially distinguished by the emargination of the extremity of the nasals; the short premaxillaries; and the rectangular form of the last upper molar. The inner posterior cone of this molar is smaller than the one in front, and quite distinct from the posterior basal ridge.

Measurements.

Extension of premaxillaries in front of canines, ...	15·mm.
Distance from end of premaxillaries to narial aperture, ..	76·
Width of nasals above end of premaxillaries, ...	95·
Antero-posterior diameter of last upper premolar, ...	43·
Transverse diameter, ...	63·
Antero-posterior diameter of penultimate upper molar, ..	77·
Transverse diameter, ...	85·
Antero-posterior diameter of last upper molar, ...	84·
Transverse diameter, ...	88·

The specimen here described was found by the writer in November last, in the Miocene of northern Nebraska.

Diplacodon elatus, gen. et sp. nov.

The genus here established presents characters in some respects intermediate between *Limnohyus* and *Brontotherium*. It agrees with the former in its complete dentition (44 teeth), and in the general form of the incisors, canines, and true molars. It resembles the latter still more closely in the premolar and molar teeth, and parts of the skeleton, especially in the vertebræ, and bones of the extremities. From the Eocene *Limnohydiæ*, already described, this genus is sharply distinguished by the last upper premolar, which has two distinct inner cones, thus agreeing essentially with the first true molar. This character, which has suggested the name of the genus, is one step toward the modern type of Perissodactyl dentition. The dental formula of the genus is the same as *Limnohyus*, viz:

Incisors $\frac{3}{3}$, canines $\frac{1}{1}$, premolars $\frac{4}{4}$, molars $\frac{3}{3}$.

In other respects the teeth most resemble those of the *Bron-*

totheridæ. From this family, *Diplacodon* differs widely in its dentition, and the absence of horns.

The cervical vertebræ are short, and opisthocœlous. The radius and ulna, and tibia and fibula, are distinct, and the feet show well marked Perissodactyl characters. There were four digits in front, and apparently three behind.

In the present species, the incisors are all well developed, and those in the lower jaw are directed forward. The canines are large, and have long curved crowns with pointed extremities. The first three upper premolars have the two inner cones connate. The upper true molars are surprisingly like those of *Brontotherium.*

Measurements.

Extent of upper molar series,	242·mm·
Extent of upper true molars,	152·
Antero-posterior diameter of first upper premolar,	14·
Antero-posterior diameter of second upper premolar,	21·
Transverse diameter,	23·
Antero-posterior diameter of fourth upper premolar,	28·
Transverse diameter,	34·
Antero-posterior diameter of first upper true molar,	42·
Transverse diameter,	57·
Antero-posterior diameter of second upper molar,	52·
Transverse diameter,	57·
Antero-posterior diameter of last upper molar,	60·
Transverse diameter,	59·
Width of palate between posterior molars,	92·
Distance between bases of canines of lower jaw (2d specimen)	46·
Antero-posterior diameter of canine, at base,	32·
Transverse diameter,	28·
Height of crown,	27·
Antero-posterior diameter of first lower premolar,	17·
Transverse diameter,	10·
Antero-posterior diameter of second premolar,	26·
Transverse diameter, in front,	15·
Antero-posterior diameter of third premolar,	28·
Transverse diameter, in front,	17·
Transverse diameter, posteriorly,	20·
Length of median cervical vertebra,	45·
Transverse diameter of anterior articular face,	60·
Vertical diameter,	63·

The remains here described belonged to an animal nearly as large as a rhinoceros. They are from the Upper Eocene beds of Utah.

Orohippus Uintensis, sp. nov.

The present species is the largest of the genus, and in some respects indicates a transition between the lower Eocene species and the allied forms in the Miocene. It agrees with the known species of the genus in the number and general structure of the teeth, and in the absence of the posterior intermediate lobe of the upper molars, and especially in the presence of the fifth digit in the manus. It differs in the much deeper transverse valleys of the upper molars, and in their wider crowns. The outer faces of the external cusps of the upper molars have a strong crest extending from the basal ridge to the apex.

Measurements.

Antero-posterior diameter of penultimate upper molar,	9· mm.
Greatest transverse diameter,	12·
Extent of lower molar series,	48·
Extent of lower premolar series,	28·
Antero-posterior diameter of last lower premolar,	8·5
Transverse diameter,	6·
Depth of jaw below third lower premolar,	14·

This species occurs in the upper Eocene deposits of Utah.

Mesohippus, gen. nov.

This genus presents characters intermediate between *Orohippus** Marsh, and *Anchitherium* von Meyer. The skull and teeth are very similar to those of the latter genus, and the dental formula is the same. In the feet, however, the lateral digits are larger; the fifth metacarpal is represented by an elongated splint bone; and the second and third cuneiform bones of the pes are not coossified. The type of the genus is *Mesohippus Bairdi*, =*Anchitherium Bairdi* Leidy. *Mesohippus celer*, =*Anchitherium celer* Marsh, is a smaller species. Both are from the Miocene.

Thinohyus lentus, gen. et sp. nov.

This genus is nearly related to *Dicotyles*, and apparently represents an earlier form of the same type. This is shown in the similar structure of the skull, and form of the teeth. The most noteworthy differences seen in the remains under description are, an additional premolar in the lower jaw, and the extension of the posterior nares between the last upper molars. The orbit is not enclosed behind, and there is no antorbital fossa. The brain was small—less than one-half the size of that of a *Dicotyles* of the same bulk—and much convoluted. There is a strong bony tentorial ridge. The molar teeth have the principal cusps more isolated than in *Dicotyles*, and the intermediate lobes larger.

In the present species the temporal fossæ are separated above only by a narrow ridge. The auditory bullæ are large, and oval in outline. The nasal bones are broad posteriorly. The postorbital process on the frontal is longer than in *Dicotyles*, and more pointed. There is a strong cingulum on the upper molars, excepting on the base of the inner cones.

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* In several recent publications, Prof. Cope has referred the genus *Orohippus* to *Hipposyus* Leidy. The two, however, as shown by a comparison of the type specimens, have no affinity, the latter belonging to the Quadrumana.

Measurements.

Distance from fronto-nasal suture to inion, on median line,	97·mm·
Distance between orbits, over frontals,	64·
Expanse of zygomatic arches,	95·
Extent of last three upper molars,	43·
Antero-posterior of first upper true molar,	14·
Transverse diameter,	14·
Antero-posterior diameter of second molar,	16·
Transverse diameter,	14·
Width between auditory bullæ,	9·
Antero-posterior diameter of auditory bulla,	24·
Transverse diameter,	18·
Length of symphysis of lower jaw (second specimen),	54·
Distance between lower canines,	13·
Space between lower canines and first premolar,	8·
Space between first and second lower premolars,	10·

The present species was somewhat larger than the *Dicotyles torquatus*. The remains here described are from the Miocene, of the John Day River, in Oregon.

Thinohyus socialis, sp. nov.

A second species of the same genus, about half as large as the preceding, is indicated by some fragmentary remains, and particularly by some portions of upper jaws with teeth in excellent preservation. In the present species, the last upper molar is proportionately narrower, and the intermediate lobes of all the upper molars are less developed. In other respects the teeth agree closely with those of the last species. The enamel of the upper molars is somewhat rugose, and there is a distinct basal ridge, except on the inner side.

Measurements.

Antero-posterior diameter of last upper molar,	12·mm·
Transverse diameter through anterior cones,	10·
Transverse diameter through posterior cones,	8·
Antero-posterior diameter of second upper molar,	12·
Transverse diameter, in front,	12·
Height of crown,	6·5

The type specimen of this species was found in November, 1871, in the Miocene beds of Oregon, by Mr. F. Mead, Jr., of the Yale party.

Eporeodon, gen. nov.

Among the species now placed in the genus *Oreodon* of Leidy there are two well marked genera which may readily be distinguished by the base of the skull, and apparently by other characters. In the form first described, of which *Oreodon Culbertsoni* Leidy may be considered the type, there is no indication of an auditory bulla, and for this group the name *Oreodon* may be retained. The other genus, which has a large auditory bulla, may be named *Eporeodon*. The type species is *Eporeodon occidentalis* = *Oreodon occidentalis* Marsh, from the Miocene of Oregon. The other known species of this genus are the following: *Eporeodon superbus*, = *Oreodon superbus* Leidy; *Eporeodon major*, = *Oreodon major* Leidy; and *Eporeodon bullatus*, = *Oreodon bullatus* Leidy. These species, so far as observed, occur in a somewhat different horizon of the Miocene, from the true *Oreodons*. They are, moreover, of larger size, and to this the proposed name refers.

Agriochœrus pumilus, sp. nov.

A number of specimens of a selenodont Artiodactyl, from the upper Eocene, agree so nearly with the known remains of *Agriochœrus* Leidy, that the species they represent may provisionally be placed in that genus. They indicate an animal less than one-half the size of the species already described. The teeth preserved agree in structure essentially with those of *A. latifrons* Leidy. The temporal fossæ were separated only by a sharp crest. Nothing has been known hitherto of the skeleton of this genus, but fortunately some of the more important bones were found with the teeth of the present species. These show that the feet are of the true Artiodactyl type, and somewhat resemble those of *Oreodon*. The tibia and fibula are distinct. The navicular and cuboid are separate. The metapodial bones are not united, and the second and fifth were present.

Measurements.

Extent of last three lower molars,	32· mm.
Antero-posterior diameter of penultimate lower molar,	10·
Transverse diameter,	8·
Transverse diameter of humerus at distal end,	24·
Transverse diameter of articular face,	17·
Least vertical diameter,	11·5
Transverse diameter at distal end,	16·
Antero-posterior diameter,	15·
Extent of three upper true molars (second specimen),	32·
Antero-posterior diameter of first upper molar,	9·5
Transverse diameter,	11·
Antero-posterior diameter of second upper molar,	11·
Transverse diameter,	13·

The present species was about three-fourths the size of the Collared Peccary (*Dicotyles torquatus*). The specimens described are from the upper Eocene of Utah.

Yale College, New Haven, Feb. 20, 1875.

THE

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[THIRD SERIES.]

Art. XXVII.—*The History of Young's Discovery of his Theory of Colors;* by Alfred M. Mayer.

The object of this communication is twofold: I desire first to give complete abstracts from the writings of Newton, Young and Wollaston, in order to put the student of science in possession of all of the early literature of Young's celebrated theory of colors. In the second place, I propose to trace the curious history of the steps by which Young was led to the final adoption of what is now known as Young's theory of color-sensation. In accomplishing the first of these objects, I shall, at the same time, attempt to show, 1st, that Young first formed an hypothesis similar to that known as Brewster's; that is, he selected red, yellow and blue as the three simple color-sensations; 2d, that he subsequently modified his hypothesis and adopted red, green and violet as the three elementary color-sensations, showing that up to the date of this change of opinion all of his ideas on the subject were hypothetical, and not based on any observations or experiments of his own or of others; 3d, that this change of opinion as to the three elementary colors was made on the basis of a misconception by Wollaston of the nature of his celebrated observation of the dark lines in the solar spectrum, and also on the basis of an erroneous observation made by Young in repeating Wollaston's experiment; 4th, that Young subsequently tested his hypothesis of color-sensation and found that it was in accord with facts reached by experiment; and that these experiments then vindicated his hypothesis and raised it to the dignity of a theory.*

Before discussing the subject proper of this article, it may be well to give the reader a clear conception of Young's theory of color, and to show in what high estimation it is at present held by men of science. This can best be done by the reading of the following short extracts from Helmholtz's "Physiological Optics" and from his "Popular Scientific Lectures."

"To speak of three fundamental colors in an objective sense would be nonsense; in fact, as long as one refers only to purely physical conditions, and while there is no reference to the human eye, the properties of compound light depend alone on the proportions in which exist lights of different wave-lengths. The reduction of light to three fundamental colors can never have anything else than a subjective signification; it consists simply in reducing all *color-sensations* to three fundamental sensations. It is in this sense that Young understood the problem, and his hypothesis gives, in fact, an exceedingly clear and simple explanation and summary of all of the phenomena found in the physiological study of colors. Young states that:

"'1. There exist in the eye three kinds of nerve fibers whose excitation respectively gives the sensation of red, of green, and of violet.

"'2. Homogeneous light excites the three kinds of nerve fibers with an intensity which varies with its wave-length.' That which possesses the greatest length of wave excites most powerfully the fibers sensitive to red, that which has an average wave-length excites the nerves sensitive to green, while that light formed of the shortest waves acts on the fibers which give the violet sensation. Nevertheless we cannot deny, but rather should admit for the explanation of numerous phenomena, that each color of the spectrum excites all three kinds of nerve fibers, but with different intensities. Imagine the colors of the spectrum arranged horizontally in going from the red, R, to the violet, V, as shown at the base of figure 1. The three curves will then represent more or less exactly the degrees of irritability of the three kinds of nerve fibers (1, the red; 2, the green; 3, the violet) for the various colors of the spectrum.

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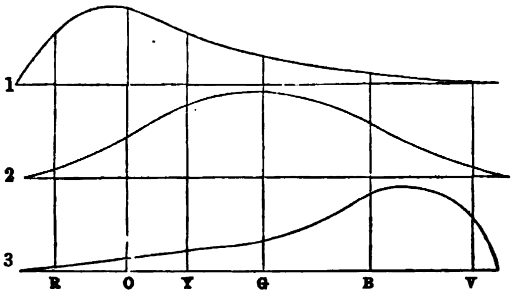
* These two terms, *hypothesis* and *theory*, are so generally misunderstood and thoughtlessly used that it may be well here to give two concise definitions; the first is by Flourens; the second is by Prof. J. Henry.

"An *hypothesis* is the explanation of facts by *possible causes*; a *theory* is the explanation of facts by *real causes*."

"A supposition or guess thus made from analogy as to the nature of the law of a class of facts, is usually called an *hypothesis* and sometimes the *antecedent probability*. When an hypothesis of this kind has been extended and verified, or, in other words, when it has become an exact expression of the law of a class of facts, it is then called a *theory*."

"Pure *red* excites strongly the fibers sensitive to red, and feebly the two other kinds of fibers; sensation, red.

1.



"Pure *yellow* excites moderately the fibers sensitive to red and to green, feebly those fibers sensitive to violet; sensation, yellow.

"Pure *green* excites strongly the fibers sensitive to green, feebly those sensitive to red and to violet; sensation, green.

"Pure *blue* excites in a moderate degree those fibers sensitive to green and to violet, feebly those sensitive to red; sensation, blue.

"Pure *violet* excites strongly those fibers specially destined to receive this sensation, and the other fibers are feebly affected by this light; sensation, violet.

"The nearly equal excitation of all of the fibers will give the sensation of white, or of whitish colors."

* * * * "The choice of the three fundamental colors is to some extent arbitrary. We can choose at will any three colors whose mixture produces white. *Young no doubt was guided by the consideration that the extreme colors of the spectrum occupied the privileged positions.** If we do not choose these colors, we must take for one of the colors a purple tint, and the curve which responds to it in the figure will have two maxima: one in the red, the other in the violet. The hypothesis, without being an impossible one, will be more complicated. As far as I know of, there exists no means of determining directly the fundamental colors but the examination of persons affected with color blindness. We will subsequently see how far that examination confirms the hypothesis of Young, at least so far as the red is concerned."

* * * "In general, then, light which consists of undulations of different wave-lengths produces different impressions upon our eye, namely, those of different colors. But the number of hues which we can recognize is much smaller than that of the various possible combinations of rays with different wave-lengths which external objects can convey to our eyes. The

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***The writer has italicized the above for the purpose of a future reference to it.**

retina cannot distinguish between the white which is produced by the union of scarlet and bluish-green light, and that which is composed of yellowish-green and violet, or of yellow and ultramarine blue, or of red, green and violet, or of all the colors of the spectrum united. All these combinations *appear* identically as white; and yet from *a physical point of view* they are very different. In fact, the only resemblance between the several combinations just mentioned is, that they are indistinguishable to the human eye. For instance, a surface illuminated with red and bluish-green light would come out black in a photograph; while another lighted with yellowish-green and violet would appear very bright, although both surfaces alike seem to the eye to be simply white.

* * * "Other colors, also, especially when they are not strongly pronounced, may, like pure white light, be composed of very different mixtures, and yet appear indistinguishable to the eye, while in every other property, physical or chemical, they are entirely distinct.

* * * "The theory of colors, with all these marvelous and complicated relations, was a riddle which Goethe in vain attempted to solve; nor were we physicists and physiologists more successful. I include myself in the number; for I long toiled at the task, without getting any nearer my object, until I at last discovered that a wonderfully simple solution had been discovered at the beginning of this century, and had been in print ever since for any one to read who chose. This solution was found and published by the same Thomas Young who first showed the right method of arriving at the interpretation of Egyptian hieroglyphics. He was one of the most acute men who ever lived, but had the misfortune to be too far in advance of his contemporaries. They looked on him with astonishment, but could not follow his bold speculations, and thus a mass of his most important thoughts remained buried and forgotten in 'The Transactions of the Royal Society,' until a later generation by slow degrees arrived at the rediscovery of his discoveries, and came to appreciate the force of his argument and the accuracy of his conclusions."

The first publication by Young of his theory of color appeared in a Bakerian Lecture entitled, "On the Theory of Light and Colors," which Young read before the Royal Society on Nov. 12, 1801. In the opening part of this lecture he says: "A more extensive examination of Newton's various writings has shown me that he was, in reality, the first who suggested such a theory [*the undulatory theory of light*], as I shall endeavor to maintain; that his own opinions varied less from this theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found in a similar form in his own works; and this by no less a mathematician than Leonard Euler, whose system of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from Newton, Hooke, Huyghens and Malebranche.

"Those who are attached, as they may be with the greatest justice, to every doctrine which is stamped with the Newtonian approbation, will probably be disposed to bestow on these considerations so much the more of their attention as they shall appear to coincide more nearly with Newton's opinion. For this reason, after having briefly stated each particular position of my theory, I shall collect, from Newton's various writings, such passages as seem to be the most favorable to its admission; and although I shall quote some papers which may be thought to have been partly retracted at the publication of the optics, yet I shall borrow nothing from them that can be supposed to militate against his maturer judgment."

The fact that Young, the *founder* of the undulatory theory of light, in this Bakerian Lecture, in which it has been said that he laid the foundations of that doctrine, should set forth his views in a series of postulates followed by citations from the writings of Newton, to give them weight and proof, may justly surprise those who have trusted to the second-hand information derived from carelessly-complied text books and from hastily-prepared popular lectures. But then, where would be the pugilistic charm of the popular lecturer on the undulatory theory of light, if Newton, his champion, the violent defender of the emanation cause, should decline to enter as a contestant?

Under the heading of Hypothesis III, of this paper, we first meet Young's theory of color-sensation.

"Hypothesis III. *The Sensation of different Colours depends on the different frequency of Vibrations, excited by Light in the Retina.*

Passages from Newton.

"The objector's hypothesis, as to the fundamental part of it, is not against me. That fundamental supposition is, that the parts of bodies, when briskly agitated, do excite vibrations in the ether, which are propagated every way from those bodies in straight lines, and cause a sensation of light, by beating and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound, by beating against the organs of hearing. Now, the most free and natural application of this hypothesis to the solution of phenomena I take to be this: that the agitated parts of bodies, according to their several sizes, figures and motions, do excite vibrations in the ether of various depths or bignesses, which being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white color; but if by any means those of unequal bigness be separated from one another, the largest beget a sensation of a red color, the least or shortest of a deep violet, and the intermediate ones of intermediate colors; much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air, of various bignesses, which, according to those bignesses, make several tones in a sound; that the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with least refraction; whence the vibrations of several bignesses, that is, the rays of several colors, which are blended together in light, must be parted from one another by refraction, and so cause the phenomena of prisms and other refracting substances; and that it depends upon the thickness of a thin transparent plate or bubble whether a vibration shall be reflected at its further superficies, or transmitted; so that, according to the number of vibrations interceding the two superficies, they may be reflected or transmitted for many thicknesses. And, since the vibrations which make blue and violet are supposed shorter than those which make red and yellow, they must be reflected at a less thickness of the plate: which is sufficient to explicate all the ordinary phenomena of those plates or bubbles, and also of all natural bodies whose parts are like so many fragments of such plates. These seem to be most plain, genuine, and necessary conditions of this hypothesis. And they agree so justly with my theory, that if the animadversor think fit to apply them, he need not, on that account, apprehend a divorce from it. But yet, how he will defend it from other difficulties, I know not." (Phil. Trans., vii, 5088; Abr. I, 145, Nov., 1672.)

"To explain colors, I suppose, that as bodies of various sizes, densities, or sensations, do, by percussion or other action, excite sounds of various tones, and consequently vibrations on the air of different bigness; so the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the ether—of various bigness; the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bigness, strength, or power; and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerve into the sensorium; and there, I suppose, affect the sense with various colors, according to their bigness and mixture; the biggest with the strongest colors, reds and yellows; the least with the weakest, blues and violets; the middle with green; and a confusion of all with white, much after the manner that, in the sense of hearing, nature makes use of aerial vibrations of several bignesses, to generate sounds of divers tones; for the analogy of nature is to be observed." (Birch, iii, 262, Dec., 1675.)

"Considering the lastingness of the motions excited in the bottom of the eye by light, are they not of a vibrating nature? Do not the most refrangible rays excite the shortest vibrations, —the least refrangible the largest? May not the harmony and discord of colors arise from the proportions of the vibrations propagated through the fibers of the optic nerve into the brain, as the harmony and discord of sound arise from the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.

After these quotations from Newton, Young brings out his hypothesis of color-sensations, under the following:

"*Scholium.* Since, for the reason here assigned by Newton, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited; for instance, *to the three principal colors, red, yellow and blue,* of which the undulations are related in magnitude nearly as the numbers 8, 7 and 6; and that each of the particles is capable of being put in motion, less or more forcibly, by undulations differing less or more from a perfect unison; for instance, the undulations of green light, being nearly in the ratio of 6½, will affect equally the particles in unison with yellow and blue, and produce the same effect as a light composed of those two species; and each sensative filament of the nerve may consist of three portions, one for each principal color."

An attentive perusal of the above quotation will show that Young's hypothesis imagines each sensitive point of the retina to contain *particles* capable of vibrating in perfect unison to those vibrations causing three principal colors (red, yellow and blue, in this the first publication of his *hypothesis*), "and that each of the particles is capable of being put in motion, less or more forcibly, by undulations differing less or more from a perfect unison." This would suppose such a triple *molecular* constitution of each nerve fibril as to cause the three species of its constituent molecules (or the atoms forming the molecules) to be *in tune* with the three rates of vibration corresponding respectively to the undulations of the ether causing red, yellow and blue. He afterward says: "and each sensitive filament of the nerve may consist of three portions, one for each principal color." We have here a conception of the *mode of action* of an ætherial vibration on the retinal nerve fibrils which has not been described by those who have given accounts of Young's theory of color-sensation. Before reading the celebrated Bakerian Lecture, the only knowledge I had directly obtained of Young's theory was from the reading of the account of it as published in vol. i, page 439, of his "Lectures on Natural Philosophy and the Mechanical Arts," London, 1807. This account, however, contains no mention of the physiological part of his theory, and last October I published in the American Journal of Science my paper No 6 of Researches in Acoustics, in which I expressed similar views to those just quoted from Young, as follows:

"For, has modern histology given us any facts concerning the structure of the human retina which point to the establishment of Young's hypothesis of three distinct sets of retinal nerve terminations? The more we study the minute structure of the retinal rods and cones, the farther appears to remove an understanding of the mode of operation of the sensory apparatus of the eye. May not research in this direction be guided by the hypothesis that the *molecular* constitution of the retinal rods and cones is such that their molecules are severally tuned to the vibrations corresponding to the colors red, green and violet? This would lead us to look for effects of actinism on the retina as showing the link existing between the transmitting and sensory functions of the eye. Do not the facts of the known persistence of chemical action, after it has been once initiated, and the time which would be required for the retinal molecules to recombine, or rearrange themselves, after the ætherial vibrations had ceased, comport with the known durations of the residual visual sensations, and with the main facts of physiological optics, better than the hypothesis that masses of the retinal elements are set in vibration, rather than their molecules?"

It requires no argument, it is evident, that the statements made by Young in the foregoing paper, concerning his color hypothesis, were entirely hypothetical, not having been based on any observation or experiment either of his own or of others.

The next publication by Young on his theory of color takes place in the following year, and is contained in the following short paragraph, incidentally written toward the conclusion of a paper read by him before the Royal Society, on July 1, 1802, and entitled "An account of some cases of the production of colours, not hitherto described."

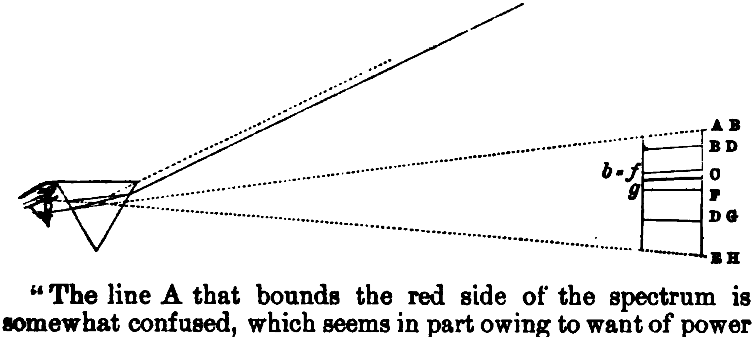
"In consequence of Dr. Wollaston's correction of the description of the prismatic spectrum, compared with these observations, it becomes necessary to modify the supposition that I advanced in the last Bakerian lecture, respecting the proportions of the sympathetic fibers of the retina; substituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6."

It thus appears that Young changed his three elementary color-sensations from red, yellow, and blue, to *red*, *green*, and *violet*, "in consequence of Dr. Wollaston's correction of the description of the prismatic spectrum." In order to understand fully the ground for this modification of his hypothesis one will be obliged to read the following abstract from Dr. Wollaston's paper, published in "The Transactions of the Royal Society" for 1802; and we are pleased to detain the reader with this paper because it contains the first publication of the observation of those dark lines to which modern spectroscopic research has given such important and prominent significance.

"I cannot conclude these observations on dispersion without remarking that the colours into which a beam of white light is separable by refraction, appear to me to be neither 7, as they usually are seen in the rainbow, nor reducible by any means (that I can find) to 3, as some persons have conceived; but that, by employing a very narrow pencil of light, four primary divisions of the prismatic spectrum may be seen, with a degree of distinctness that, I believe, has not been described nor observed before.

"If a beam of daylight be admitted into a dark room by a crevice $\frac{1}{20}$ of an inch broad, and received by the eye at the distance of 10 to 12 feet through a prism of flint glass, *free from veins*, held near the eye, the beam is seen to be separated into the four following colours only, red, yellowish green, blue and violet; in the proportions represented in fig. 2.

2.



"The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet. But C, the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark lines, *f* and *g*, either of which, in an imperfect experiment, might be mistaken for the boundary of these colours.

"The position of the prism in which the colours are most clearly divided is when the incident light makes about equal angles with two of its sides. I then found that the spaces A B, B C, C D, D E, occupied by them, were nearly as the numbers 1 6, 2 3, 3 6, 2 5."

In the light of the subsequent careful examinations of the spectrum made in 1814–15 by the celebrated optician Fraunhofer of Munich, we can ascertain what Wollaston really saw. Wollaston, in short, only observed *imperfectly* the dark lines of the spectrum, now known as Fraunhofer's lines, but he *imagined* he saw a spectrum so pure, that is, formed by such a degree of dispersion, that it became divided into four distinct and separated "primary divisions." He at once inferred, and erroneously, that Newton's analysis of the sun's light was false; that no *orange* or *yellow* exists in the spectrum, but between the red and the blue there exists only a "yellowish green." Further on we shall see how Young made a similar but even greater error in his description of this observation. I imagine that when Wollaston's sharp eye caught the glimpse of the divided spectrum, he naturally thought he saw in those divisions uniform colors. It was a natural mistake, and only too readily made, by reason of his mind imagining that the dark lines were *the dividing lines* of the pure simple colors of the solar spectrum.

In the figure illustrating Wollaston's observation, I have placed alongside of his lines A, B, *f*, *g*, D, and E, the corresponding Fraunhofer letters B, D, *b*, F, G, H, thus giving the reader a clear idea of what Wollaston really saw. Wollaston's line "B, between the red and the green," and which he says "in a certain position of the prism is perfectly distinct," we at once recognize as the dark solar line D, occupying a position between the orange and yellow, and well know to be caused by the reversal of the bright yellow light of sodium vapor. No one, however, could now say, after an examination of the spectrum as observed by Wollaston, that the line D divides the red from the green. Wollaston also calls his D and E lines (the G and H lines of Fraunhofer) as "the two limits of the violet;" we now know that G is really on the indigo and that H is within the limits of the violet.

For a more satisfactory comparison of the colors of the solar spectrum as observed by Wollaston and Fraunhofer, I give below the following table. Fraunhofer's results are taken from his colored figure of the spectrum. Both spectra are from flint glass, and their lengths are supposed divided into 360 equal parts.

	Fraunhofer.	Wollaston.	
Red,	56	57·6	Red.
Orange,	27		
Yellow,	27		
Green,	46	82·8	Yellowish green.
Blue,	48	129·6	Blue.
Indigo,	47		
Violet,	109	90	Violet.
	360	360	

Fraunhofer's observations are irreproachable, and are to this day in high esteem for their accuracy. They were made by placing the prism in front of a telescope, mounted on a divided horizontal circle; and viewing a distant slit through the prism and telescope, he observed spectra as pure as those given by modern spectroscopes of low power. Fraunhofer discerns orange and yellow and green where Wollaston only sees yellowish green. Also, Wollaston did not see all of the violet, as we might suspect from his having bounded its upper limits by the line H. Fraunhofer saw 109 parts of violet, Wollaston only 90.

The above discussion, I think, has clearly shown that Wollaston made a false interpretation of his observation in supposing that he had discovered a pure spectrum naturally divided by dark lines into four simple colors, and that he also erred in the proportions which he relatively gave to them. Also, I have shown that Young, in finally selecting red, green and violet as the three elementary color-sensations, was not, as Helmholtz states, guided in their choice "by the consideration that the extreme colors of the spectrum occupied the privileged positions," but selected those colors on hearing of Wollaston's supposed complete analysis of the sun's light into red, greenish blue and violet colors, separated from each other in the spectrum by dark spaces.

We hear no more from Young about his theory of colors until 1807, when he published the first volume of his celebrated work, "A Course of Lectures on Natural Philosophy and the Mechanical Arts."* On page 439 *et seq.* of this work Young gives a concise statement of his views on the analysis of the sensations of color, and supports these views with conclusive experiments with rotating colored discs; but, strange to say, he omits from this account of his theory all mention of the physiological explanation of it which he gave in the Bakerian Lecture of 1801. The following extracts from the Natural Philosophy give all that it contains on the theory of colors. The italics are our own.

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* "We have heard it remarked," says Dean Peacock in his Life of Young, "that no writer, on any branch of science which the lectures treat of, can safely neglect to consult them, so rich is the mine of knowledge which they contain; and it is a well-known fact, that many important propositions and discoveries have been more or less clearly indicated in them, which have only been recognized or pointed out when other philosophers discovered them independently, or announced them as their own."

"It has generally been supposed, since the time of Newton, that when the rays of light are separated as completely as possible by means of refraction, they exhibit seven varieties of colour, related to each other with respect to the extent that they occupy, in ratios nearly analagous to those of the ascending scale of the minor mode in music. The observations were, however, imperfect, and the analogy was wholly imaginary. Dr. Wollaston has determined the division of the colored image or spectrum in a much more accurate manner than had been done before: by looking through a prism, at a narrow line of light, he produces a more effectual separation of the colors than can be obtained by the common method of throwing the sun's image on a wall. The spectrum formed in this manner *consists of four colors only, red, green, blue and violet,* which occupy spaces in the proportion of 16, 23, 36 and 25, respectively, making together 100 for the whole length; the red being nearly one-sixth, the green and the violet each about one-fourth, and the blue more than one-third of the length. *The colors differ scarcely at all in quality within their respective limits, but they vary in brightness;* the greatest intensity of light being in that part of the green which is nearest the red. *A narrow line of yellow is generally visible at the limit of the red and green,* but its breadth scarcely exceeds that of the aperture by which the light is admitted, and *Dr. Wollaston attributes it to the mixture of the red with the green light.* There are also several dark lines crossing the spectrum within the blue portion and in its neighborhood, in which the continuity of the light seems to be interrupted. This distribution of the spectrum Dr. Wollaston has found to be the same, whatever refracting substance may have been employed for its formation; and he attributes the difference, which has sometimes been observed in the proportions, to accidental variations of the obliquity of the rays. The angular extent of the spectrum formed by a prism of crown glass is $\frac{1}{27}$ of the deviation of the red rays; by a prism of flint glass $\frac{1}{19}$." Fig. 3.*

Fig. 3. "The spectrum produced by looking through a prism at a narrow line of light.

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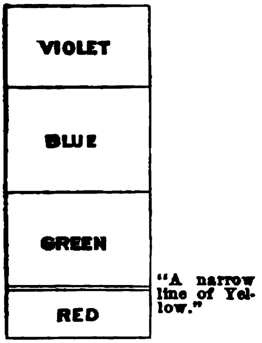
* These figures, 3 and 4, are copied of the exact size of those given by Young in the plates appended to his Natural Philosophy. The descriptions under the figures are those given by Young. The colors in Young's figures we have indicated in type. There are six other figures illustrating Young's account, but we have found it impossible to convey *in print* clear ideas of their tints.

3.



"In light produced by the combustion of terrestrial substances, the spectrum is still more interrupted; thus, the bluish light of the lower part of the flame of a candle is separated by refraction into five parcels of various colours; the light of burning spirits, which appears perfectly blue, is chiefly composed of green and violet rays; and the light of a candle into which salt is thrown abounds with a *pure yellow, inclining to green,* but not separable by refraction. The electric spark furnishes also a light which is differently divided in different circumstances.

"If the breadth of the aperture viewed through a prism is somewhat increased, the space occupied by each variety of light in the spectrum is augmented in the same proportion, and each portion encroaches on the neighboring colours, and is mixed with them; so that the red is succeeded by orange, yellow, and yellowish green, and the blue is mixed on the one side with the green, and on the other with the violet; and it is in this state that the prismatic spectrum is commonly exhibited.

4.

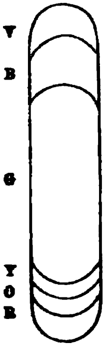


Fig. 4. "The appearance of a circular aperture, moderately large, viewed through a prism.

* * * * * * *

"Sir Isaac Newton observed that the effect of white light on the sense of sight might be imitated by a mixture of colours taken from different parts of the spectrum, notwithstanding the omission of some of the rays naturally belonging to white light. Thus, if we intercept one-half of each of the four principal portions into which the spectrum is divided, the remaining halves will still preserve, when mixed together, the appearance of whiteness; so that it is probable that the different parts of those portions of the spectrum which appear of one colour, have precisely the same effect on the eye. It is certain that the perfect sensations of yellow and of blue are produced respectively by mixtures of red and green, and of green and violet light, and there is reason to suspect that those sensations are always compounded of the separate sensations combined; at least, this supposition simplifies the theory of colours; it may, therefore, be adopted with advantage, until it be found inconsistent with any of the phenomena; and we may consider white light as composed of a mixture of red, green and violet, only, in the proportion of about two parts red, four green, and one violet, with respect to the quantity or intensity of the sensations produced.

"If we mix together, in proper proportions, any substances exhibiting these colours in their greatest purity, and place the mixture in a light sufficiently strong, we obtain the appearance of perfect whiteness; but in a fainter light the mixture is grey, or of that hue which arises from a combination of white and black; black bodies being such as reflect white light, but in a very scanty proportion. For the same reason, green and red substances mixed together usually make rather a brown than a yellow colour, and many yellow colours, when laid on very thickly, or mixed with black, become brown. The sensations of various kinds of light may also be combined in a still more satisfactory manner by painting the surface of a circle with different colours, in any way that may be desired, and causing it to revolve with such rapidity, that the whole may assume the appearance of a single tint, or a combination of tints resulting from the mixture of the colours.

"From three simple sensations, with their combinations, we obtain seven primitive distinctions of colours; but the different proportions in which they may be combined afford a variety of tints beyond all calculation. The three simple sensations being red, green and violet, the three binary combinations are yellow, consisting of red and green; crimson, of red and violet; and blue, of green and violet; and the seventh in order is white light, composed of all three united. But the blue thus produced, by combining the whole of the green and violet rays, is not the blue of the spectrum, for four parts of green and one of violet make a blue differing very little from green; while the blue of the spectrum appears to contain as much violet as green; and it is for this reason that red and blue usually make a purple, deriving its hue from the predominance of violet.

"It would be possible to exhibit at once to the eye the combinations of any three colours in all imaginable varieties. Two of them might be laid down on a revolving surface, in the form of triangles placed in opposite directions, and the third on projections perpendicular to the surface, which, while the eye remained at rest in any one point, obliquely situated, would exhibit more or less of their painted sides, as they passed through their different angular positions: and the only further alteration that could be produced in any of the tints would be derived from the different degrees of light only. The same effect may also be exhibited by mixing the colours in different proportions, by means of the pencil, beginning from three equidistant points as the centers of the respective colours."

On certain portions of the above extracts, which I have italicized, I will venture a few observations. In the first passage, thus indicated, Young says: "The *spectrum formed in this manner consists of four colours only, red, green, blue and violet.*"

Young then replaces Wollaston's "yellowish green" by "*green*," and farther on he adds, "The colours differ scarcely at all in quality within their respective limits, but they vary in brightness; the greatest intensity of light being in that part of the green which is nearest to the red," thus confirming our view that both Young and Wollaston were of the opinion that when a narrow bright crevice is observed through a prism, that the spectrum so viewed consists of only four colors, red, green, blue and violet, "differing scarcely at all in quality within their respective limits;" which limits they supposed naturally existed in the dark spaces which, as they imagined, bounded these elementary colors. Young, however, somewhat modifies this opinion in the next sentence, when he says: "A narrow line of yellow is generally visible at the limit of the red and green, but its breadth scarcely exceeds that of the aperture by which the light is admitted, and Dr. Wollaston attributes it to the mixture of the red with the green light." It would, indeed, appear from the last portion of this sentence that Young obtained directly from Wollaston one of the main facts on which his theory was founded, namely, that yellow can be reproduced by the mixture of red and yellow lights. But Wollaston, in his paper of 1802, from which we have cited, makes no such statement as to the composition of yellow light, and it is therefore probable that Wollaston communicated orally this view of the subject to Young. Every student of optics now knows that the description, already given, of Fraunhofer's observation on the color composition of the spectrum is the correct one; yet the errors of observation of Wollaston and of Young were errors which led to a great discovery, as we shall see on the further examination of the history of this beautiful and comprehensive theory of color.

Farther on in the Natural Philosophy we read that, "The sensations of various kinds of light may also be combined in a still more satisfactory manner by painting the surface of a circle with different colours, in any way that may be desired, and causing it to revolve with such rapidity, that the whole may assume the appearance of a single tint, or of a combination of tints, resulting from the mixture of the colours." These experiments were evidently first made by Young; and are fully described in the text and perfectly illustrated in the colored discs in the plates of Young's work. These experiments have been carefully repeated by Helmholtz, Maxwell and others, and of their general accuracy there is no doubt. We can readily imagine the delight with which Young must have viewed these beautiful experiments, which, however, together with other truths unfolded by him, were destined to remain unnoticed, "until a later generation, by slow degrees, arrived at the *discovery* of his discovery."

It must now recur to the reader to inquire, *when* were made these experiments which first confirmed Young's hypothesis and placed it among the best established truths of optical science; and why it was that Young should for so long a time have been satisfied with a hypothetical statement of his views on the color-sensations, and should have deferred to bring those views to the test of experiment. For reasons already stated, Young, in July, 1802, changed his three elementary color-sensations, red, yellow and blue, to red, green and violet. The experiments with the rotating colored discs were first published in 1807. Young printed the syllabus of his first course of lectures on January 19th, 1802, in a volume of 250 pages. I have not been able to procure a copy of this syllabus, but evidently it does not contain even the corrected statement of his theory of color, for that was based on Wollaston's observation, which appeared subsequently to the syllabus, on June 24th, 1802. It is therefore evident that unless Young made the experiments with the rotating colored discs during the latter part of his course of lectures, he must have made them during the interval between his retirement from the professorship at the Royal Institution and the publication of his lectures on Natural Philosophy in 1807. Young delivered his first lecture before the Royal Institution on January 20th, 1802, and was very busy with his lectures until July 4th, 1803, when he retired. I think that we may fix the date of these remarkable experiments as somewhere between 1803 and 1807, and it is highly probable that the theory was never given to the public in a lecture before the Royal Institution, but first appeared in the publication of his Lectures on Natural Philosophy.

That Young should have delayed to bring to the test of experiment a plausible hypothesis, when other men would at once have appealed to the instruments in their laboratories, is explained by the fact that Young "at no period of his life was fond of repeating experiments or even of originating new ones. He considered that, however necessary to the advancement of science, they demanded a great sacrifice of time; and that, when a fact was once established, that time was better employed in considering the purposes to which it might be applied or the principles which it might tend to elucidate." Indeed, this peculiarity receives abundant confirmation from his own words; thus, in the Bakerian Lecture, already so often referred to, he says, "Nor is it absolutely necessary in this instance (in speaking of the proofs to be adduced in support of the undulatory theory of light) to produce a single new experiment; for of experiments there is already an ample store;" and in a letter written in November, 1827, to his sister-in-law, Mrs. Earle, on the respective honors given by Herschel, in his Optics, to Young and Fresnel, he says: "And acute suggestion was then, and indeed always, more in the line of my ambition than experimental illustration." Young carried his opinion of the secondary importance of experiment so far as even to object to the increase of the fund left by Wollaston to the Royal Society to aid experimental inquiries, in these words: "For my part, it is my pride and pleasure, as far as I am able, to supersede the necessity of experiments, and more especially of expensive ones."

ART. XXVIII.—*A redetermination of the Constants of the Law connecting the Pitch of a Sound with the Duration of its Residual Sensation;* by ALFRED M. MAYER.

IN my "Researches in Acoustics, Paper No. 6," published in this Journal in October, 1874, I gave the result of many experiments on the durations of the residual sonorous sensations, and embodied those determinations in this law:

$$D=\left(\frac{53248}{N+23}+24\right)\cdot 0001,$$

in which D = the duration of the residual sonorous sensation corresponding to N number of vibrations per second.

The precise determination of the durations of the residual sonorous sensations are difficult by reason of the complex character of the sound perceived when the vibrations of a tuning fork are sent intermittently into a resonator by means of a revolving perforated disc; and the difficulty of the determination is increased by the fatigue and deadening of the sensitiveness of the ear produced by the beats which enter it from the resonator.

The important applications that have been made of this law in the physiology of audition, and in the elucidation of the fundamental laws of musical harmony, have made me desire to have my determinations reviewed by ears more highly cultivated than mine in the appreciation of pitch and of musical intervals, and more skilled in the direct analysis of composite sounds into their simple component tones. Since my publication in October last, I have had the good fortune to have elicited in Madame Emma Seiler and in her son, Dr. Carl Seiler, a profound interest in my researches. They have spent considerable time in the redetermination of the durations of the residual sonorous sensations; making use, under my directions, of the same apparatus which I employed in my original experiments. Madame Seiler assisted Helmholtz in the experiments contained in his renowned work on physiological acoustics, and unites to educated musical perceptions a thorough knowledge and appreciation of all recent advances in physiological acoustics. I have, therefore, great confidence in the following results, which I desire my

S	N	D	L
UT$_1$	64	$\frac{1}{25}$ = ·0395 sec.	2·5
UT^2	118	$\frac{1}{45}$ = ·0222 "	2·8
UT$_3$	256	$\frac{1}{70}$ = ·0142 "	3·6
SOL$_3$	384	$\frac{1}{102}$ = ·0098 "	3·7
UT$_4$	512	$\frac{1}{130}$ = ·0076 "	3·9
MI$_4$	640	$\frac{1}{153}$ = ·0065 "	4·1
SOL$_4$	768	$\frac{1}{166}$ = ·0060 "	4·6
UT$_5$	1024	$\frac{1}{180}$ = ·0055 "	5·6

readers to substitute for those contained in the table given on page 244 of vol. viii of this Journal.

From the above data the law given on page 146 becomes

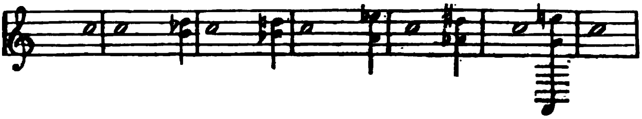
$$D=\frac{3{\cdot}2}{N+31}+{\cdot}0022.$$

The adoption of the law with these new constants requires the following corrections to be applied to my paper:

Page 246, *dele* "The ordinate of MI$_4$," &c., to end of paragraph.
" 249, line 19 from bottom, for $\frac{1}{11}$ read $\frac{1}{20}$.
" " " 17 " " " $\frac{1}{500}$ " $\frac{1}{438}$.
" 250, " 1 " top, " $\frac{1}{10}$ " $\frac{1}{18}$.
" " " 3 " " " " " "
" " " 6 " bottom, " $\frac{1}{227}$ " $\frac{1}{291}$.
" " " 5 " " " $\frac{1}{29}$ " $\frac{1}{53}$.
" " " 3 " " " $\frac{1}{58}$ " $\frac{1}{120}$.

The corrections under "5. *Quantitative applications of the Laws to the fundamental facts of Musical Harmony*," can be readily applied from the law as given above. Suffice it here to say that on the table on page 252 the nearest consonant interval in the octave of C$_1$ is a Fourth+$\frac{2}{3}$ of a semitone; while in the octave of C$_6$ the nearest consonant interval of two simple sounds has contracted to only one tone.

In the opening part of this communication I spoke of the difficulty of the determination of the residual sonorous sensations by reason of the complex character of the sounds perceived when the vibrations of a tuning fork are sent intermittently into a resonator by means of a revolving perforated disc. I will now describe the character of the successive sensations experienced when, starting from rest, we gradually increase the velocity of rotation of the disc until the separate beats blend into a smooth continuous sensation. When the disc is stationary, with one of its openings opposite the mouth of the resonator, it is evident that the ear will experience a simple sonorous sensation when a tuning fork is brought near the mouth of the resonator; on revolving the perforated disc, two additional, or secondary, sounds appear; one slightly above, the other slightly below the pitch of the fork. An increased velocity of rotation causes the two secondary sounds to diverge yet farther from the note of the beating fork, until the velocity reached is so great that the two secondary sounds become separated from each other by a major sixth, while, at the same moment, a resultant sound appears, formed by the union of the sound of the fork with the upper and the lower of the secondary sounds. This resultant is the second octave below the note given by the fork. On further increasing the velocity of the disc, the two secondary sounds and the resultant disappear, and the ear has alone the sensation of the simple sound produced by the beats of the fork; which, at this stage of the experiment, blend into a smooth continuous sensation. These successive and gradual changes, as they happen with a UT_4 fork, we have indicated in steps of semi-tones in the appended musical notation. The sound of the fork is given in the semibreve; the crochets represent the secondary sounds and the resultant sound. In the fourth bar the upper note E flat proceeds to D sharp in the fifth bar. This is so because, in the natural scale, D sharp is higher in pitch than E flat.



In conclusion, I request my readers to transfer the comma from after *Dissonanz* to after *continuirliche*, in the quotation from Helmholtz at the beginning of my paper (vol. viii, p. 241).

ART. XXIX.—*On the Action of the Less Refrangible Rays of Light on Silver Iodide and Bromide;* by M. CAREY LEA, Philadelphia.

IT will be the object of the present investigation to show:

1st. That silver iodide and bromide are sensitive to all the colored rays of the spectrum.

2d. That silver iodide is to all the less refrangible rays more sensitive than silver bromide.

3d. That the theory of M. E. Becquerel as to existence of "exciting rays" and "continuing rays" is not supported by a careful examination of the phenomena in question.

The first of these positions differs from those generally accepted in extending farther to the less refrangible end of the spectrum the sensitiveness of both the compounds in question, especially of silver iodide.

The second of these positions, that in reference to the comparative sensitiveness of AgI and AgBr, differs essentially from the views hitherto accepted, according to which AgBr has been held to be by far the more sensitive to the less refrangible rays. I shall endeavor to show that the contrary is the case.

In these investigations I have confined myself to studying the effects obtained upon the silver compounds as formed in the body of pure paper, applying in all cases the silver solution after that of the alkaline haloid, and immediately washing out the excess of silver nitrate.

The colored light was separated by means of colored glasses. Without wishing to detract from the value of observations made with the prismatic spectrum, I am inclined to believe that those obtained with colored glasses, whose absorption spectra have been carefully and exactly made out with the aid of spectroscopic analysis, are at least equally reliable, and in some cases, certainly more decisive. It is also to be remarked that prismatic solar spectra, as usually obtained, are open to the suspicion of not being absolutely pure, but liable to admixture of rays coming from light falling upon the prism outside of the true image of the slit, by reason of diffraction, and also possibly from other sources. A very careful prismatic analysis of the spectrum itself would be necessary to detect the presence of faint violet light cast by any such means on the red end of the spectrum. With colored glass, on the contrary, the detection of any such admixed color may be accomplished by a simple inspection of its absorption spectrum. If it be suspected, for example, that a given piece of red glass, or any number of pieces, permit the passage of a trace of violet light, the spectroscope gives us at once a decisive answer, because we can project any such violet light that may be present against an absolutely dark field of view, and decide positively on its presence or absence. Another serious difficulty with the spectrum arises from the very long exposure which is necessary to make apparent the effect of the less refrangible rays, and which long exposure can be less advantageously managed with the solar spectrum than with colored glass.

Still another advantage in the use of colored glass is that the pieces used can be of any desired size, so that one can operate over a large space and can simultaneously expose several preparations to identical influences, and thus obtain a more accurate comparison than is possible with the successive exposures necessary with the solar spectrum. By interposing a suitable glass negative between the colored glasses and the paper, the effects of the exposure are rendered much more marked: the distinctness of the reproduction of the image of the negative serves in each case as a measure of the degree of sensitiveness. This has a particular importance when a development process is used, because even with the utmost care, there will sometimes be a discoloration arising from the action of the development bath. And if the arrangements are such that the action of light produces a flat tint only, it may easily become impossible to distinguish between the effects due to the development of an impression of light and those depending upon a spontaneous deposit from the bath; moreover, this spontaneous deposit is most apt to occur, in the case of a very weak impression, exactly where it is most able to induce error. The employment of a negative avoids this difficulty, and if its image has a great variety of tones, the observer is able to measure very closely the relative extent to which it is reproduced on the sensitive surface.

Direct or Developed Images.—A certain amount of ambiguity has been introduced into what has been published on the subject of the sensitiveness of silver salts, by not distinguishing in all cases whether the sensitiveness spoken of relates to images which appear during the exposure, or are subsequently evoked by development. The capacity to form developable images is in all cases the true test of sensitiveness: any other criterion would lead to most erroneous conclusions. In this paper it will always be understood, except when otherwise mentioned, that the sensitiveness referred to is that which is exhibited under a development by means of gallic acid and silver nitrate, controlled by acetic acid.

The papers used were first floated on weak solutions of potassium iodide and bromide respectively, using them in equivalent proportions, so that each might take up an equal quantity of silver from the silver bath. This last was always made of the same strength, viz: 30 grains to the ounce, and a fresh solution was used for every piece, because, as silver iodide and bromide are soluble in solutions of silver nitrate, there might otherwise result a transfer and the formation of appreciable quantities of silver iodobromide, the reactions of which substance are different, and so energetic that the presence of the least trace would destroy all value in the results obtained. For the same reason the potassium salts employed must be pure. Therefore the KI and KBr, which were exclusively used, were submitted to careful examination at the outset. The results subsequently obtained as to sensitiveness of AgI were so different from what has hitherto found acceptance, that it was deemed advisable at the close to submit the KI to a second rigorous examination for bromine. This examination fully confirmed the first conclusions as to the absolute purity of the specimen used.

The papers were next blotted off in clean filtering paper (because if hung up to dry, the lower end becomes more richly charged than the upper), and after complete removal of the surface moisture were floated on the silver nitrate solution. They were next thrown into a large vessel of water, washed with running water and dried. In a few cases specially mentioned, the silver solution was allowed to dry on the paper for comparative experiment.

RED RAYS.

Examination of the Glass.—The ruby glass of commerce differs very much in different specimens; some pieces, though appearing to the eye to be of a strong, pure red, nevertheless transmit a good deal of green light, and show in the spectroscope a strong green band. Other pieces are of a much purer color, but I have never met with any, even the darkest, which did not, with a critical examination, show in their absorption spectrum traces of the more refrangible rays. The band which represents the proper color extends from very near the extreme red end to a point a little beyond the double sodium line D.

When two such pieces are superposed, the trace of more refrangible light is excluded. The proper band of color is also shortened at its more refrangible end, and recedes to the other side of D. Two pieces, a dark red and a medium red, gave as their limit a wave-length λ 594. Two dark pieces, superposed, had a limit of λ 600. A large number of experiments were made with the combination last mentioned. But, as it was my desire to make these investigations, as far as possible, absolutely correct, all my results were revised, and confirmed by final trials with light filtered through *three* thicknesses of ruby glass. These showed as the limiting wave-length of their absorption spectrum, λ 605, a limit which totally excludes the yellow, and represents the red rays with at most a slight admixture of orange, as the light at and near this limit of λ 605 was extremely feeble, and the whole strength of the band found its place beyond. The measures here given were repeated a number of times on different days, with concurrent results.

Result.—With *two red plates*, having a limit of λ 594, a strong, *direct* image was obtained upon the iodide paper by an exposure of 15 minutes to bright sunshine. The bromide paper, exposed under the same glasses and negative, simultaneously, showed nothing. By a very long development, exceedingly faint traces of an image were rendered visible. The contrast between these two was very striking.

With *two stronger red plates*, having a limit of λ 600, and of course a diminished illumination, no trace of an image by development could be obtained on the silver bromide paper, after exposures of various lengths up to an hour and a half of brilliant sunshine (from 12.15 to 1.45 P. M., Jan. 30, 1875, bright sunshine on snow).

On the contrary, silver iodide gave, after 20′ exposure, a developed image showing some detail, and with 40′ exposure, a faint, *direct* image, visible without development.

With *three red plates*, having a limiting wave-length λ 605, there was necessarily a further great diminution of illumination. Nevertheless, silver iodide gave, with 30 minutes' exposure (middle of the day, bright sunshine on snow, Feb. 1, 1875), a faint, and with four hours' exposure, a full image. Feb. 2d, three hours' exposure gave an image showing considerable detail. Feb. 5th, same result. These were of course all developed images.

The corresponding bromide papers, receiving identical exposures under the same glasses, side by side with the iodide, absolutely failed to develop anything. These developments were prolonged for several hours, in order that the faintest traces, if present, might render themselves visible. But in no case did silver bromide, when exposed under the three red glasses, show the faintest trace of any image, even with a four hours' exposure. That silver bromide is not wholly destitute of sensibility to red rays, was shown by the previous experiments; but this sensibility is small, and when the red light is not only very pure, but very faint, it may be exerted for a very long time without result. Still, I believe that with a day's exposure under a strong summer sun, an impression capable of development could be produced. On the other hand, in no case, even with exposure as short as half an hour to a winter sun, did silver iodide fail to give an image, under these three ruby glasses, whose faintest light terminated at λ 605. It appears, therefore, certain that both silver iodide and silver bromide prepared on paper, with excess of silver nitrate removed, are distinctly sensitive to red light, and that silver iodide is, under these conditions, at least ten times as sensitive as silver bromide.

YELLOW RAYS.

It is by far more difficult to isolate yellow rays than either red or green, because almost all media that transmit yellow rays also transmit red, and many also transmit green. The yellow glass that is found in commerce lets through the whole spectrum, except the extreme violet end, and for a time I thought that the isolation of the yellow rays by colored glass would be impracticable. I finally succeeded very well by combining a deep brown glass with a dark green. The brown glass transmitted the yellow and red, absorbing the rest, and the dark green admitted principally green and yellow, cutting off the orange and red.

A spectroscopic analysis gave the following results:

Extreme limit of wave-length at less refrangible end of the spectrum,	λ 638
At more refrangible end,	λ 527
Point of maximum illumination,	λ 570

The limits here given are extreme limits at which the absorption spectrum ended. It was estimated that *at least* nineteen-twentieths of the illumination was pure yellow, with perhaps a very faint admixture of orange, and of the less refrangible green rays, bordering on the yellow (any close observation of the spectrum will show how little pure yellow light it contains).

Result.—Silver iodide showed itself also more sensitive to yellow light than silver bromide, but the difference was greatly less than in the case of red light, and the results were more variable. Feb. 4, 1875, with exposure to bright sunlight from 12 M. to 3 P. M., distinct images were got by development on silver iodide, whilst with four trials of equal length on bromide paper, nothing was got. On the following day, with an exposure of about equal length, but before midday, well marked images were obtained on silver bromide. Those obtained at the same time on silver iodide, exposed side by side, developed to an equal strength in one-third the time.

I conclude, therefore, that silver iodide and bromide are both sensitive to yellow light, and the iodide more so than the bromide.

GREEN RAYS.

Much of the green glass found in commerce admits nearly the whole spectrum except the red rays. There exists, however, a very dark shade of green, which narrows the transmitted band very much. When two such pieces of dark green were superposed, their absorption spectrum was as follows:

Extreme limit toward red end,	λ 601
Extreme limit toward blue end,	λ 488

This last limit corresponds very nearly with the solar line F, whose wave-length is 486, and which approximately separates the green from the less refrangible blue. For exact experiments, however, a narrower band is desirable. Accordingly, three plates were superposed. This involved such a reduction of illumination that the sun could be viewed through the glass without inconvenience. This combination gave the following measurement:

Extreme limit toward red end,	λ 581
Extreme limit toward blue end,	λ 497

Near these limits the light was extremely faint. To ascertain the rays of really effective strength transmitted by this combination of plates, another measurement was taken, resulting as follows:

Effective limit toward red end,.......... ...... λ 569
Effective limit toward blue end, ... λ 517

The portions of the band which lay between λ 569 and λ 581 on the one side, and λ 497 and λ 517 on the other, were so extremely faint as to have no appreciable agency in affecting the result.

It will be observed that even the limit of the faintest rays does not extend as far as the solar line F; the blue is therefore absolutely excluded. Toward the yellow side, even the faintest rays do not extend so far as the sodium double line D and the effective rays terminated at λ 569. The yellow is therefore virtually excluded.

Result.—With two dark green glasses (λ 488–601). A powerful image was developed on silver iodide after three minutes' exposure. On silver bromide the same exposure gave faint traces of an image only. The exposure of an hour and a half failed to produce on silver bromide as strong an impression as did the three minutes on the iodide.

An exposure of forty minutes gave on silver iodide a plain, direct image. With ninety minutes, no direct image was produced on silver bromide. Other exposures and degrees of exposure gave concurrent results.

With three dark green glasses (extreme limit λ 497–581, effective λ 517–569). Silver iodide gave the following results: Two and a half minutes, faint image developed. Six minutes, distinct; fifteen minutes, strong; thirty, very full exposure; three hours and a half, completely overdone.

Silver bromide: two and a half, and six minutes, nothing; fifteen, faint trace; thirty, a little stronger; three and a half hours, moderately strong.

Comparing the above: AgBr with fifteen minutes about the same as AgI with two and a half; AgBr with thirty minutes not quite so strong as AgI with six: AgBr with three and a half hours much the same as AgI with fifteen minutes, if anything less strong. Other trials gave conformable results

The conclusion seems, therefore, irresistible that AgI and AgBr both are sensitive to pure red, yellow and green rays; and that AgI, at least under the conditions of these experiments, show to yellow rays a slightly superior, and to green and red rays a markedly superior sensitiveness.

AgI and AgBr together.—Although the scope of this investigation was directed to the action of pure AgI and pure AgBr, a few experiments were made on the two used together. They were used in equivalent proportions and in such quantity that the paper containing the two salts together should take up exactly as much silver as the papers prepared with AgBr and AgI separately. The result demonstrated a materially greater sensitiveness to both red and green light than with either used separately. The experiment was not extended to the yellow, but it would doubtless have given similar results, as also the more refrangible rays of the spectrum.

[It may be worth mentioning, in passing, that the result of this part of the investigation led to the making of a series of experiments on the introduction of silver iodide into the "emulsion process" for preparing photographic dry plates. The result was that when either equal or equivalent quantities of AgBr and AgI were employed, nothing material was gained, but when the quantity of AgI was much reduced, and especially when AgCl was added, plates were obtained possessing a degree of sensitiveness much beyond anything heretofore attained by any collodion method, either wet or dry. A fully exposed image was obtained by an exposure under a negative to a gas flame for one second. The details necessary to the production of this result have been communicated to the Photographic Society of Philadelphia.]

Sensitivensss to White Light.—Although this investigation was directed to the action of certain colored rays, a few experiments were included upon the comparative action of white light on AgI and AgBr. There was found a moderate superiority of sensitiveness in AgI, though much less marked than in the case of the red and green rays. Exposure under a negative for three seconds to a weak diffused light sufficed to produce a strong latent image on both the AgI and AgBr papers, but most strong upon AgI.

Influence of Free Silver Nitrate.—The presence of free silver nitrate in a film of AgBr considerably increased the capacity to receive a latent image, but its agency in producing a direct image was still more marked. Thus an exposure of ninety minutes under two green glasses (limits λ 488–601) failed to produce any visible image on washed AgBr, whereas when the paper contained free silver nitrate, a plainly visible direct image was obtained in forty minutes.

As respects silver iodide, my opinions have always differed from those which have prevailed among photo-chemists. Many years ago I proved the opinion that silver iodide, absolutely isolated, was insensitive to light, to be erroneous. I covered glass plates (preferably ground glass, for better adhesion) with thin specular films of silver, and then iodized these through and through by means of a solution of iodine, and succeeded without difficulty in developing images received on such films.

The opinion also that silver iodide, in the absence of free silver nitrate, is comparatively insensitive, receives its disproof from the foregoing investigation, which decisively shows that washed films of silver iodide possess a high degree of sensitiveness to white light, and some sensitiveness to the less refrangible rays: in either case a higher degree than silver bromide.

EXCITING AND CONTINUING RAYS.

More than thirty years ago, M. E. Becquerel published his well-known theory of the existence of two classes of rays, the "exciting" and the "continuing" rays. According to it, the more refrangible rays had alone the power of originating an impression; the less refrangible were powerless to commence, but capable of continuing and re-inforcing an impression which had been commenced by the more refrangible rays. This view is still maintained by M. Becquerel in his comprehensive work "La Lumière," published a few years since, and is adopted by Jamin, Ganot and others in their treatises on physics, though the acceptance has been far from universal.

The foregoing results are evidently quite incompatible with this theory, for they prove the existence in the less refrangible rays of the power to impress both silver iodide and bromide. In only one way could this testimony be set aside, namely, by the allegation that these papers might have received, in the course of their preparation, some chance effect of light sufficient to lay the foundation of an impression, and that this was afterward continued only, not originated, by the colored light to which the papers were subsequently exposed.

It would seem to be a sufficient answer, that all due care was taken to avoid this danger, and, as the experiments themselves showed, successfully. Such an uncertainty might exist with experiments made with the spectrum, but not where, as in these investigations, a negative is interposed. For, according to Mr. Becquerel, the preliminary impression of light which is necessary in order to enable the less refrangible rays subsequently to exert their continuing power on AgBr, AgI or AgCl formed on paper, is that which would enable them to receive a development by gallic acid, etc. When this impression has been received the less refrangible rays can continue this commencement, and this result may be obtained either with the spectrum, or with colored glass.*

But as in my experiments the latent images were always developed with gallic acid, it follows that, had the images evoked been due to a continuing power of the colored light upon an original impression caused by light accidentally admitted during the preparation, then the papers must have darkened all over by the agency of gallic acid, instead of developing an image. So that the system of experiment adopted was in itself incidentally a precaution against any such source of error.

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* La Lumière, vol. ii p 76.

Nevertheless, wishing to obtain a result that would be entirely decisive, paper was prepared for this special purpose under conditions of exceptional care. It was prepared at night, using light, not only most carefully guarded by colored glass, but as little of it as was possible to manipulate by. Even this faint light was carefully excluded from the preparations. The paper was covered whilst being sensitized on the silver bath and was then washed with special precaution and dried in absolute darkness. As soon as dry it was placed in the frames and under the glasses under which it was to be exposed next day. The precautions taken enable me to say that most certainly the paper received in its preparation no effect of light capable of influencing a development in any way. Its action proved to be precisely the same as that of the paper prepared in my usual way, thus demonstrating that all the rays of the spectrum possess the power of originating an impression on silver iodide and bromide.

In the foregoing pages, I have briefly given the result of one hundred and sixty experiments The results, with such slight and altogether unimportant variations as necessarily arise from slight differences of preparation and differences in the character of the sun's light, were remarkably concordant, and may be summed up as follows:

1. AgBr and AgI are sensitive to all the visible rays of the spectrum.
2. AgI is more sensitive than AgBr to all the less refrangible rays and also to white light.
3. The sensitiveness of AgBr to the green rays was materially increased by the presence of free silver nitrate.
4. AgBr and AgI together are more sensitive to both the green and the red rays (and probably to all rays) than either AgI or AgBr separately.
5. There do not exist any rays with a special exciting or a special continuing power, but all the colored rays are capable both of commencing and continuing the impression on silver iodide and bromide.

Philadelphia, March 6, 1875.

ART. XXX.—*On the Silurian age of the Southern Appalachians;* by FRANK H. BRADLEY.

1. *Introduction.*

EMMONS, in his American Geology, refers most of the rocks along the western line of North Carolina* to his Taconic system, and says (vol. i, pt. 2, p. 24) that "the locality at the Warm Springs [Madison County, N. C.] is a good exhibition of the development of the Lower Taconic rocks in the Southern States;" but he also states (p. 25) that, in Cherokee County [near Murphy], the system is separated [divided] a few miles, by the interposition of a ridge of primary schists with staurolite.

Safford, in his Geology of Tennessee, 1869 (pp. 177–8), says, of the metamorphic rocks of the eastern border of that State: "A portion of the beds are certainly referable to the Ocoee group: the remainder, although conformable, may be older, and most likely are. * * The question of the greater age of [these] other parts is not so easily settled, and must remain open for the present. I know of no sufficient reason for referring any of these rocks to the Huronian or Laurentian series of Canada." Again (p. 193): After the Ocoee, the Chilhowee sandstones reappear and continue up the [French Broad] river to the State line. * * In North Carolina, a short distance beyond the line, the Ocoee group sets in again, and is the formation to within a mile of the Warm Springs. Then follows a Knox belt. The Springs are located on the Knox dolomyte. * * Beyond the Springs follow Chilhowee sandstones, then Ocoee rocks, and finally gneiss." The Knox group, here referred to, includes the Quebec group and the Calciferous: the Chilhowee sandstone is the typical Potsdam; and the Ocoee is the Lower Potsdam or Acadian group.

Professor Kerr has just issued a new geological map of North Carolina, which shows a strip of "Huronian" along the western line of the State, apparently covering the whole area there assigned by Emmons to the Taconic, even including the Warm Springs region. As the text of his report has not yet appeared, we cannot fairly criticize his non-adoption of the conclusions of Safford, although that geologist had followed the strata so connectedly, from their less altered and more fossiliferous regions, and was therefore least of all liable to be in error.* East of this Huronian is marked a belt of "Laurentian," the southern part of which is included in the area in question. The writer would here express an opinion, long held as a suspicion, but latterly confirmed by the results of the recent examinations of the crystalline rocks by Dana, Irving, Brooks and others, that the typical Huronian strata of the lake region, as well as large areas elsewhere so named, are simply metamorphic Silurian. It is pleasant to know, from correspondence, that some eminent geologists have reached the same conclusion.

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* Since taking residence in Knoxville, in 1869, the writer has had his attention particularly drawn to the local features of the Lower Silurian rocks of East Tennessee and of the metamorphic strata which occupy the adjoining portions of North Carolina and Georgia. Most of the data given in this paper have been gathered incidentally, in connection with trips for the examination of mining properties; though, the clew to the Blue Ridge puzzle having thus been secured, a final trip was made solely for the purpose of completing its solution. Thus much to explain what some will regard as a trespass upon the preserves of the State geologists of North Carolina and Georgia, though neither of these officers has yet been able to make other than very hasty visits to the region in question. Without an opportunity of examining all the literature of the subject, reference can be made only to the works of Emmons, Safford and Kerr.

The writer's conclusion, briefly stated, is that the rocks of that portion of North Carolina south and west of the Little Tennessee, together with the metamorphic area of Georgia, north of a line parallel with and ten miles south of the Chattahoochee (and *probably* that south of this line), and the entire metamorphic area of Alabama, are *Silurian or newer*, with the possible exception of two or three small patches not over ten miles in diameter. To the northeast of the Little Tennessee, there are large areas of rocks of the same age, but of undetermined boundaries, and including some patches which are pretty certainly Archæan. One of these latter forms and surrounds the "Bluff," on the borders of Cocke County, Tenn., and Madison County, N. C., where there are extensive outcrops of protogine and unakyte, with heavy beds of porphyritic hematite. This area—probably of the same age as the iron-bearing rocks of Missouri—adjoins the Warm Springs region before mentioned; and it may be well to say here that in Emmons' figure (l. c.) of the section there exposed, No. 15 is Chilhowee sandstone; 14 and 13 are Ocoee, separated by a fault from 12, which is Knox dolomyte, and whose cavernous structure give vent to the waters warmed by flowing through the deep fissures of the fault to the heated substrata; No. 11 is Knox shale; 10 is Chilhowee sandstone; 9 to 1 are Ocoee, probably resting upon Archæan. If this is "typical Lower Taconic," it is typical Lower Silurian as well. It is perhaps pertinent to note here that Emmons's Upper Taconic (l. c., pp. 62–68) is plainly only the slightly (or not at all) metamorphosed Silurian of the Great Valley.

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* It is but justice to Professor Safford to say here that, during five years of frequent reference to his Report, in connection with field-work upon nearly all parts of the geological series exposed in East Tennessee, the writer, while recognizing many *local* deficiencies and some errors, such as are unavoidable in rapid work, has been constantly gratified and surprised at the thoroughness of the Report in all its *general* features. It has been and continues to be a great loss to Tennessee, as well as to science, that Professor Safford has not been employed to complete the detailed survey of the State.

Before giving further details of observations, it may be well to mention briefly the characters of the unaltered portion of at least the lower part of the series supposed to be represented by the metamorphic strata in question, beginning with the upper layers.

2. *Silurian of East Tennessee.*

Throughout East Tennessee the Cincinnati group is composed chiefly of shales, more or less calcareous, but includes generally two marked beds of limestone—the upper a red, compact, crinoidal marble; the lower, called by Safford the "iron-limestone," a strongly ferruginous rock, and so siliceous as to leave in many cases a tolerably solid skeleton, after its lime has all been removed by percolating waters, though in others, again, it decays entirely to a sandy clay. This lower bed is quarried, near Knoxville, as a flagging-stone, and proves very satisfactory, though its surfaces are quite irregular. Fine specimens of ripple-marks are common on these slabs; and the false-bedding of beach-sands is often well shown upon weathered edges of layers. The lowest beds of this group are generally shales. The total thickness has been estimated by Safford at about 1,850 feet.

The Trenton group is represented by heavy-bedded, red, gray and variegated marbles, of an estimated thickness of 380 feet, certain layers of which are extensively quarried and shipped for both building and monumental purposes.

The Chazy—"Maclurea beds" of Safford—consists of about 500 feet of bluish, impure limestone, generally very shaly, though at some points quite solid. The usual *Maclurea magna* is quite abundant, as are also various undescribed species of Sponges.

This is followed, below, by the Knox group, consisting of three members—a limestone, a shale and a sandstone. The limestone is mainly a very impure dolomyte, though with some purer layers, and includes also two or more thin beds of sandstone, one of which is probably the equivalent of the Saccharoidal sandstone of Missouri. The middle and lower portions are especially siliceous, being commonly crowded with cherty nodules and bands, the layers sometimes showing a thickness of five feet or more of solid chert. These are so numerous that the outcrops of this series are constantly marked by vast quantities of cherty masses, large and small, and more or less disintegrated, according to the varying percentage of included dolomyte. This included mineral is often crystallized, and its removal by weathering leaves numerous rhombohedral cavities, which Safford has noted as peculiar to chert of this horizon. Perhaps, a still more constant mark is the oölitic structure, which is often very perfect throughout large masses. This structure also frequently occurs in the lower layers of the limestone, where the upper graduates into the middle member, or Knox shale. This upper member carries a greater variety of ores than any other bed in all this region; but, as yet, it appears doubtful whether any of them are in sufficient quantity and of such purity as to be of much practical value. Pyrite, galenite and sphalerite are of frequent occurrence in small quantities; and, at a few points, there are indications of considerable bodies of lead and zinc ores, the weathered portions of the latter showing much calamine and smithsonite. Barite is also quite abundant, though often impure. Fluorite also occurs in small quantities. These mainly occur at the filling of cavern-like chambers irregularly following the "joints" of the strata; but little is yet known of their extent, and nothing as to any law of their distribution. They also occur in grains disseminated through the mass of the rock. The most abundant ore along the outcrop of this series is limonite, which occurs in immense beds, both compact and ochreous. It is plainly a surface accumulation, resulting from the decomposition of pyrite and siderite in the limestone, and varying greatly in degree of purity, according to local conditions. Some beds are so filled with larger or smaller masses of chert as to be worthless: while others are almost entirely free from such impurities. Most of this ore contains more or less manganese ore, partly in the form of wad, partly as psilomelane. The very general dissemination of iron through the limestone, as sulphide, as oxyd, or as carbonate, is evidenced by the color of the heavy beds of reddish-brown clay accompanying every outcrop, which have been formed from the impurities of the rock, as the lime and magnesia salts have been dissolved away.*

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* These immense beds of debris, along the outcrops of all but the very hardest and most siliceous rocks, are the constant characteristic of all this southern region, and, with the entire absence of drifted material above the level of the old river-terraces, afford the best of evidence that no glaciers have ever existed in the region. The immense amounts of river gravel, however, which bestrew the slopes of the valleys, up to from 170 to 200 feet and more above the present stream-levels, would indicate vast accumulations of snow upon the higher parts of the mountains; and it is not improbable that traces of glacial action may yet be found about their summits. These deposits indicate that, about Knoxville, the Holston was 170 feet above its present level, and filled a valley at least four miles wide, while its discharge was by the valley of the Coosa River, directly to the Gulf of Mexico, instead of by the present valley of the Tennessee. The Clinch must then have formed the main source of the Tennessee, and possibly this may also have found a southwestern outlet after passing Chattanooga.

The deep disintegration of the rocks will also account for the *entire absence of lakes* in this region, since any body of water, if ever formed here, would find little difficulty in cutting down its outlet so as to drain its bed. The most southern body of water in the Appalachians is said to be at Mountain Lake, Giles County, West Virginia, which is not far from the region, on the Greenbrier River, where Stevens recently reported finding glacial markings. (This Journal, III, vi, 371).

The middle member of the Knox series is the Knox shale, which in the neighborhood of Knoxville is a nearly pure clay shale; but it is said to become more calcareous to the northeast, forming at some points a pretty solid limestone, not readily distinguished from the Knox dolomyte proper. The Knox sandstone is ferruginous and quite commonly very argillaceous and glauconitic. The Knox group, as a whole, represents the Quebec group and the Calciferous sandrock; and the plane of division is not readily determined. The thin bands of limestone at the top of the shale contain numerous trilobites, mostly of species as yet undescribed, which appear to be closely related to those occurring at the base of the Quebec group in Canada. The maximum thicknesses are stated by Safford as: Dolomyte, 4,000 feet; shale, 1,500 to 2,000 feet; sandstone, 800 to 1,000 feet. At some points, the two lower members are very thin or even wanting.

Next comes the Chilhowee sandstone, generally a heavy-bedded rock, occasionally white, generally ferruginous, often pyritous. This, at most of its outcrops, is metamorphosed into an extremely compact quartzyte, though commonly interlaminated with some few thin beds of sandy shale. *Scolithus* borings occur abundantly in this rock at some points; but, instead of this being the universal rule, as stated by Safford, it is the exception at outcrops thus far examined by the writer. Thickness. 2,000 feet or more. As already stated, this is considered to be the typical Potsdam sandstone.

Lowest of all the recognized Silurian, we have the Ocoee group, of the Acadian epoch, or Lower Potsdam. The beds are all more or less metamorphosed, and consist mainly of slates and conglomerates. As these are of very uniform character, and outcrop in the region of greatest disturbance and most numerous faults, it would be extremely difficult, if not impossible, to determine their actual thickness. Safford says that this *may* be 10,000 feet.

While the Chilhowee and Ocoee groups, within the Tennessee line, may especially be considered semi-metamorphic, the sandstones of the former having been well cemented without concealing or even confusing the granular structure, the shales of the latter having been squeezed into smooth slates, but showing no crystallization, and the pebbles of its conglomerates well combined with the finer paste by a sort of aqueous fusion while yet plainly showing their pebbly character; yet lesser degrees of metamorphism are plainly to be traced in many of the higher layers, far out into the Great Valley. Good examples of this are abundant in the "iron-limestone" about Knoxville, where the lines of original stratification are often much contorted, while the mass is most thoroughly compacted.

3. *From Athens to Murphy.*

As was long ago stated by Rogers, the dips, throughout large portions of the Appalachians, are mainly to the southeast, the upward displacement along the numerous lines of faults being generally on their southeastern sides. As a consequence, it is at many places difficult to recognize the successive outcrops of the different beds, until one has become somewhat familiar with their local features and forms of metamorphism. In tracing the section, however, from the railroad at Athens, Tenn., across to Murphy, N. C., and thence to Clarkesville, Ga., there appears to be less than usual of this faulting; and the equivalencies of the beds are determined with comparative ease. At Athens, the shaly, calcareous beds, with some of the heavier and more compact ones of the iron-limestone, of the Cincinnati group, form the hills, with moderate northwesterly dips. About two miles southeast, these are suddenly cut off by a fault, the Knox dolomyte abutting against them, though still showing northwesterly dips. Two miles farther on, at the crossing of Middle Creek, we pass an anticlinal, along whose back the erosion has cut slightly into the Knox shale. Knox dolomyte follows, with southeast dips, until, about two miles farther, at Jesse Dodson's, another anticlinal of the shale appears, after which regular southeast dips bring in the complete series for about four miles, the light-colored Trenton marbles appearing about nine miles out, and ten miles bringing us into a gap of a line of high red knobs formed of the iron-limestone beds. These include many thin streaks of hematite, partly in the compact "specular" form, partly oölitic, partly in a powdery or scaly condition, which has been locally mistaken for cinnabar. The hard ore can sometimes be traced, within short distances, from the "specular" condition into the unaltered oölitic "dyestone," inclosing corals and other fossils in perfect preservation, or again into a ferruginous sandstone. The ores of this belt have not yet been found in sufficiently thick beds to pay for mining, though many carloads of good ore have been gathered from the accumulations along the weathered outcrops. These iron-limestone beds recur, in several successive waves, for about four miles, one of the synclinals bending low enough to show a considerable mass of the overlying "red marble." Finally, thirteen miles out, the layers rise in sharp northwest dips, as we approach the foot of the southwestern section of the Chilhowee Mountain range. The Knox group forms the base and northwestern face of the mountain; and the heavy beds of the Chilhowee sandstone form its crest, in bold cliffs overlooking the valley to the southeast, 1,100 feet below. White Cliff Springs, of chalybeate and sulphur water, are a favorite summer resort, near the summit, and sixteen miles from Athens. Such springs are frequent along the outcrop of this sandstone, resulting from the decomposition of the pyrite therein contained.

Looking northeastward, toward the other section of this mountain, to be hereinafter described, we see no intervening ridges and no apparent outcrop of the sandstone, but only a confused cluster of low, rounded, shale hills.

Descending the southeastern face of the mountain, we find its foot composed of the Ocoee slates, semi-metamorphosed, dipping northwest. In the middle of the valley is a knob of the Ocoee conglomerate, apparently occupying the axis of the anticlinal, since southeast dips at once recur, the slates forming the ridge bounding the valley on the southeast. The second and third ridges show small synclinal patches of the Chilhowee sandstone on their crests, with slight intervening anticlinals, while the fourth is a sharp anticlinal in the slates, which brings us to the Coca Creek waters, where the outcrop of these slates, with some small quartz veins, has yielded small amounts of placer gold. At one point here, I saw a small outcrop of the Chilhowee, dipping southeastward, and apparently cut off by a fault. Indeed, slight faults are frequent in the slates which here form the mass of the Smoky Mountains along the State line. These disturbances, however, though preventing any accurate determination of the true thickness of the beds, in no way interfere with the continuity of the mass and the constancy of the general southeasterly dips. These dips continue for about five miles beyond the State line, the beds showing more and more thorough metamorphism, the slates becoming micaceous and talcoid schists, and the inclosed beds of conglomerate becoming gneisses, more or less pebbly or even porphyritic. Here, on the eastern slope of Long Ridge, just beyond Hennegar's, northwest dips set in again, and continue about nine miles, to Davidson's, in similar beds: at this latter point, an anticlinal of softer hydromica schists and gneisses, partly staurolitic, with thin quartz veins and much iron sand, appears for half a mile, and represents the copper-bearing beds of Ducktown, the ores of which are said to be exposed in the bed of the Hiwassee, perhaps three miles west of this point. The beds appear to lie conformably beneath the true Ocoee, on both sides of the anticlinal; and there seems to be no reason for referring them to a distinct group, as was done by Emmons; and here Kerr has not followed him. The series shows a much wider outcrop at Ducktown, a dozen miles southwest, where, as is well known, rich copper mines have been developed.* The ore-deposits here are irregular masses of "stock-work," though filling crevices which run nearly parallel with the inclosing strata, in consequence of schistose structure especially favoring splits in that direction. At many points, they *look* like *regularly interstratified beds.* Conditions have here favored most thorough metamorphism, probably by reason of a more open and porous condition of the material, allowing more abundant percolation of the heated mineral waters. The wall-rock, at some points, is of the toughest possible quartzyte; at others, a micaceous gneiss; again, a tremolyte or hornblendyte. Both the walling and "horses" of the same material are often permeated with copper, iron and zinc sulphids. The true gangue is quartz, at some points very abundant, at others scarce.

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* Still more extensive deposits would doubtless have been located, had more convenient access made mining more profitable. In the present condition of affairs, the Ducktown works, though economically managed, return hardly a living profit, and await the coming of a promised railroad for lower freights and cheaper fuel.

Passing on from this belt, the southeast dips bring in again the schists and gneissoid conglomerates of the Ocoee, and the gneisses and gneissoid quartzytes of the Chilhowee and the Knox sandstones. Above this latter bed, which is here, of course, undistinguishable from the Chilhowee, the Knox shale is represented by fine-grained blue mica slates speckled with mica crystals; and, within a quarter of a mile of Murphy, we find the Knox dolomyte, in white, speckled, gray, dove-colored and nearly black marbles. These materials being so much more easily eroded and dissolved than most of the quartzose rocks, have caused the formation of a long line of valleys, and are themselves generally covered. Before treating of them in detail, or passing on to more easterly outcrops, let us review the strata along another line of approach.

4. *From Knoxville to Murphy.*

At Knoxville, just north of the railroad track, a line of fault, essentially the equivalent of, and probably continuous with, that noticed just southeast of Athens, separates the upper shales of the Cincinnati group from the Knox dolomyte, both having southeast dips. The dolomyte, with its characteristic cherts and sandstones, forms the ridge upon which the main part of the city of Knoxville stands, and extends across to the south bank of the Holston, where it is regularly overlaid by the shaly limestones of the Chazy, the fine quarry-marbles of the Trenton, and the shales and iron-limestones of the Cincinnati, which latter form a line of high red knobs, with thin streaks of compact and oölitic hematite. About three miles out, a low rounded hill, between flat shale valleys, shows a synclinal of the upper "red marble." The iron-limestone, in successive waves, then occupies the surface nearly to Rockford—say for six miles—and then yields place to the Knox dolomyte, which continues to beyond Maryville. This town, sixteen miles from Knoxville, stands near the crown of a low arch, which appears to be the equivalent of the sharp anticlinal in the valley southeast of White Cliff, though here somewhat farther out from the main mountains. The northeastern section of the Chilhowee Mountain, indeed, is still nine miles beyond us. In approaching it, we pass over all the upper beds of the Lower Silurian, together with the shales and oölitic hematites of the Upper Silurian (Clinton, or Safford's "Dyestone group"), and the Black shale and soft gray calcareous shales of the Lower Subcarboniferous, and suddenly reach a fault of perhaps 10,000 feet, where the last-named beds abut against the Ocoee slates and conglomerates. The slates, at some points in this neighborhood, are quite chloritic, and so nearly black as to have been frequently mistaken for *anthracite*. Above the Ocoee, the mass of the mountain consists of the heavy beds of the Chilhowee sandstone, with high southeast dips. Montvale Springs, a favorite summer-resort, nine miles from Maryville, are near the foot of the mountain, and derive their chalybeate waters from the decaying pyrite of this formation. Several other strong chalybeate springs occur at various points along the mountain, but have not been similarly improved. Passing the summit, we find the southeastern foot-slopes faced with imperfect slates, representing the Knox shale, and the hollows beyond occupied by the Knox dolomyte, much disturbed and altered. The rough form of the country does not favor a full understanding of the disturbances without a thorough study of details; but it is at least evident that the dolomyte finally abuts against the Ocoee group, across a fault of several thousand feet, near the foot of the State line range, here called the Smoky Mountains. The Ocoee continues, with southeastern dips, for several miles up the Little Tennessee; but, at Rocky Point ferry, three miles beyond the line, let us cross the river, pass over a high spur to Cheowa River, and ascend the valley of that stream. The slates and conglomerates here pass rapidly into schists and gneisses. About fifteen miles from the ferry, we encounter the Chilhowee, in the form of a heavy-bedded, fine-grained gneiss, occupying, for a mile or so, the axis of a synclinal. It is reported that, on Snow Bird Creek, a western fork of Cheowa, a bed of limestone, which must be the Knox dolomyte, occupies a considerable area on the line, joining this outcrop with the synclinal noticed at Long Ridge on the other route. Northwestern dips of the Ocoee now continue on to the head of the river, consisting of hydromica schists and gneisses with staurolites and garnets, chlorite schists with garnets and quartz veins, and some bands of gneiss and quartzyte. As we approach the summit gap, toward Valleytown, we find an irregular anticlinal, equivalent to that at Davidson's on the other route, followed by very confused dips, as though the mountain bed suffered a tremendous squeezing in this region. These disturbed foldings continue even down to the valley-level, where sharp anticlinal folds occur in the micaceous gneiss, just before it dips beneath the Knox marble. It is not improbable that careful examination might detect small patches of the marble caught and held in some of the folds high up on the mountain-side. The point at which we have now struck this formation is about seventeen miles northeast of Murphy, along the direct line of outcrop.

If, instead of crossing at Rocky Point and ascending Cheowa, we follow up the Little Tennessee, we find heavy-bedded, gray, Chilhowee quartzytes exposed as, near Hazelnut Creek, we approach and pass the synclinal just noticed as running from Long Ridge across Cheowa. Hereabouts are also laminated micaceous sandstones and chloritic slates, with veins of milky quartz. These and similar beds continue to above the mouth of the Tuckaseege, whose dip is at first pretty regularly northwest, but soon becomes irregular, turning to southwest and even to south, as if some transverse axes of fold were developed, not far to the northeastward. As we approach and pass the mouth of Nantahala River, the same beds come down again, after we pass the axis of an anticlinal about a mile below Ashe's Mill: the dips, at first nearly east, soon become S. 20° E. Along the axis of the anticlinal, which is the equivalent of that running from Ducktown past Davidson's, there are said to be copper ores in schist along Stekoa Creek. Five or six miles above the Nantahala, near Wm. Dehart's, we are supposed to reach the range of the Valley River marble, though its outcrop has not yet been reported quite so far northeast.

(To be continued.)

Knoxville, Tenn., Jan. 25th, 1875.

ART. XXXI.—*Bentham, On the recent Progress and present State of Systematic Botany.*[*]

HAD Mr. Bentham remained a little longer in the presidential chair of the Linnean Society, his recent elaborate paper would substantially have been the staple of the last of that series of annual addresses, most of which have been noticed and several reprinted in this Journal. The present report is too long to reprint, and very difficult to abridge. A sketch of the progress of the science as regarded by a botanist who personally conversed with an active correspondent of Linnæus (Gouan of Montpellier); who received useful hints on the method of botanical study from A. L. de Jussieu, the founder of the Natural System; who was in intimate relations with the elder DeCandolle, Brown, Lindley, and Hooker; who has studied in all the European herbaria, and been in correspondence with all and in personal acquaintance with most of the notable botanists of the last forty or fifty years, and, finally, who is at the present time the most productive and the soundest of systematic botanists, cannot be otherwise than replete with interest.

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[*] **A Report made to the British Association for the Advancement of Science, 1874, separately issued, pp. 54, 8vo.**

We will pass over Mr. Bentham's retrospect of the first great movement of modern botany over the Linnæan thoroughfare to a knowledge of genera and species, and also of that of his and our own days, by which we have ascended a higher platform of ordinal classification along the path, once so difficult, but now made available and easy through the labors and genius of Jussieu, Brown, DeCandolle, and other but less illustrious pioneers; and we come down to "the next period in the progress of systematic botany, the seventeen years that elapsed from 1832 to 1859."

"The change from the technical to the scientific study of plants, which, during the preceding period, had been working its way through so many obstacles, was now complete. The Linnæan platform, established on the relations of genera and species, had now been so long and so universally adopted as the basis or stand-point, that the credit due to its founder was almost forgotten in the triumphant destruction of the sexual scaffolding he had erected for the ascent of the higher stages, and now completely superseded by the progress of the Jussiæan roads, although it was chiefly by the consistent following out of the principles laid down by Linnæus himself that the change had been effected. No would-be botanist was allowed any longer to eschew the labor of the methodical study of plants, or to indulge in the belief that their technical sorting constituted the science.

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"It would seem that at this advanced stage of our progress the guide-posts of the principal paths had become so firmly established, the principles upon which plants should be scientifically classed so clearly laid down, and so far carried into practice, that little remained to be done toward completing the survey of the territory—toward a general distribution of species according to their natural affinities—beyond the more accurate delineation of details and the interpolation of newly-discovered species; and that the systematic botanist could already look toward that summit upon reaching which his labors in aid of the general advance of the science might come to a close.

"But there was a rock ahead which had long been looming in the distance, and which on a nearer approach opposed a formidable obstacle. What is a species? and what is the meaning of those natural affinities according to which species are to be classed? were questions which in 1859 it was generally thought vain to discuss, or the answers to which, given to us by doctrinal teachers, unsupported by or independent of facts, it was considered as sacrilegious to doubt. We were taught, and some may still believe, that every species, such as we now see it, was an original creation, perpetuated through every generation within fixed limits which never have been and never will be transgressed. We were less authoritatively told that resemblances of different species were owing to their having been formed upon one plan variously modified. To the question why they were so modified, the ready answer was, such was the will of the Creator; and in order not to suppose that that will was influenced by mere caprice, it was suggested that the modifications were either to suit the plant to the circumstances it was placed in, or to remedy defects in the original plan, or we were simply told that the subject was beyond our powers of comprehension.*

"One consequence of this apparent impossibility of proceeding further in the investigation of the causes of affinities and of this necessity of taking species as separate creations in enormous numbers, with resemblances and differences in endless variety according to the inscrutable will of the Creator, was the encouragement it gave to arbitrary classifications and interminable disputes as to the limits of individual species. It was, indeed, generally admitted that plants should be arranged in genera, orders, &c., in groups of higher and higher grades according to the importance of the characters they had in common, and that the test of species was the persistence of its characters through two or more generations; but there were no means of estimating the importance or value of characters except by such vague standards as the number of species in which they had been observed to prevail, no means of determining what degree of variation and persistence actually distinguished the species from the variety. The botanist who affirmed that *Rubus fruticosus*, *Draba verna*, or *Sphagnum palustre* were each one very variable species, and he who maintained that they were collective names for nearly four hundred, for at least two hundred, or for some twenty separately created and invariably propagated species, had each arguments in their favour to which no definite reply could be given; and systematic botany was in too many cases beginning to merit the reproach of German physiologists, that it was degenerating into an arbitrary multiplication and cataloguing of names and specimens, of use to collectors only, and serving as impediments instead of aids to the extension of our scientific knowledge of the vegetation of the globe.

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* "In my frequent intercourse during the above period with foreign botanists, I heard more than one German Professor affirm that a type-form was created for each natural order (the common clover, for instance, being that for Papilionaceæ), that Nature set to work to modify this type-form in framing species of a more complicated structure, till, tired of the exertion, she next produced new species by the simple omission of some of the complications. A French botanist of great eminence, to account for the number of plants in cultivation which are not known to exist in a wild state, observed that we could not suppose that man would have been created without a simultaneous creation of plants for him to cultivate for food, quite independent of the wild vegetation which existed before him for the food of animals. And many other still wilder theories were propounded to account for facts inconsistent with the presumed independent creation and absolute fixity of species. The best authorities went no further than defining affinity as correspondence of characters, physiological or structural, and estimating the value of characters and the importance of peculiarities or modifications of character according to their known connexion with the phenomena of life."

"It is true that long before the period under consideration some indications by which this great obstacle to further progress might be surmounted had been vaguely given, and the theory of a common descent of modern species had been broached, or generally proposed as a solution of some of the difficulties; but not in a manner sufficiently plausible to overcome the prejudices against following up any such track, nor supported by facts and observations sufficient to awake the attention of the most anxious pursuers of the science. It was reserved for the publication of the 'Origin of Species' in 1859 to mark out a practicable path by which the higher summits might be attained. The doctrine of evolution of species, according to laws originally fixed, instead of arbitrary intervention upon each and every occasion, was in this remarkable work clearly traced out, supported by powerful arguments, and founded upon facts and observations the accuracy of which no one could doubt; and a way was thus opened up to a pinnacle, which in a wonderful degree enlarged the range of vision of those who had the courage to follow its propounder up the giddy height. It was immediately and successfully taken to by several of the most eminent of our naturalists accustomed to philosophical deductions from ascertained facts; it was blindly accepted, but misused, by some German and Italian speculators, who, in their hurry to adopt Darwinism before they well understood it, and in their eagerness to go beyond the point to which the road had been securely marked out by the author, or to diverge into by-paths which led to precipices and pitfalls, added to the alarm of the timid; whilst it was not only shunned, but denounced as fraught with the utmost danger by the great majority who were accustomed to place tradition above reasoning. We systematists hesitated at first to advance in a direction so contrary to that which we had determinately followed for so long a period; but after a careful study of the facts and arguments upon which the new course was founded, and of the guide-posts which had been set in it, we most of us have felt but little doubt of its safely leading us over difficulties, which we had so long reckoned as insurmountable, into a vast and entirely new field of observation, calculated to give a stability to the results of our labours, of which we had hitherto formed no conception. The last of the eminent observers of nature who persistently maintained the independent creation and absolute fixity of species (the late distinguished Professor Agassiz) has recently gone from among us; and it may now be given as a generally received doctrine, that all natural methods must be founded on affinities as dependent on consanguinity. Fifteen years have sufficed to establish a theory, of which the principal points, in as far as they affect systematic botany, may be shortly stated as follows:—

"That although the whole of the numerous offspring of an individual plant resemble their parent in all main points, there are slight *individual* differences between them.

"That among the few who survive for further propagation, the great majority, under ordinary circumstances, are those which most resemble their parent, and thus the *species* is continued without material variation.

"That there are, however, occasions when certain individuals with slightly diverging characters may survive and reproduce races in which these divergences are continued even with increased intensity, thus producing *Varieties.*

"That in the course of an indefinite number of generations circumstances may induce such an increase in this divergency, that some of these new races will no longer readily propagate with each other, and the varieties become *New Species*, more and more marked as the unaltered or less altered races, descendants of the common parent, have become extinct.

"That these species have in their turn become the parents of groups of species, i.e., *Genera*, *Orders*, &c., of a higher and higher grade according to the remoteness of the common parent, and more or less marked according to the extinction or preservation of unaltered primary or less altered intermediate forms.

"As there is thus no difference but in degree between a variety and a species, between a species and a genus, between a genus and order, all disputes as to the precise grade to which a group really belongs are vain. It is left in a great measure to the judgment of the systematist, with reference as much to the use to be made of his method as to the actual state of things, how far he should go in dividing and subdividing, and to which of the grades of division and subdivision he shall give the names of Orders, Suborders, Tribes, Genera, Subgenera, Sections, Species, Subspecies, Varieties, &c., with the consequent nomenclature. In the limitation of his orders, genera, species, &c., he must carefully observe those cases where the extinction of races has definitely isolated groups having a common parentage; and in other cases where the preservation of intermediate forms has left no such gaps, he is compelled to draw arbitrary lines of distinction wherever it appears to be most convenient for use. In the pre-Darwinian state of the science we were taught, and I had myself strongly urged, that species alone had a definite existence, and that genera, orders, &c., were more arbitrary, established for practical use, and founded on the combination of such characters as appeared the most constant in the greater number of species, and therefore the most important. We must now test our species, as well as genera or other groups, by such evidences as we can collect of affinity derived from consanguinity.

"In valuing these evidences, in estimating the comparative value of characters, a new difficulty has arisen, that of distinguishing the two classes of characters to which Professor Flower has appropriately given the names of *essential* and *adaptive*,—the former the result of remote hereditary descent, the latter the more recent effect of external influences. This distinction is often the more difficult, as the essential ones are often only to be found in embryos, in the early stages of organs, or are merely indicated by slight rudiments requiring close observation to detect them; whilst the adaptive ones, of comparatively small systematic importance, are often developed in external form, in ramification, spinescence, foliage, &c., and are the most striking to the eye. One consequence is, that the systematist of the present day sees more and more the necessity of preparing a double arrangement of his genera, species, and other groups—a natural one according to the best evidences of affinity for the purpose of scientific study, and an artificial clavis by which the student can be led to identify genera or species by the more readily observed characters, which may only form part, or be but chance accompaniments, of the essential ones. The greatest change, however, which the adoption of the doctrine has effected in the methodical study of plants is the having rendered it necessary, in the case of every genus or other group, to take into account and specially to estimate the value of all the characters observed—no one can be taken as so absolute as to obviate the need of considering others, no one can be passed over as theoretically worthless; and whilst this adds immensely to the labour of the systematist and to the calls on his judgment, it gives equal increase to the value of the results obtained."

The above statement, as we believe, clearly expresses the general view of the leading systematic botanists of our time, as to the derivation of species, as well as some of the grounds which have inevitably led to it. As by Mr. Bentham in England, so also here, such views were early reached, not through general theorizing nor under partizan predilection, but upon sober consideration of the facts that we knew most about, and to which years of impartial study have been given. Long-continued conscientious work in systematic botany, upon copious materials, gives excellent training; and it is pretty sure to lead most minds to the conclusion that the present forms came by way of derivation and diversification. The results of the study of the Tertiary and Cretaceous fossil botany of the northern hemisphere bring confirmation which every year makes stronger; and the riddles of geographical distribution finding easy solution thereby, the adoption of the hypothesis as the practical one seems unavoidable, if there is to be any science in botany.

[To be continued.]

ART. XXXII.—*Spectroscopic Examination of Gases from Meteoric Iron;* by ARTHUR W. WRIGHT.

THE well-known investigation of Professor Graham, upon the gases occluded by meteoric iron, showed that the Lenarto meteorite contained at least 2·85 times its volume of gaseous substances, consisting of hydrogen, carbonic oxide, and nitrogen.* The percentages of the several gases were: hydrogen, 85·68; carbonic oxide, 4·46; nitrogen, 9·86. As he had observed that it is difficult to produce an absorption by iron of more than its own volume of hydrogen under a pressure of one atmosphere, he concluded that the Lenarto iron had derived this gas from an atmosphere containing it in abundance, and at a pressure much greater than is found at the surface of the earth, such, for instance, as must exist near the surface of the sun or of the larger fixed stars.

Similar results were obtained subsequently by Professor J. W. Mallet,† in experiments upon a meteoric iron from Augusta Co., Virginia, in which he found the same gases present, in a somewhat different proportion, with the addition of a small amount of carbonic di-oxide, the percentages being: hydrogen, 35·83; carbonic oxide, 38·33; carbonic di-oxide, 9·75; nitrogen, 16·09. The percentage of hydrogen in the first portion of gas collected was greater than in the portions obtained later. The volume of the gases was 3·17 times that of the iron.

On the supposition that the meteoric iron has received its hydrogen and other gases from an extra-terrestrial source, if not from the sun, possibly from some other body having a similar atmosphere of great density, it seemed probable that the unknown gaseous elements, assumed to be present in the solar corona and chromosphere, might be detected in the gases yielded by meteorites; and this investigation was undertaken in the hope that the spectroscope would reveal them if present, though their small amount or peculiar character should render their discovery by the ordinary chemical means difficult or impossible.

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* Proc. Royal Soc., xv, 502. † Ibid., xx, 365.

The mode of obtaining the gas was similar to that pursued by Graham and Mallet, with the exception, that the iron, instead of being a single solid mass, was in the form of fine chips, prepared by boring into the meteorite with a steel drill upon a lathe, those portions which came from near the exterior being rejected, with the observance of suitable precautions to prevent the admixture of foreign matter. The fine particles of the iron were placed in a thick tube of very hard glass, which had been carefully cleaned, and were pressed into it with a glass rod. It was found possible to bring the glass tube, thus partially filled, to a red heat, without its yielding very much to the atmospheric pressure.

For the exhaustion a very efficient Sprengel pump was used, provided with a U-gauge, the limbs of which have an internal diameter of about six millimeters, and which is capable of giving accurate readings to a fraction of a millimeter. The mercury is supplied to the fall-tube by a recurved tube, the branches of which are thirty-two inches long, an arrangement which effectually prevents the entrance of air. The pump is capable of carrying the exhaustion to such a point that it becomes difficult to estimate the difference of level of the mercury surfaces in the gauge, and the electric discharge through a vacuum-tube exhausted by it becomes very feeble. That there was no leakage was shown by the fact that, after the exhaustion had been carried to the extreme, the gauge maintained its position unchanged for days.

To the exhaust-tube of the pump was attached a horizontal glass tube having two branches at right angles to its length. To one of these a vacuum-tube of the form ordinarily employed for spectroscopic work was attached, so as to have a vertical position. The other was closed by a stop-cock by which air or any gas could be admitted. The outer end of the horizontal tube was also provided with a stop-cock, and beyond this the tube containing the iron was firmly attached with cement.

It was important to know what lines would appear in the spectrum from an ordinary tube exhausted by the pump, and containing air, in order to judge correctly as to the presence or absence of any gaseous bodies coming from the iron. It is well known that the lines of hydrogen, and bands due to carbon compounds,* appear when the air in the tube is rarefied to a high degree. A number of preliminary trials showed that these lines, with very faint carbon bands, probably due to fatty matters employed in lubricating the stop-cocks, invariably appeared toward the limit of the exhaustion, while lines due to the mercury were generally visible during the later stages of the rarefaction, having greater or less brilliancy according to the temperature of the room. The red hydrogen line is frequently dimly visible when the tension has fallen to four or five millimeters, but is neither distinct nor relatively bright until the pressure is much less than that, while the other hydrogen lines appear distinct only after a much higher degree of rarefaction is attained.

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* Plücker and Hittorf. On the Spectra of Ignited Gases and Vapors, Phil. Trans. Royal Soc., vol. 155, p. 1.

The first trial was made with a piece of meteoric iron, which is a fragment of the great Texas meteorite, in the cabinet of Yale College. This meteorite, of which a description has been published,* is a large mass weighing 742 kilograms, having the following composition: Fe, 90·91; Ni, 8·46; insoluble portion containing some carbon, 0·50.† The chips produced by the borer were mostly very small particles. Much of the metal was reduced to powder, and the coarser portions were crushed in the process of boring, so as to destroy the solidity of the iron and break up its structure. A quantity of the borings representing 0·384 cubic centimeter of the solid iron were placed in the glass tube, which was then fastened in its place. The stop-cock was closed and the pump set in operation. When the gauge stood at 17 mm. the cock was momentarily opened, and was then closed. The gauge rose to about 100 mm., and when the pump had brought it down again to 17 mm. the spectroscope was applied to the vacuum-tube. The red hydrogen line was seen bright, the rest of the spectrum having the ordinary banded structure due to nitrogen and oxygen. As the exhaustion proceeded the other hydrogen lines appeared, and when the tension was reduced to 4 mm. both H_α and H_β were bright and distinct. H_γ was visible, though less prominent. The carbon bands also were distinctly seen. At 2·5 mm. pressure the stop-cock was opened, causing the gauge to rise 12·5 mm., after which it remained nearly stationary for fifteen minutes, although the pump was in action. A simple calculation shows that the first rise of 12·5 mm. is just what should have been produced by the air contained in the tube with the iron. But the fact that the gauge maintained this position for a considerable time, while the pump was continually withdrawing the air, shows that the iron gave off a portion of its gas without the application of heat, and it was repeatedly observed, in other experiments, that when the stop-cock was open, and the pump not in action, the gauge continued to rise very slowly, sometimes as much as two millimeters in an hour or two.

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* Profs. B. Silliman and T. S. Hunt, this Journal, II, ii, 370.

† Loc. cit.

A gentle heat was now applied to the tube containing the iron, by means of a Bunsen burner. This brought the gauge in a few minutes to about 6 mm., and produced a marked change in the appearance of the vacuum-tube, which before had the appearance of an ordinary hydrogen tube. The light in the broad portion became a straight, hazy stream of a dull greenish-white color, very similar to that given by a tube containing either of the oxides of carbon. After the tube had been exhausted to 2 mm., heat was again applied rather more strongly than before, but still below redness, carrying the gauge to 9 mm. in about ten minutes, the effect upon the spectrum being merely to increase the intensity of the carbon bands. The tube was now wrapped with copper foil, and the temperature, by means of a Bunsen flame, carried to low redness, so that the glass softened and began to yield. But a small quantity of gas was given off, the gauge at the end of ten minutes standing at 5 mm. The stop-cock being closed, the exhaustion was continued to 1·5 mm. At this point the spectrum was nearly the same as before, but was somewhat less brilliant. Certain other lines appeared in the spectrum, of which mention will be made in a later paragraph.

A second set of experiments was made with a specimen of meteoric iron from Tazewell County, Tennessee. The meteorite, of which this was a portion, weighed when found twenty-five kilograms, and has the composition, Fe, 83·02; Ni, 14·62; other substances, 1·93. No carbon was found in it.* The iron was found to be softer than the previous specimen, and was bored with comparative ease, the fragments produced being coarser than those from the Texas iron. The experiments were made in the same manner as in the previous examination, 0·634 cubic centimeter of the iron being placed in the glass tube upon the pump, and the stop-cock closed. After exhausting to one millimeter, the cock was opened for a moment to withdraw the air from the specimen-tube, and then closed. This produced a rise in the gauge of 104 mm. On again exhausting to one millimeter, and opening the stop-cock, the gauge rose 20 mm., not coming to rest immediately, but continuing to ascend slowly for some time. From calculation it appears that air at a tension of 105 mm. in the tube containing the iron would raise the mercury in the gauge about 15 mm. only, the remaining effect evidently arising from evolution of gas from the iron. This the spectroscope confirmed, as the red and green hydrogen lines were brilliant, even before the pump had further reduced the pressure, and they increased in relative intensity as the rarefaction increased. The carbon bands, if present, were not especially noticeable, as there is no record of them in the notes of the experiment.

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* Prof. J. L. Smith, this Journal, II, xix, 153.

The tube containing the iron being strongly heated, gas was given off, which brought the gauge to 29 mm. in about 15 minutes. The heat was such as to redden and soften the glass. The vacuum tube, when the discharge was passed through it, appeared like an ordinary tube containing carbon compounds, and the spectrum gave the hydrogen lines very brilliantly, with the four chief carbon bands in great strength. The cock was again closed and the pump set in operation, the spectrum being observed from time to time throughout the process of exhaustion. As the tension of the gas decreased, the hydrogen lines became relatively stronger, and the carbon bands grew narrower; the one in the red beginning to be resolved into lines when the pressure was much reduced. At 1 mm., the carbon bands were still prominent, and some narrow bands belonging to nitrogen were observed.

In the first of the specimens of iron thus far examined, the proportion of carbon found by chemical analysis was very small, and in the second none at all was detected. The third series of experiments was made with an iron containing a larger amount of carbon. This was a fragment of the well-known meteorite from Arva, in Hungary, which has the following composition: Fe, 90·471; Ni, 7·321; residuum containing carbon, silica, and cobalt, 1·404.* On attempting to bore into this iron, as had been done with the others, great difficulty was experienced, as the metal has nearly the hardness of steel. After the expenditure of much labor, a sufficient quantity of the iron was obtained for the examination, representing 0·224 cubic centimeter of the solid metal. It was in a state of minute subdivision, being almost entirely reduced to fine powder. After it was placed in the tube connected with the pump, the air was exhausted from the latter as long as any change was visible in the gauge, bringing it finally very nearly to zero. The stop-cock had been closed from the first. The work having been interrupted for several days, the gauge was found, on resuming it, not to have suffered the slightest change, showing that the joints of the pump were perfectly tight. The gauge rose, on opening the stop-cock, to 98 mm. As the gases were rarefied by the pump, the hydrogen lines shone out very brilliantly when the pressure was reduced to a few millimeters, and at 2 mm., the vacuum tube had to the eye the appearance of an ordinary hydrogen tube while the electric discharge was passing through it. At this pressure, the second and third carbon bands, counting from the red end, were quite bright. The pump was stopped when the tension was reduced to 1·5 mm., but the gauge rose very slowly, gaining about a millimeter in three hours. The spectrum was as before except that the first carbon band was now visible, and the others were stronger. There could be no question as to the evolution of both hydrogen and the carbon gases, without the application of heat, by simple diminution of the atmospheric pressure.

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* **Analysis of A. Löwe. Notice in this Journal, II, viii, 439.**

A very gentle heat was applied to the tube for a few minutes, making it hardly warm enough to give pain when touched by the hand. This had the effect to raise the gauge to 28 mm. in fifteen minutes, and cause an entire change in the appearance of the vacuum-tube, which became greenish in the broad part, like those in the former experiments. The spectrum, when the pressure was reduced to a few millimeters, showed the carbon bands quite bright, and as the pressure decreased the first one became clearly resolved into five fine lines nearly equidistant, upon a faintly luminous background.

A somewhat stronger heat was now brought to bear upon the tube, but far less than would have been necessary to bring it to redness, as it was desired to avoid the possibility of error from the oxidation of the carbon by traces of oxygen left in the tube. The gauge rose to 50 mm., sinking to 47 mm. as the tube regained its ordinary temperature, doubtless from the contraction of the gas on cooling; for, on subsequently exhausting, it was found that the gauge continued to rise, though with extreme slowness. The spectrum did not differ essentially from that before observed, except in the greater intensity of the carbon bands. A few cubic centimeters of the gas, collected during the second heating, when tested qualitatively, gave evidence of the presence of hydrogen, carbonic oxide, and carbonic di-oxide. The proportion of the latter did not appear to be more than three or four per cent, that of the carbonic oxide being probably greater. The gas burned with a flame like that of hydrogen. The tube was subsequently heated to redness, and kept at that temperature until the gauge became very nearly stationary, the change in it being so slow as to be scarcely perceptible. Enough gas was given off to raise the mercury to 33 mm., after which the heat was discontinued.

For the sake of comparison, a trial was made with borings from a ball of soft iron. The ball had a diameter of three centimeters, and was solid. The exterior was covered with rust. The examination was made in the same manner as in the previous cases, excepting that the stop-cock remained open during the process of exhaustion. When the tension was reduced to 9 mm. the red and green hydrogen lines were brilliant, and the carbon bands appeared, with feeble intensity, as the rarefaction increased. On heating the tube strongly, though not to redness, gas was given off in abundance, raising the mercury in the gauge to 28 mm. in a few minutes, and causing the vacuum tube to assume the appearance of an ordinary CO or CO_2 tube.

The spectrum also showed a great increase in the intensity of the carbon bands. It was in every way similar to that observed in the case of the meteoric irons, except that the relative intensities of the parts due to hydrogen and the oxides of carbon were different, indicating a much smaller proportion of the former. The hydrogen lines neither appeared so early, nor were they so bright as in the former experiments.

Although foreign to the main purpose of the investigation, it seemed of interest to determine the amount of the gaseous products yielded by the three specimens of meteoric iron. By measurement it was found that, when the tubes containing the iron borings were attached to the pump, the gauge was lowered 10·4 millimeters for every cubic centimeter of gas carried out by the mercury. It was thus easy to find the amount of gas given off by simply observing the reading of the gauge. From calculations made in this way it was ascertained that the Texas iron gave off 4·75 times its volume of the mixed gases, and the Tennessee iron 4 69 volumes. Although it was evident that the greater portion of the gas had been driven off by a comparatively moderate elevation of temperature, the whole amount was by no means exhausted, as the heat was withdrawn before the evolution of gas had entirely ceased. These volumes, it will be observed, are considerably larger than those obtained by Graham and Mallet from the specimens examined by them. This is probably due to the minute subdivision of the iron, which would favor a more rapid and complete evolution of the gas. It was found, in their experiments, where the iron was in a single solid mass, that gas was still given off after the high temperature had been continued for several hours.

In the case of the iron from the Arva meteorite, which was in the state of fine powder. the yield was very much greater, a little more than thirty volumes of gas being given off in the experiments already described, in which the highest temperature was far below a red heat. On subsequently heating the tube and its contents to low redness fourteen volumes more came off, making the whole amount somewhat more than forty-four volumes, not reckoning the additional quantity gained in a subsequent experiment. It seemed not impossible that a portion of this large amount of gas might be atmospheric air condensed upon the fine particles of iron, but it was difficult to determine this point with certainty. After the last heating just mentioned, the iron was removed from the tube, exposed to the air for several hours, and then replaced. On re-heating it very strongly a considerable amount of gas was evolved, but the spectroscope, when applied to the vacuum-tube, showed the presence of hydrogen and the carbon compounds as before, making it probable that the increase due to condensed air was not large. The recent experiments of M. Cailletet* show that ordinary iron, under certain conditions, as when electrolytically deposited, may absorb nearly two hundred and fifty times its volume of hydrogen.

The descriptions of the spectra thus far given have reference to those lines and bands which were caused by the presence of hydrogen and the oxides of carbon. The vacuum-tubes exhibited differences depending chiefly upon the relative proportions of these gases present, and which have already been sufficiently noted. In the spectra from all the tubes there appeared a number of other lines, which were found to belong, for the most part, to oxygen. The tube containing gases from the Tennessee meteorite gave also several lines due to nitrogen. The latter were somewhat different, as to the order of their relative intensities, from those observed with nitrogen alone. None of them were very bright, and it was a matter of some difficulty to fix their positions with entire accuracy by measurement. The observations were made with a spectroscope of six prisms, with a repeating prism, giving the dispersion of twelve in all. One of the lines appeared to coincide with the chief coronal line 1474 K., but it is not so sharp as the latter appears in the solar spectrum. A narrow and rather faint band appears at this point also in the spectrum of the electric spark between platinum points in air. Of the oxygen lines, one falls some distance below 1474, having the position 1462 K., very nearly. This, though not brilliant, is the strongest line in that region of the oxygen spectrum as obtained from a vacuum-tube. It falls very near the position assigned to a bright coronal line by the Italian observers, Denza and Lorenzoni, of the eclipse of Dec. 22, 1870.† The former gives 1463 K. as the approximate position, while the latter assigns the same number, with the remark that the probable error of the reading is four divisions of Kirchhoff's scale. A second line of oxygen, less bright, but sharp and distinct, has the position 1359±1 K. A coronal line, which also appears in the auroral spectrum, has, from Prof. Young's observation, the position 1350±20 K., but the limits thus stated are too wide to give the supposition of a coincidence between it and the oxygen line anything more than a doubtful probability.

These lines of oxygen and nitrogen in the spectra observed are somewhat variable in their character, doubtless owing to the presence of the hydrogen, for the same appearances can be reproduced substantially by employing a mixture of these gases. Several experiments were made with a tube containing at first only air. When this was exhausted the bands arising from the compounds of nitrogen and oxygen appeared as usual, but on admitting a little hydrogen the spectrum was completely changed, and lines appeared, belonging to oxygen and nitrogen, the one or the other, or both, according to the amount of the gases present, and varying with the temperature and pressure. Lockyer and Frankland, in their experiments upon mixtures of hydrogen and nitrogen,* found that the result was not a mere superposition of the spectra of the two gases, but that the presence or absence of lines, and their relative intensities, depended upon the proportions of the two gases, as also upon the degree of rarefaction, and the energy of the electric discharge.

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* L' Institut, Nouv. Sér., Ann. 3, p. 44.

† Rapporti sulle Osservazioni dell' Ecclisse totale di Sole, dell 22 Dicembre, 1870. pp. 62, 106.

It will be seen, then, that the investigation, so far as the presence of new substances in the gases of meteoric iron is concerned, gives only a negative result. But the fact, incidentally observed, of the varying character of the oxygen and nitrogen lines in the presence of hydrogen, and the near coincidence of two of them with prominent coronal lines, with the possible coincidence of a third line, goes to show that the characteristic lines in the spectrum of the corona, so far from indicating the presence of otherwise unknown elements, are due simply to hydrogen and the gases of the air, namely, oxygen and nitrogen.

Yale College, March 18, 1875.

ART. XXXIII.—*The Discovery of the duplicity of the principal Star of* Σ 1097; by S. W. BURNHAM.

THE naked-eye star, B A. C. 2470, is given in *Mensuræ Micrometricæ* as a wide pair, the secondary being of Struve's 8·7 magnitude. The following measures have been made:—

Struve	(1832·1)	D = 20″·20	P = 312°·1
Smyth	(1834·1)	20·00	315·0
Radcliffe	(1863·1)	20·52	311·2

Sir John Herschel subsequently noted another companion of the 12th magnitude at an estimated distance of 25″. A short time since I examined this with my 6-inch Clark refractor to get the position angle of the third star, which Herschel has not given, and perceived at once that the bright star of the group was itself a close double. The components are separated only about 0″·7, and considerably unequal, being 6 and 7½ magnitudes. No suitable opportunity has since occurred to measure the angle, but the close pair and the distant companion appeared to be almost exactly in the same line. Taking the latest measures of Struve's pair, with the estimated distances and angles of the new and distant companions, the system would stand as follows:—

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* Solar Physics, p. 530.

A and B	D= 0″·7 ±	P= 160°· ±
AB and C	20·52	311·2
AB and D	30· ±	160· ±

The star, D, is very nearly one and a half times the distance of C. With a power of 400, my instrument just separates the close pair.

Chicago, March 13th.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Ethyl Phosphite and the Constitution of Phosphorous Acid.*—Under the direction of Professor Wislicenus, Zimmermann has made some experiments upon ethyl phosphite, with a view to unravel the constitution of phosphorous acid. The ether was prepared by a modification of Railton's method, i. e., by acting upon sodium ethylate with phosphorous chloride, special precautions being taken to ensure the dryness and purity of the materials. The yield was from 65 to 70 per cent. The properties of the ethyl phosphite agreed with those described by Railton; it was a disagreeably smelling liquid of 1·075 sp. gr., boiling in the air at 191°, soluble in water, alcohol, and ether, and slowly decomposing in contact with the air. On analysis it yielded numbers corresponding to the formula $PO_3C_6H_{15}$. Upon saponification and precipitation with barium chloride, a barium phosphite corresponding to the formula $(BaHPO_3)_2+H_2O$ was obtained, thus proving that the ether had not the composition $C_2H_5PO(OC_2H_5)_2$. No ethyl phosphite was formed. Oxidation with nitric acid gave phosphoric and oxalic, but no ethyl-phosphoric acid. Oxidation in the air converted it into ethyl phosphate. Heated in a sealed tube to 250° for ten hours, hydrogen phosphide and ethylene gases were evolved, and phosphoric acid, but no triethyl phosphine remained in the liquid. The reaction appears to be

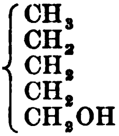
$$(P(OC_2H_3)_5)_4=(PO(OH)_3)_3+PH_3+(C_2H_4)_3.$$

On treating the pure crystallized acid with sodium hydrate in excess, and adding absolute alcohol, a syrup was precipitated which, after washing, showed a weak alkaline reaction. Placed in a vacuum over sulphuric acid, it showed no tendency to crystallize even after months. Upon analysis it yielded phosphorus and sodium in the ratio of 1:3. The author believes consequently in the existence of tribasic phosphorous acid, the normal acid of trivalent phosphorus, and a trihydroxyl derivative.—*Liebig's Ann.*, clxxv, 1, Dec. 30, 1874. G. F. B.

2. *New Salts and Reactions of Cæsium and Rubidium.*—Some time ago Godeffroy described the precipitate produced in cæsium solutions by antimonous chloride. (To this he now assigns the formula $(CsCl)_6SbCl_3$.) He has continued his investigation and now announces that not only antimonous chloride, but the chlorides of many other metals give, with cæsium salts, crystalline precipitates, difficultly soluble in hydrochloric acid. He mentions of these: iron-cæsium chloride $Fe_2Cl_6(CsCl)_6$; bismuth-cæsium chloride, $BiCl_3(CsCl)_6$; zinc-cæsium chloride, $ZnCl_2(CsCl)_2$; cadmium-cæsium chloride, $CdCl_2(CsCl)_2$; mercuric-cæsium chloride, $HgCl_2(CsCl)_2$; copper-cæsium chloride $CuCl_2(CsCl)_2$; manganese-cæsium chloride $MnCl_2(CsCl)_2$; and nickel-cæsium chloride, $NiCl_2(CsCl)_2$. These salts fall readily as precipitates when the metallic chloride, dissolved in concentrated hydrochloric acid, is mixed with a similar solution of cæsium chloride. The double salts are very soluble in water and in dilute hydrochloric acid, but crystallize out easily on evaporation. An investigation was then made into the behavior of the other alkali-metals and of ammonium with the metallic chlorides above mentioned, but no results were obtained with any of them except rubidium, which gave with the chlorides of antimony, bismuth, iron, zinc, cadmium, mercury, copper, manganese and nickel, beautifully crystallized double salts, completely analogous with the cæsium double chlorides in composition. Since these cæsium salts are not perfectly insoluble except in concentrated acid, only the platino-chloride can be used for its separation.—*Ber. Berl. Chem. Ges.*, viii, 9, Jan., 1875. G. F. B.

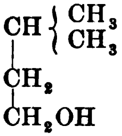
3. *On Diethylcarbinol, a new Isomer of Amyl alcohol.*—The present theory of chemical constitution points out the existence of eight isomeric amyl alcohols, of which four are primary, three are secondary and one is tertiary. These are constituted as follows:—

Primary.



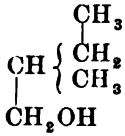
$\left\{\begin{array}{l}CH_3\\CH_2\\CH_2\\CH_2\\CH_2OH\end{array}\right.$

Normal Butyl carbinol.



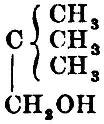
$\begin{array}{l}CH\left\{\begin{array}{l}CH_3\\CH_3\end{array}\right.\\ |\\CH_2\\ |\\CH_2OH\end{array}$

Isobutyl carbinol.



$\begin{array}{l}\quad\;\; CH_3\\ \quad\;\; |\\CH\left\{\begin{array}{l}CH_2\\CH_3\end{array}\right.\\ |\\CH_2OH\end{array}$

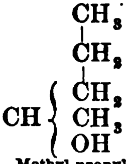
Secondary-butyl carbinol.



$\begin{array}{l}C\left\{\begin{array}{l}CH_3\\CH_3\\CH_3\end{array}\right.\\ |\\CH_2OH\end{array}$

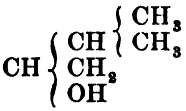
Tertiary-butyl carbinol.

Secondary.



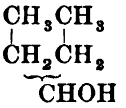
$\begin{array}{l}\qquad CH_3\\ \qquad |\\ \qquad CH_2\\ \qquad |\\CH\left\{\begin{array}{l}CH_2\\CH_3\\OH\end{array}\right.\end{array}$

Methyl-propyl carbinol.



$CH\left\{\begin{array}{l}CH\left\{\begin{array}{l}CH_3\\CH_3\end{array}\right.\\CH_2\\OH\end{array}\right.$

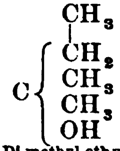
Methyl-isopropyl carbinol.



$\begin{array}{c}CH_3CH_3\\ |\quad\;\; |\\ \underbrace{CH_2CH_2}\\ CHOH\end{array}$

Di-ethyl carbinol.

Tertiary.



$\begin{array}{l}\quad CH_3\\ \quad |\\C\left\{\begin{array}{l}CH_2\\CH_3\\CH_3\\OH\end{array}\right.\end{array}$

Di-methyl-ethyl carbinol.

Of these alcohols, the first discovered and the best known is the one produced during the fermentation of grain, potatoes, beet roots, etc., and known as fusel oil. It was discovered by Dumas in 1835 and its constitution was fixed by Erlenmeyer in 1867, by proving that it yielded on oxidation a valeric acid identical with one prepared by him from isobutyl cyanide, and also with the isopropylacetic acid of Frankland. It is therefore the second of the alcohols above given, viz., isobutyl-carbinol. Wurtz, in 1862, prepared a second amyl alcohol from amylene, which was proved subsequently, both by himself and Kolbe, to be the sixth alcohol above given, viz., methyl-isopropyl-carbinol. The third amyl alcohol was produced synthetically by Popoff in 1867, by acting with zinc ethyl on propionyl chloride. This synthesis, as well as its oxidation products, proved it to be the eighth, or tertiary, alcohol given above, viz., di methyl-ethyl-carbinol. Wurtz in 1868 produced a fourth isomer, which he called ethyl-allyl-hydrate, since it was prepared by the action of zinc ethyl on allyl iodide. It corresponds to formula number five in the above list, being methyl-propyl-carbinol. The fifth isomer was prepared in 1870 from normal butyl cyanide, by Lieben and Rossi. Its mode of preparation as well as its oxidation product, normal valeric acid, prove it to be the first of the series, or normal butyl-carbinol. Only three of the possible amyl alcohols remain, therefore. WAGNER and SAYTZEFF now communicate the discovery of one of the two remaining secondary amyl alcohols, viz., di-ethyl-carbinol. The method by which this was accomplished, which is general, was suggested by the fact discovered by Frankland and Duppa, that, while the action of an organo-zinc compound upon oxalic ethers caused the substitution of a hydrocarbon radical for oxygen in one of the carboxyl groups, a further similar substitution in the second carboxyl group could not take place; a condition being produced by the first substitution, which is unfavorable to the second. Hence the facility of substitution of the oxygen of a carboxyl group by a hydrocarbon radical belongs only to those ethers in which the carboxyl group is not in combination with any radicals, the carbon and hydrogen being directly united. For these reasons, zinc ethyl should act upon ethyl formate to produce a compound which, acted on by water, should yield a secondary amyl alcohol. Experiment fully confirmed this view. A series of trials led to the adoption of the following method of procedure: In a flask connected with an inverted condenser are placed four molecules of ethyl iodide, one of ethyl formate, a small quantity of zinc-sodium, and enough granulated zinc to fill the flask to the level of the liquid. The whole was heated on the water bath, under slight pressure, until it ceased to boil and no more gas was evolved. After cooling the crystalline mass was treated with iced water, then with hydrochloric acid to dissolve the zinc hydrate, and submitted to distillation. From the distillate the diethyl-carbinol was obtained, as an imperfectly mobile liquid, having the irritating odor belonging to common amyl alcohol. Its sp. gr. is 0·822 at 0°, and it boils at 116°–117°. On oxidation it yields diethylketone. Several of its derivatives are described. The authors have also produced secondary butyl alcohol by the new method.—*Liebig's Ann.*, clxxv, 351, Jan., 1875. G. F. B.

4. *Production of Allyl alcohol in the dry Distillation of Wood.*—ARONHIEM has discovered allyl alcohol among the products of the dry distillation of wood. The crude wood spirit—which owes its penetrating odor to this substance—after distillation of the methyl alcohol, leaves a residue having a density of 76° to 78° Tralles. This, upon repeated fractioning over caustic lime, affords a distillate boiling at 88–89°, possessing the odor of allyl alcohol. The low boiling point is due, however, as analysis showed, to the presence of a molecule of water, which could not be removed by fractional distillation. When dehydrated by chemical means, the boiling point rose to 96–97°, which is that of the pure allyl alcohol. The identity of the product was further established by direct conversion into bibromhydrin, boiling at 217° and into allyl iodide, boiling at 101°.—*Ber. Berl. Chem. Ges.*, vii, 1381, Nov., 1874. G. F. B.

5. *On the Gases evolved from Apples.*—BENDER has made an examination of the gases evolved from ripe apples. The fruit cut in pieces was placed in water free from air contained in a flask furnished with a delivery tube. On raising the temperature to 60°, the gas began to come off, and at 100°, the evolution was rapid. Four moderately sized apples afforded about 100 c.c. of gas. Upon eudiometric analysis this gas—great care having been taken to exclude the air—was proved to consist of 59·37 per cent of nitrogen, and 40·20 per cent of carbonic acid, the remaining 0·43 per cent being oxygen. In a subsequent experiment, 31·07 per cent of the gas was carbonic acid and 68·93 per cent of nitrogen. The author believes that the carbonic acid comes from a fermentation within the fruit, the ferment being produced at the time of ripening.—*Ber. Berl. Chem. Ges.*, viii, 112, Feb., 1875. G. F. B.

6. *On the Influence of Light upon Cane Sugar.*—KREUSLER has made a series of experiments to test the assertion of Raoult that pure cane sugar in aqueous solution, both air and ferments being completely absent, underwent an inversion solely through the influence of light. While Raoult used only a solution containing 10 grams of white sugar to 50 grams of water, Kreusler employed solutions of various strengths. These were sealed up in glass tubes, after careful exhaustion, and with the exception of one, were exposed, in a window facing the southeast, to the direct sunlight for eleven months; more than twice the time given by Raoult. On opening the tubes, they still showed a good vacuum, and the liquid was clear and free from microscopic vegetation of any sort. But when tested with Fehling's solution, neither the liquid exposed to the light nor that preserved in darkness gave the slightest reaction. Upon repeating the experiment, however, taking care to introduce a small quantity of air into the tubes before sealing, organic ferments were found present, and the liquid reduced strongly the copper test; that exposed to light being the more active. The author believes therefore that an imperfect exhaustion in Raoult's tubes was the cause of the results he describes.—*Ber. Berl. Chem. Ges.*, viii, 93, Feb., 1875. G. F. B.

7. *Reflecting Lever.*—M. Cornu has devised an extremely simple but ingenious substitute for the spherometer which he calls the reflecting lever, or reflecting spherometer. Suppose a short lever like the beam of a balance supported on four points, one at each end, and two replacing its central knife-edge; also that a vertical plane mirror is attached above it in a plane perpendicular to its length. If now the four points lie in one plane and a thin plate is placed under the two central ones, the lever will be in unstable equilibrium, and will rock from side to side. Viewing with a telescope the reflection of a distant scale, the thickness of the plate is readily deduced from the angular motion. For, calling i the inclination of the beam, p the thickness of the plate, and l the half length of the lever, we have $\sin i = \frac{p}{l}$, or if i is small, $p = il$. The motion of the beam is double this, and the reflected ray travels twice as rapidly as the mirror; hence calling n and n' the two readings, and D the distance of the scale, we have $p = il = \frac{n-n'}{4D} l$. If the four points do not lie in one plane a correction is readily applied. Comparing the results, he concludes that while this instrument is very easily made and absolute measures readily obtained within a tenth of a per cent, it compares favorably, even in accuracy, with the best spherometers.

This instrument is also readily applicable to the measurement of curvatures and for many other purposes where a minute change of position is to be noted.—*Journ. de Phys.*, iv, 7. E. C. P.

8. *Maps of the Solar Spectrum.*—Mr. Lockyer laid before the Royal Society, at a recent meeting, a portion of a new map of the solar spectrum. The scale is four times that of Angström and includes the portion between the wave-lengths 39 and 41. A photographic print of the spectrum was employed, and from it curves were constructed by which the wave-lengths of the new lines were obtained graphically. In Angström's map (spectre-normal) 39 lines are given within these limits; Angström and Thalen's map, 185; Cornu's new map, 205; and in the present map, 518.

The spectra of the following metals are also given: Fe, Co, Ni, Mn, Ce, U, Cr, Ba, Sr, Ca, K, Al, representing 416 lines.—*Nature*, 238. E. C. P.

9. Mr. J. H. N. Hennessey has also prepared a map of the atmospheric lines drawn on the same scale as that of Kirchhoff. The observations were made at an elevated point, and the spectrum, when the sun was high, compared with that at sunset. Observations were also made to ascertain whether any of the lines which appeared, when the sun was low, were due, not to specific atmospheric absorption but to the general weakening of the light. The intensity of the spectrum was reduced in various ways, but the best results were obtained by observing the sun through a haze, with the result that none of the additional lines appeared to have any other origin than elective atmospheric absorption.—*Nature*, 278. E. C. P.

10. M. Cornu has prepared a map of the ultra-violet portions of the spectrum, to form a continuation of that of Angström and Thalen. It includes, in common with their map, the portion from *h* to *H*, but contains about four times as many lines. Compared with the plate of Mascart, it has the double advantage of being a normal map, that is, the position of the rays are proportional to their wave-length, and that the general appearance of the lines is represented more faithfully. Three series of photographs were taken with a ruled plate of Nobert, a flint-glass prism and a prism of Iceland spar, using the ordinary ray. From the first of these, Cornu determined the wave-lengths of 36 of the more marked lines which serve as a basis for the rest. The prismatic spectra are more distinct and of greater extent, and from them he has deduced over six hundred additional rays between *N* and *O*, or between the wave-lengths of 4100 and 3637. The agreement of the principal lines with the measurements of Mascart is complete.—*Bib. Univ.*, ccv, 62. E. C. P.

11. *Index of Refraction of thin plates.*—M. Wiedemann describes a new method of determining indices of refraction applicable in certain cases where the ordinary methods fail. The light of a gas-burner passes through the slit of a collimator and is rendered parallel. It then passes through a tank formed of plates of glass cemented together, and filled with oil of cassia. In this tank is immersed the plate of glass to be tested. It is fixed to the axis of a theodolite, whose limb serves to show the angles through which it is turned. The light is then dispersed by a prism and the spectrum thus formed is observed by a telescope. As oil of cassia has an index much greater than that of most kinds of glass, there may be produced a total reflection at the surface separating the oil and glass. Then on turning the plate, at first normal to the light, either to the right or left, a gradual extinction of the spectrum is effected, commencing with the more refrangible rays. The spectrum is extinguished as if a black curtain was drawn over it, the edge indicating the limit of the total reflection. Calling a the angle between the two symmetrical positions of the glass corresponding to the limit of extinction of a particular ray, sin $\frac{1}{2}a$ will be the index of refraction of the oil with regard to the glass. Multiplying this by the index of the oil gives the index of the glass. The thinner the plates the more exact are the results given by this method. The index of refraction of liquids may be determined by a similar method. Two plates of glass are cemented together, leaving between them a thin layer of air. When this is too thin the limit of total reflection is not clearly marked, because a portion of the light penetrates the layer of air and enters the second plate. If, on the contrary, the layer is too thick, the limit of total reflection also loses its clearness, in consequence of the numerous interference bands. By interposing sheets of mica, whose thickness is gradually reduced, the best thickness is readily obtained. On turning the plates so that the angles of incidence become smaller and smaller, starting from a position a little beyond that of total reflec-

tion, interference rays are observed which pass over the spectrum from the red toward the blue. The angles of incidence having become smaller than the limiting angle, the direction of the displacement of the rays alters, and they move from the blue toward the red. These last are the interference rays ordinarily produced by thin films.—*Bib. Univ.*, cciv, 340. E. C. P.

II. GEOLOGY AND NATURAL HISTORY.

1. *The Elevation of certain Datum-points on the Great Lakes and Rivers and in the Rocky Mountains;* by JAS. T. GARDNER, Geographer. Washington, 1875. (Annual Rep. U. S. Geol. and Geogr. Survey of the Territories, for 1873, pp. 629–659.)—As Geographer in the Rocky Mountain Expedition under the charge of Dr. F. V. Hayden, Mr. Gardner found it necessary to fix upon some datum-point to serve as a base for the reckoning of altitudes, and met with a first difficulty in the different altitudes assigned to Denver, Colorado, they diverging between 200 and 300 feet. To eliminate the error, he undertook the "reconstructing of all possible lines of levels from the ocean to the Rocky Mountains, using only official reports by engineers, and checking them by personal examinations of their note-books and working profiles whenever practicable." The chapter here published contains the results of his long labor. Difficulty was found in the fact that railroad surveys from the Atlantic border had not started from the same stage of tide level, in errors from careless computation, and from other causes. The final results have proved that the elevation of the Great Lakes and the surrounding country is about *nine feet* more than previously reported by the State Geologist of Ohio; that St. Louis is 23 feet higher than made by Humphreys and Abbott; that Kansas City and the surrounding country for many hundred miles south and west is more than 100 feet higher than heretofore reported; Omaha 31 feet higher; Indianapolis about 100 feet. The following are a few of the levels ascertained:

Mean level of Lake Ontario above mean tide level	249·99 ft.
" Lake Erie	573·08
" Lake Huron	580·99
" Lake Michigan	589·15
Low water in Ohio at Cincinnati	440
Cairo city base, ordinary low water	291·23
Saint Louis directrix	429·29
Omaha, low water base of U. P. R. R.	977·90
" Depot grounds	1,060·40
Denver, Col., O. P. & K. P. R. R. passenger depot	5,196·58
Cheyenne, U. P. passenger depot	6,075·28
Golden, Colorado	5,728·98
Ogden, Utah, depot track	4,303·30
Pike's Peak	14,146·68

The level of mean tide at Albany, N. Y., above mean tide at New York City, was taken at 4·84 feet, as ascertained by the Coast

Survey. The mean height of Lake Erie was arrived at by means of levelings (1) on the Erie Canal from Albany to Buffalo; (2) from Philadelphia by railroad through Harrisburg, Pittsburg, Crestline to Cleveland; (3) from Albany by railroad to Buffalo; (4) by the North Central Pennsylvania R. R.; (5) by the Erie R. R. to Dunkirk and Cleveland; (6) by the Canadian Grand Trunk R. R. The results obtained by these six routes respectively are the following: 573·08, 572·037, 572·670, 570·750, [581·20], 571·67 feet. This example illustrates the method used for obtaining the true elevation, and shows also the high degree of confidence that may be placed in them; and further that Mr. Gardner makes in his memoir a contribution to North American topography of extreme importance.

A few others of the heights ascertained are: Quebec, mean tide level, 15·37 feet; Montreal, summer water-level, 30 feet; Lake Champlain, mean level at Whitehall, 100·84; Pittsburg, Pa., low water in river, 699·20; Louisville, Ky., low water above Falls, about 404; New Albany, Ind., low water in 1857, 379·75, and the depot of L. N. A. & C. R. R., 451·75; Rock Island, Ill., high water in Mississippi in 1852, 566·68; Terre Haute, Ind., high water in Wabash, 485·55, and ordinary water, 467·45; Mount Lincoln, Colorado, 14,296·66 feet. J. D. D.

2. *Geological Survey of Canada*, Alfred R. C. Selwyn, Director. *Report of Progress for the year* 1873–4. 268 pp. 8vo. Montreal, 1874. (Dawson Brothers.)—No annual Geological Reports on this side of the Atlantic exceed in value those of the Canada Geological Survey. The volume recently issued contains, besides an introductory report of 16 pages by Mr. Selwyn, observations in the Northwest Territory, from Fort Garry to Rocky Mountain House, by the same; Report on the country between Red River and the South Saskatchewan, with notes on the Geology of the region between Lake Superior and Red River, by Mr. R. Bell; Report on Geological explorations in British Columbia, by Mr. James Richardson; Report on explorations and surveys in Frontenac, Leeds and Lanark Cos., with notes on the graphite of Buckingham and apatite of Templeton and Portland townships, Ottawa Co., by Mr. H. G. Venner; Reports on the survey of the Coal Field of Cumberland Co., Nova Scotia, by Mr. S. Barlow and Mr. W. McQuat; on explorations and surveys in Cape Breton, Nova Scotia, by Mr. C. Robb; on the iron ores of Canada and their development, by Dr. B. J. Harrington; and notes on the Cretaceous fossils collected at Vancouver and the adjacent islands, from the coal series, by Mr. J. F. Whiteaves. A Report by Mr. Billings on fossils from Gaspé, and another by Mr. Weston are in progress; and also a Report by Prof. Bailey and Mr. Ells, on New Brunswick Geology.

Mr. Selwyn states, in his Introduction, that the facts gathered tend to prove that *lithology* among crystalline rocks is not a safe test of geological age, observing that this conclusion is favored by the recent researches of Mr. Richardson in British Columbia, which show that "epidotic, chloritic and serpentinous rocks, with crystalline limestones and magnetites, are as characteristic of Upper Paleozoic, and perhaps also of even later formations, where they have been subjected to an equal amount of plication and folding, as they are of the oldest Paleozoic and protozoic strata, such as those of Eastern Canada and the New England States." He observes that Sir William Logan has been carrying forward extended and thorough investigations in Canada on this point, connected with a study of the true stratigraphical relations of the rocks, and that he will give a full report as soon as "the necessary examinations have been completed."

The Report also states that *boring for water and coal* in the Northwest Territory with a diamond-pointed drill was undertaken the past year, but that there was not time after receiving the apparatus for obtaining any important results. The boring will be resumed this season.

In his Report on the Northwest Territory, Mr. Selwyn states that, going westward from Fort Garry (or Winnipeg), the capital of Manitoba, lat. 49° 52′ N., long. 96° 50′ W., *only drift*—sand, clay and gravel—was seen along their course for a distance of 885½ miles. The first terrace plain in that direction has an average breadth of 120 miles, is 75 to 100 feet above Lake Winnipeg, and but 700 or 800 feet above the sea level. The drift of its surface is apparently underlaid at no great depth by Silurian and Devonian strata. Its western limit is marked by a range of low hills extending from Pembina Mountains by the Riding, Duck and Porcupine Mountains, to the Basquia Hills. Here there is a rise to a *second* prairie level, which is undulating or hilly and "attains an average elevation of 1600 feet." A *third* prairie level commences at the Thickwood Hills, 20 miles west of Carleton; it is 1900 to 2000 feet above the sea; the limestone boulders on it do not reach farther west than the longitude of Fort Pitt. Sand is the material of the deposits, and it is mostly unstratified, becoming imperfectly stratified and calcareous to the westward. At Fort Ellice, the valley of the Assiniboine (as stated by Dr. Hector) is 240 feet deep: the upper 100 feet consist of this drift deposit, and the rest of Cretaceous beds.

The watershed between the Qu' Apelle to the southwest and the Saskatchewan and Assiniboine to the northwest, is mostly a great salt plain, with many pit-holes and some large salt lakes.

On the North Saskatchewan River, between Edmonton and Rocky Mountain House (138 miles), several horizontal *coal beds* were observed, one of them 18 to 20 feet thick, and favorable for working. The coal is hard and bright, jet-like, and much superior to that found farther south along the boundary line and in the Qu'Appelle valley. The age of the latter is pronounced probably Tertiary; of the former, it is stated to be yet uncertain. The plants in the two are the same. The coal-field is stated to extend at least to the Athabasca River on the north, and Red Deer River on the south, and to have an area of 25,000 square miles. Analyses of the coals by Dr. B. J. Harrington are given on pages 63, 64.

Mr. Bell states that the valley of Red River from Fort Garry to Lake Winnipeg is underlaid by Lower Silurian magnesian limestone, and that above are Devonian rocks, which are exposed on various points and islands in Lakes Manitoba and Winnipegoos. Coal was found among sandy and clayey strata in the Dirt Hills, 96 miles southwest of Fort Qu'Appelle, and at the Woody Mountains in the intermediate region. At the latter place, where the formation is 200 feet thick, eight seams were counted, one of them eight and another five feet thick. Mr. Bell, adopting the view that the coal-bearing strata above the *Inoceramus* shales and sandstones of the Souris are probably Tertiary, states that the line between the Tertiary and the Cretaceous formation enters British territory in the neighborhood of the Roche Percè, and thence runs northwestward to the Elbow of the South Saskatchewan, passing about 40 miles north of the Dirt Hills. The greater part of the area between the two branches of the Saskatchewan is occupied by the "coal-bearing Tertiary." The coal beds are partly true lignite.

With regard to the *Drift*, Mr. Bell states that *the direction of the striæ* to the north and northwest of Lake Superior is in general *southwestward*, and that the same course prevails as far east as the Ottawa River; and "there is little doubt that the same course prevails for some distance to the north and west of Lake Winnipeg; for the plains west of the lake and of West River are covered with the debris of rocks derived from the east of the lake."

These observations on the scratches northeast of Lake Huron confirm the statements of others, and sustain the conclusion of the writer therefrom*—that the greater height of the ice-surface in Canada was over the watershed between the St. Lawrence and Hudson Bay. The facts from the vicinity of Lake Winnipeg show further that a line of *greatest height* was continued from this region northward or northwestward. It does not follow that the height along this line to the northward was as great as over the watershed; but only that the greatest height for those latitudes was there, or that it was great enough to determine a southwestward movement in the ice. The greatest height would have been where there was the greatest amount of precipitation, supposing the melting the same; and this, as the writer observes in the same article,† would have been toward the southern and coastward limit of the glacier. Over the interior of the continent, both the small amount of precipitation and the high degree of heat in summer would have put the southern limit of the ice north of the limits of the United States. The summer isotherm of 72° F., which passes over southern Ohio—the south limit there of the drift,—extends in the interior, about the meridians 98° to 105°, northward to latitude 47°; and over this area and also its extension far north and likewise far west to a line passing by Sierra Nevada and the Dalles in Oregon, the amount of annual precipitation, according to Schott's Rain Chart, is *sixteen inches and less*.

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* This Journal, III, ii, 324; v, 204.

† Ibid, v, 206, 208.

Mr. Harrington's report on the rocks and minerals is an important one. It contains analyses of oligoclase, hornblende, magnetites, hematites, limonites and other species, besides observations on the associations of the species. J. D. D.

3. *An Essay concerning important Physical Features exhibited in the Valley of the Minnesota River and upon their signification;* by G. K. Warren, Major of Engineers and Bvt. Maj. General, U. S. A. 22 pp., 8vo., with several maps. Engineer Dept., U. S. Army.—Major Warren draws attention, in this Report, to the width and extent of the valley of Minnesota River (one of the head tributaries of the Mississippi) a feature continued even to its source in Lake Traverse, and to the great disproportion between its size and that of the stream now occupying it. He next states facts, derived from Prof. H. Y. Hind and other observers, respecting the great extent of the level region and stratified drift deposits about Lake Winnipeg, and the uninterrupted continuation of the deposits to Lake Traverse. The conclusion deduced is that in the Glacial era the land to the north about Lake Winnipeg was more elevated than now, so that the drainage from that lake was to the south, instead of, as now, through Nelson's River, to Hudson Bay; and that Lake Traverse was really the south end of Lake Winnipeg, and Minnesota River the outlet by which the great flood of waters passed south to the Mississippi. The Mississippi River would then have had its head waters in the streams that enter Lake Winnipeg from the west and north.

The highest part of the region about Lake Traverse is 995 feet above the sea, while that of Lake Winnipeg is about 630; so that a rise of but 365 feet to the north would produce the change of course. He observes that the height of the lake terrace above the ancient bed west of Pembina, as given by Mr. Owen, is 210 feet; that the lake bed there is about 50 feet above Red River, and this river at this point is about 100 feet above Lake Winnipeg; and hence he adds, "this lake terrace is 360 feet above the level of Lake Winnipeg—a height sufficient, as I have shown already, to extend the present Lake Winnipeg to Lake Traverse." Professor Hind, as quoted, says that "the Pembina Mountain [but 210 feet high] is *par excellence* the ancient beach in the valley of Lake Winnipeg. Dr. Owen thus describes it as it presents itself a few miles south of the 49th parallel."

When the southward slope of the surface came to its end through a northern subsidence, Nelson's River became the outlet—a narrow gorge abounding in water-falls. J. D. D.

4. *An examination of the Theories that have been proposed to account for the Climate of the Glacial Period;* by Thomas Belt, F.G.S. (Quart. J. Sci., Oct., 1874.)—After a discussion of the merits of various theories for accounting for the cold climate of the Glacial period, Mr. Belt argues strongly for the old theory of a change in the obliquity of the ecliptic. He admits that the decision of astronomers is against it, but he holds that "until astronomers have reconsidered this question with the light of our

present knowledge of the figure of the earth, geologists should not be prevented from speculating on the probability of great changes in the obliquity of the ecliptic having caused former great variations of temperature." Mr. Belt also touches on the lowering of the sea-level by the accumulation of ice over the land, and says that, on the supposition that the ice was equal in the two hemispheres at the same time (the view he sustains), the lowering of the sea-level "could not have been less than 2000 feet, and may have been much more."

Since any lowering of the water line of the ocean by the abstraction of water to make the Glacial ice would have been alike on all the coasts of the world, Mr. Belt's estimate of the amount in the Glacial period is open every where to demonstration. On the Atlantic border of North America, such a change—which is equivalent to a rise of the land of 2000 feet—would have led to an immense amount of valley-excavation along the many rivers; and such valleys, so recently made, should still intersect the alluvial, Tertiary, and Cretaceous deposits of the very wide coast region of the continent, or be but partially obliterated. Instead of this, this coast region is remarkably flat and even and is intersected only by narrow river channels. There is nothing comparable in any part to the fiords of Glacial latitudes; and, more than this, there is little evidence of recent excavation very much below the present sea-level. The same fact is true of other continents.

A source of cold not considered by Mr. Belt is suggested in the writer's Geology (new edition), viz: that there was *an elevation of Arctic lands sufficient to exclude the Gulf Stream from the Arctic regions.* If these tropical waters—which now distribute over the northern latitudes, according to Mr. Croll, 154,959,300,000,000,000,000 foot-pounds of energy in the form of heat per day—were thus prevented from making their circuit over the polar regions north of the continents, the flow northward by Iceland would be stopped, and, owing to the northeastward trend of the American coast from Florida to the southeastern cape of Newfoundland, the Gulf stream would be mostly confined to a circuit within the tropical and warm-temperate zones.

This cause, in addition to the direct cold-producing effect of an increased amount of Arctic land, would thus have been sufficient to produce all the lowering of temperature needed for a Glacial period. And it would have made an epoch of cold in any period of the globe. Moreover, the deep submergence of Behring's Strait (now but 180 feet in depth), and of the region around, by letting in the Pacific tropical current, would at any time, as in the Miocene period, have made a warm Arctic. This explanation of the warm Arctic climate in the Miocene, which is presented in the Geology,* was suggested to the writer, as there stated, by Professor F. H. Bradley; and the other respecting Glacial climate may be considered a corollary from it.

Professor A. E. Verrill has recently called the attention of the writer to another effect from this confining of the Gulf Stream to the Atlantic: that the heat which is now carried to the north would then be kept mainly within the warm temperate and tropical latitudes of the ocean, and that, consequently, the other requisite of a Glacial era, *very abundant precipitation, would have existed concurrently with that of unusual cold over the land*—conditions, as Professor Verrill states, that would make a storm region of tremendous effects over the ice within 300 or 400 miles of the warm sea limit. All the conditions demanded are therefore met by this theory.

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* Manual of Geology, 2d edit., p. 541.

The high-latitude changes of level here appealed to may have produced effects at intervals through all geological time, though of feeble intensity in the Paleozoic. Agassiz, in 1840, in his *Études sur les Glaciers*,—a work that made an epoch in geology by its demonstrations of the glacier origin of the Drift,—suggested also that the principal epochs of life-extermination in geology were epochs of cold—" que les plus grands froids ont terminé chaque epoque géologique,"—cold being a cause of extermination; and there is reason to believe that he was right as to some at least of the epochs. The writer recognizes two such in his Geology, one at the close of the Carboniferous, and another at the close of the Cretaceous (the latter in the 1st edition of the work); and he alludes to the subject here in order to give the credit of first bringing forward the principle to Agassiz. " The indications of floating ice which Ramsay has found in the British Lower Permian may be a mark of the slow approach of such an era of cold"* at the close of Paleozoic time; and the Triassic rocks of eastern North America seem to bear, in their large rounded stones and the nature of the beds, evidence that the excessive precipitation and also the cold of such an epoch had then not yet passed.† J. D. D.

5. *On the Submergence during the Glacial period.*—Professor Croll, in the Geological Magazine for July and August, 1874, presents the following as the cause of the apparent subsidence, or rise in the water level, which took place before the disappearance of the ice of the Glacial period; or, as he has it, the submergence during the Glacial period, he holding that the whole period was a period of lower level of the land than now. He proceeds on the supposition, before argued for by him, that the cold of the Glacial period was due to the eccentricity of the earth's orbit, and that it affected the northern and southern hemispheres alternately.

The removal of two miles of ice from the Antarctic Continent would displace the center of gravity 190 feet, and the formation of a mass of ice of one-half this thickness in the Arctic regions would carry the center of gravity 95 feet farther in the same direction. Further, the area of the Antarctic ice-cap being $\frac{1}{23 \cdot 46}$ of that of the ocean, therefore, allowing 0·92 for the density of ice, the melting of 25½ feet of ice from the cap would raise the general level of the ocean one foot; and a mile of ice, 200 feet. The other mile of ice melted is balanced by the abstraction of water to add a mile of ice to the Arctic. These numbers added, says Mr. Croll, give 485 feet for the amount of submergence that would be thus occasioned at the North Pole, and 434 feet for that in the latitude of Edinburgh; or if the ice had twice the supposed thickness, the amount of submergence would have been twice this amount.

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* Manual of Geology, 2d edit., p. 431.

† The agency of ice in the deposition of the Trias of Eastern North America has been suggested by T. A. Conrad, and also by H. Wurtz, and by both in 1869.

The facts from Eastern North America do not appear to favor Mr. Croll's conclusion. The amount of subsidence indicated by raised shell-beds on the St. Lawrence at Montreal, near latitude 45°, was about 500 feet; on the coast of Maine, near latitude 44°, only 200 feet, when it ought by the theory to have been but a few feet less than at Montreal; and along the southern shore of New England, near latitude 41°, only 50 to 100 feet, the larger of which numbers is much less than a fourth of what the theory requires. It seems hence to follow that the curve of submergence did not correspond with the curve which would have been produced in the ocean's surface by a change of the center of gravity.

The Coral Island subsidence over the Pacific, which affected a very large portion of the tropical part of that ocean, must have been in progress through the Glacial period, and the amount of it was certainly several thousand feet. Here was a great change of level *in the tropics*, which no change in the earth's center of gravity could have produced. It was in a sense local, the continental lands adjoining not sinking with the ocean's bottom, nor all parts of the ocean alike. It is evidence that the cause of the great change of water level during the Quaternary age, as well as in earlier time, resides in the crust itself. The facts from the shell beds of the St. Lawrence and the coasts of New England appear to be equally evidence of a *bona fide* subsidence, which no cause affecting the ocean as a whole will account for. J. D. D.

6. ***Recent Changes of Level on the Coast of Maine, with reference to their origin and relation to other similar changes;*** by N. S. Shaler. (20 pp., 4to, 1874, from the Mem. Bost. Soc. Nat. Hist., ii, 322.)—The evidence of the changes of level here discussed was gathered from the Quaternary deposits of Maine and of the coast south. Professor Shaler concludes that the evidence sustains the view that there was a subsidence or "depression" of the land increasing in amount, though somewhat irregularly, to the northward, after "the first division of the Glacial period;" and that subsequently there was "a return of the ice in the shape of a set of local glaciers which covered the shore at Mt. Desert, and along most of the territory at least as far as New Brunswick, and persisting until the final re-elevation of the land to near its present level."

The cause of the depression is discussed in the closing pages. He remarks that the only hypothesis as yet advanced to account for it is one proposed by Adhémar; overlooking the fact that the writer, first in 1856,* offered another, referring the great Quaternary movements of the crust,—the *upward* for the Glacial period, the *downward* for the Champlain or Fluvial period, and then a succeeding *upward* rise—like the other great movements, to the lateral pressure in the earth's crust due to contraction from cooling.

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* Amer. Jour. Sci., II, xxii, 346. The same conclusion is involved in his earlier paper in 1846, 1847.

Adhémar's hypothesis, as explained by Mr. Shaler, supposes a displacement of the center of gravity in consequence of the unequal accumulation of ice at the poles, much like that recently proposed by Mr. Croll. Professor Shaler rejects this explanation, chiefly because the Glacial phenomena of the two hemispheres have not yet been proved to be alternate, and because the amount of depression does not increase with regularity over the whole northern hemisphere; and then presents his own hypothesis, which attributes the depression to *the weight of the ice-mass over the land.* After speaking of the continents as having their positions determined by "constant tensions" (meaning, perhaps, to include *lateral pressure*, which far exceeds tensions in importance) and the weight of the mass, he says, "it seems evident enough that we may more reasonably look to the weight of the ice accumulated on the continents during the Glacial period for the depression of the land-areas it occupied, than to any other cause."

This theory assumes that a cap of ice "a mile or more thick," equivalent, as the author states, to half this thickness in "ordinary rock," depressed the earth's crust 500 feet and upward over the area from the St. Lawrence and great lakes to the Arctic, notwithstanding that the arched crust had a thickness of probably 100 miles. There is no doubt that such a weight—equal to a cap of rock, say one hundredth as thick as the earth's crust—would tend to cause a depression, for this tendency exists even under any small weight. But, considering the form and nature of the crust, and the lateral or tangential pressure existing throughout it, shown by Mallet to be enormous, it seems to be far from probable that mere weight can account for so great a movement. Lateral pressure has been the chief agent in oscillations of level and mountain-making over the earth; since Archæan time, making (1) the Green Mountains, (2) ridges in Nova Scotia and New Brunswick, and (3) the Alleghanies, on the *eastern* border of the North American Continent, at long intervals; and *subsequently*, after other long intervals, raising (1) the lofty Wahsatch Mountains and Sierra Nevada, (2) ranges of Cretaceous mountains, and (3) Tertiary ridges, on the western side of the Continent; besides (4) elevating, during the Cenozoic, the great Rocky Mountain mass 8,000 to 10,000 feet, —an effect that no weighting was employed to produce. And even in the Quaternary, though perhaps the work was begun before, there was the great subsidence over the tropical Pacific—the coral island subsidence (and another similar, probably over the tropical Atlantic), which the weighting of the islands by coral growths was certainly inadequate to produce, and which is naturally another consequence of the uneasiness of the crust owing to lateral pressure.

Here are vast results — elevations and subsidences — accomplished by lateral pressure. Now, that elevation of the land over the higher latitudes which brought on the Glacial era, is a natural result of the same agency, and a natural, and almost a necessary, counterpart of the coral island subsidence, which must have been then in progress. The accumulating, folding, solidification and crystallization of rocks attending all the rock-making and mountain-making through the Paleozoic, Mesozoic and Cenozoic eras, had greatly stiffened the crust in these parts, and hence, in after time, the continental movements resulting from the lateral pressure necessarily appeared over the more northern portion of the continent, where the accumulations and other changes had been relatively small. To the subsidence which followed the elevation the ice-cap may have contributed in some small degree. But the great balancing movements of the crust of the continental and oceanic areas then going forward must have had a greatly preponderating cause in the oscillating agency of all time, lateral pressure within the crust. J. D. D.

7. *Age of the Lignitic Coal Formation of Vancouver Island.* Letter to the editors, from Alfred R. C. Selwyn, F.R.S., Director of the Geological Survey of Canada, dated March 3d.—I wish to record my dissent from the statement made by Professor Lesquereux, page 365 of Dr. Hayden's Report on the U. S. Geological and Geographical Survey of Colorado, 1873, to the effect that the coal of Nanaimo, Vancouver Island, is referable to the lower American Eocene.

Careful surveys have now been made by the Canadian Geological Survey of the Nanaimo coal basin, and it is proved beyond the possibility of a doubt that the coal beds there are overlaid by a succession of strata, shales, sandstones and conglomerates, having a thickness of nearly 4000 feet, and holding from base to summit marine Cretaceous fossils, *Ammonites*, *Baculites*, *Inocerami*, and others.

Maps and sections showing the relative position of these beds and of the coal seams are given in the Report of the Geological Survey of Canada, 1872–'73; and I would beg to refer Professor Lesquereux to them for information concerning the coal rocks of Vancouver Island. As he makes no reference to the report named, I conclude he has not seen it.*

8. *Note on the genus Opisthoptera* Meek, 1872, *and Anomalodonta* Miller, 1874. (Communicated.)—Mr. S. A. Miller, in a reply to my note on the above mentioned genus,† published in the September number (1874) of the Cincinnati Journal of Science, endeavors to defend his substitution of the name *Anomalodonta* for Meek's earlier name. This he does on the ground (1) that *Megaptera*, having been previously used for a genus of whales, could not stand; (2) that although Mr. Meek had subsequently

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* The facts are briefly mentioned from the Canada Geological Report for 1872–1873 in vol. vii of this Journal, pp. 517, 518, 1874.—Eds.

† See this Journal, viii, 218, 1874.

(but previous to the publication of Mr. Miller's name) proposed to substitute for it the name *Opisthoptera*, he did it only *provisionally*, and had not himself adopted it in the subsequently published Ohio report; and (3) that neither Meek and Worthen jointly, nor Mr. Meek alone, had fully defined the generic characters of their type.

No further notice would be taken of the matter, were it not that silence might be construed into acquiescence in views and practices that it is the interest of all working naturalists to discountenance. In the first place, it is by no means a *settled* question among naturalists that the same name may not be used for genera in different sub-kingdoms, or even in different classes of the same sub-kingdom. This is the practice of some of the best and most renowned naturalists. *Troglodytes* is constantly used for a genus of apes, and also for a genus of birds, and many other cases might be mentioned. I think, however, with Mr. Meek, that it would be better to change the latter name, but he seems to have deferred to the high authority mentioned in only provisionally proposing to change it in the case of *Opisthoptera.* That he did not himself formally adopt the latter name in the Ohio Report has no bearing on the question. By repeating there the previously published note on *Opisthoptera*, he manifestly repeats the proposal to substitute the latter name on precisely the same ground that he did at first, namely, in case it should be found generically distinct from *Ambonychia* and the name *Megaptera* should be objected to. But it would not have altered the case if he had there said nothing about it, or even if he had proposed to retract both *Megaptera* and *Opisthoptera* entirely. As soon as the name was published it became the property of science, and he had no more right to dispose of it than any other person.

Mr. Miller's argument that both *Megaptera* and *Opisthoptera* should be discarded because *full* generic descriptions of them were not published, is not, and cannot be, sustained by general usage. Even in recent zoology, where it is possible to ascertain clearly all the characters, no such rule is generally followed. Such a rule would be utterly inadmissible in the department of fossil shells, because of the exceeding rarity of specimens that are even approximately perfect; for even in the best specimens the most important features (as, for example, the internal markings and teeth in bivalves) are generally obscured. Consequently, many of the genera of the latter kind of fossil shells have been proposed mainly or entirely upon external characters. The rule would endanger the name *Anomalodonta*, because nothing is yet known of the pallial line and pedal muscular scars of that shell, to say nothing about the extraordinary position of the adductor impression in Mr. Miller's figure. Hundreds of cases might be mentioned of genera established upon external characters.

Unlike some other genera, *Opisthoptera* has its more conspicuous characters external, namely, its form and surface ornamentation, which when taken together are quite sufficient to distinguish it from related types, even from the broad types of *Myalina*, from which the hinge characters shown by Mr. Miller would not separate it. As Meek and Worthen, and still later, Mr. Meek alone, have fully described and illustrated these external characters of their type, they have given sufficient means for its identification, which even Mr. Miller found no difficulty in doing. To Mr. Miller's other remarks it is hardly necessary to reply.

As to the affinities of *Ambonychia* with the *Aviculidæ*, which Mr. Miller refuses to admit, on account of the equality of the valves, I need only to remark that to make such family distinctions as that proposition, would require us to divide well defined genera, some species of which would fall in one family and some in another, the genus *Inoceramus* being an example. The true (Triassic typical) *Monotis*, *Halobia*, and others are included by the highest authorities among the Aviculidæ, and yet they are equivalve. He refers to McCoy as the first to refer *Ambonychia* to the Aviculidæ, being evidently unaware of the fact that in 1833 Goldfuss in his great work, *Petrif. Germ.*, did the same. Conrad, Woodward, Brown, Dr. Carpenter, and Dr. Stoliczska all have done the same, the latter even placing it near *Avicula* in the section Aviculinæ.

Returning to *Opisthoptera*, I will state its synonymy thus:

Genus OPISTHOPTERA Meek. *Megaptera* Meek and Worthen, 1866 —not *Megaptera* Gray, 18—. *Opisthoptera* Meek, 1872. *Anomalodonta* Miller, 1874. C. A. W.

9. *Costa Rica Geology.*—Mr. W. M. Gabb, in a letter dated San José, Costa Rica, February 1st, corrects his former calculation of the height of Pico Blanco, stated on page 199 of this volume, making it 11,877·8 feet. The error came from a slip in the use of a formula.

Mr. Gabb also states that on the 26th of January he ascended the volcanic peak Irazu, and obtained barometrically for its height 11,356·5 feet. The volcano is extinct, excepting a little smell of sulphur in the crater. He further remarks that the peak of Turrialba, nine or ten miles from Irazu, is so nearly equal to the latter in height that much difference of opinion exists as to which is the highest. This volcano "sends out a constant small cloud of vapor."

10. *The Gulf of Mexico in the Miocene.*—Prof. Hilgard has suggested, in view of the absence of marine Miocene beds over the Eocene of Louisiana, Mississippi and Alabama, and the presence there of the beds of the Grand Gulf group without remains of marine life, that the Mexican Gulf was fresh or only brackish in the Miocene, and made so by the elevation of a barrier across from Yucatan.* Mr. W. M. Gabb, in letters to the writer, states that his observations among the West India Islands lead him to believe, in view of the distribution of marine Miocene beds, that "in the Miocene period, Cuba, like Jamaica and Santo Domingo, was much smaller than at present, and may even have

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* This Journal, II, ii, 397.

been only a string of small islands; while Yucatan, the Bahamas, Bermudas and much of Florida did not yet exist, and their surface beds had not yet been deposited."

He remarks: "The additional facts I have acquired in Costa Rica geology go strongly to sustain the opinion I have already advanced, that no sedimentary rock older than the Miocene is found here. The Carboniferous formation is Miocene; and wherever it is unaltered it contains coal-beds, some of which are two yards thick. Now I have seen this coal, as the green streaks show, all the way around from near San José to the end of Talamanca, and we know that heavy beds occur in Chiriqui *on the Atlantic side.* And all along the coast, as at Boca Brava, Punta Uvita, Pirris, Tarcoles, etc., coal is found. The existence of these Miocene beds on the Atlantic side proves that the gulf was occupied by marine waters as now. J. D. D.

11. *On the Earthquakes of Southern Italy;* by EDWARD SUESS. 32 pp., 4to, with three plates. Vienna, 1874. (From vol. xxxiv, Denkschriften der Kaiserlichen Akademie der Wissenschaften.)—Professor Suess first describes with some detail the geological structure of Sicily, and of the neighboring districts of Italy, to the knowledge of which he has contributed so much by his personal labors. It is shown that in this region there is, in the crystalline rocks, a zone of granite and gneiss, and a bordering zone of schists, both more or less interrupted, an association closely resembling that so common in the Alps. The facts displayed warrant the conclusion, previously advanced by the author, that these older rocks of Southern Italy, with the isolated portions observed on the western coast, are the continuation of the Alps by Genoa, while the western side of the peninsula is to be regarded as an immense area of subsidence.

A careful discussion follows of the important earthquake phenomena which have been observed in Southern Italy. The main conclusion reached is, that the earthquake-shocks of Sicily and Calabria may be separated into three classes:

1. Eruptive-shocks, having their center in a volcano, and involving in general only the immediate region about the mountain, such as have often accompanied the eruptions of Mt. Etna.

2. Radial shocks, which also have their origin in a volcano, but are sent out from it along definite lines. Examples of these are afforded by the many earthquakes which have come to Southern Italy from the Lipari Islands.

3. Peripheral shocks, those which have not a volcano as their center, although there may be a certain connection between them and neighboring volcanoes. The line of these shocks may be traced from Cosenza southwest through Oppido and Reggio, and is probably continued through Mt. Etna toward Palermo, thus forming a complete semicircle concentric with the Lipari Islands. These shocks differ from the radial shocks in that they are propagated forward or backward on this line, so that they may visit the same place within a short time from different directions.

Another line of shocks is shown to have the direction from Mt. Vultur southwest through Potenza to Papasidero.

From the facts presented, Professor Suess shows that there is a close connection between the volcanoes and the earthquakes. He concludes, moreover, that the volcanic phenomena of the Italian peninsula are parts of one system, and that they are to be regarded not as the cause of the mountain elevation, but as accompanying phenomena resulting from fractures that have been produced through other means. E. S. D.

12. *Distinctive optical properties of the Feldspars.*—DesCloizeaux has recently investigated the optical properties of the triclinic feldspars (C. R., lxxx, Feb. 8, 1875). The principal results obtained by him are contained in the following table, in which *Bx* stands for *bisectrix.*

	ALBITE.	OLIGOCLASE.	LABRADORITE	ANORTHITE
Acute bisectrix	always +	generally — sometimes +	always +	always —
Angle made by the + Bx. with a normal to $i\text{-}\breve{\imath}$ (g)	15°	18° 10′	30° 40′	Position of the Bx. has no simple relation to the planes observed on the cryst'ls.
Same, with a normal to O (p)	78° 35′	68°	56°	
Angle made by the line in which the plane of the optic-axes cuts $i\text{-}\breve{\imath}$, with edge $i\text{-}\breve{\imath}\,\vert\,O(g'\vert p)$	20°	Line parallel to the edge $O\,\vert\,i\text{-}\breve{\imath}$.	27°–28°	
Same, with edge $i\text{-}\breve{\imath}\vert I(g'\vert m)$	96° 28′(front)	" "	37°25′–36°25′	
Ordinary dispersion	$\rho<v$(+ Bx.)	$\rho<v$(+ Bx.)	$\rho>v$(+ Bx).	$\rho<v$(− Bx.)
Dispersion parallel or perpendicular to plane of polarization	*Inclined;* probably also slight *horizontal.*	*Crossed;* also slight *inclined.*	*Crossed;* also slight *inclined.*	*Inclined.*
Appar'nt optic-axial angle (in air)				
—— for red rays	80° 39′	89° 35′	88° 15′	84° 58′
—— for blue rays	81° 59′	88° 31′	87° 48′	85° 59′
	(Roc tourné)	(Sunstone. Tvedestrand)	(Labrador)	(Somma)

The axial divergence is quite constant for albite, labradorite and anorthite, but varies for oligoclase even in different sections taken from the same specimen. DesCloizeaux concludes from his observations that labradorite and oligoclase have an equal right with albite and anorthite to be considered independent species, contrary to the views presented by Tschermak. Andesite he concludes to be altered oligoclase, while tschermakite is identical with albite. E. S. D.

13. *Insects and other fossils from the Permian of Weissig in Saxony.*—Mr. EUGENE GEINITZ, in a second paper on the carbonaceous shale of the Permian at Weissig, near Pillnitz, mentions the occurrence there of four species of Blattina, one a nearly perfect specimen of the *B. didyma* Germar, another a new species, the *B. porrecta?* Geinitz, and the others the *Blattina Carbonaria* Germar and the *B. Mahri* Goldenberg. The same locality has afforded also, *Uronectes* (*Gampsonyx*) *fimbriatus* Jordan, *Estheria tenella* Jordan; the fishes, *Acanthodes gracilis* Beyrich, *Palæoniscus angustus* Ag., *P. Vratislaviensis* Ag.; the Articulate, *Gyromyces Ammonis* Göpp. (*Spirorbis Carbonarius*); and several plants of the genera

Annularia, *Asterophyllites*, *Sphenopteris*, ? *Hymenophyllites*, *Odontopteris*, *Neuropteris*, *Dictyopteris*, *Cyatheites*, *Alethopteris*, *Tæniopteris*, *Walchia*, besides some others.—*Jahrb. f. Min.*, 1875.

14. *Batrachians in bituminous shales of the Permian at Millery, France.*—M. A. Gaudry states that a single slab of shale from Millery, France, has on it seven little Batrachians, resembling the species which had been obtained by M. Roche at Igornay. He names the species from the two localities *Salamandrella Petrolei*, because of the affinities to the Salamanders, and their occurrence in oil-yielding shales. They are very small, the largest 35 mm.; and yet they were probably adults. No traces of scales were found. The fore and hind limbs are four-toed and nearly of the same size.—*Ann. Mag. N. H.*, IV, xv, 233.

15. *Coal in Patagonia.*—On the Peninsula of Brunswick, in the Straits of Magellan, at a place called Vaqueria by Capt. Corey, rich beds of coal have been opened. The place is not far from the Chilian Colony of Punta-Arenas, lat. 53° 10′ S. and long. 70° 54′ W. (from Greenwich). The Chilian Government has conceded it to a French company. A detailed report has been published by M. F. Arnal, Civil Engineer. The coal is very compact, black, inflames easily and burns without odor. There are three beds, having an aggregate thickness of about 26 feet. The age of the beds is not stated; but as the coal is spoken of as related to the lignites, it is probably Tertiary or Mesozoic.—*L'Institut*, Feb. 17.

16. *Icones Muscorum, or Figures and Descriptions of most of those Mosses peculiar to North America, which have not yet been figured;* by the late WILLIAM S. SULLIVANT, LL.D. *Posthumous Supplement;* with 81 copper plates. Imp. 8vo. Cambridge: Charles W. Sever. London: Trübner & Co. December, 1874.—To give an idea of the nature of this volume, and of the circumstances under which it has been produced and published, we append the brief preface, written by the author of this notice. His biographical sketch of Mr. Sullivant, published by the American Academy of Arts and Sciences, is prefixed to the volume. The mosses which form the subjects of the present series of incomparable plates, are partly Californian, from the indefatigable Bolander and others, or from the Rocky Mountains, &c., and partly new discoveries in the Atlantic States, by Messrs. James, Austin, Peck, Ravenel, &c. Only a small edition is printed and put on sale. Its price, as of the original volume, is fixed—at least for the present—in reference to the ordinary means of students and devotees of Bryology, rather than to the actual cost of the work. Even if all the copies were sold at the price ($10 for this supplement, and $14 for the original volume, which is still to be had), the gift of the lamented author to his fellow-botanists would yet be large. If desired, copies will be supplied from the Herbarium of Harvard University.

"Since the publication of the Icones Muscorum, in the year 1864, the lamented author made careful studies and sketches of the copious new materials as they came to his hands, with a view to a continuation. A portion of these was selected for the first Supplement, and the drawings and engravings of these eighty-three plates were completed before his death, in April, 1873. The drawings were made under his immediate superintendence, by the same meritorious draughtsman who executed those of the original volume, Mr. August Schrader, and they were engraved on copper by Mr. William Dougal. Some progress had been made in the preparation of the letter-press, but only a small portion of it was found to be in readiness for the printer. Its preparation and completion, as well as the oversight of the press, have devolved upon his friend and intimate companion in Bryological studies, Mr. Leo Lesquereux, to whom this has been a labor of love and the paying of a tribute of affection to an endeared memory. Inasmuch as he had to draw up a large proportion of the following descriptions from mere notes and remarks appended to the specimens in the herbarium, it would have been only just, no less to Mr. Sullivant than to Mr. Lesquereux, that the name of the latter should be placed upon the title-page as the editor of this posthumous work. But he has strenuously objected to this, and his decided wishes have been deferred to. Regarding the incomparable "Icones" and this Supplement as a fitting memorial of one who will rightly be remembered as the father of American bryology, he declines to have his own name inscribed upon the monument, however subordinately. Accordingly, it only remains for the writer of this preface to express—on the part of the botanists who are to be benefited, and of the Sullivant family, at whose expense this memorial is published—most grateful acknowledgements to Mr. Lesquereux for his valuable and essential services." A. G.

17. *Attar of Roses*, for European commerce, is almost exclusively supplied by a small tract of country in Rumelia, on the southern side of the Balkan Mountains. There has been some question as to what species of Rose was employed; but of late it has generally been thought to be the Damask Rose. Mr. Hanbury, having received a packet of specimens from the rose fields of the Balkan, submitted them to Mr. Baker of Kew, he being the most accomplished rosarian; and Mr. Baker has determined them to be for the most part ***Rosa damascena***. This species is quite unknown in a wild state; and Mr. Baker regards it "as most likely a cultivated race of ***R. Gallica***, which spreads in a wild state from France to Kurdistan." (See Jour. Bot. for Jan., 1875.) A. G.

18. ***British Wild Flowers considered in relation to Insects;*** by Sir JOHN LUBBOCK, Bart, F.R.S., M.P., with numerous illustrations. London: Macmillan & Co. 1875. pp. 186, 12mo.—Although the revival of this fascinating and fruitful subject, and the first clear exposition of its importance in the vegetable economy, are due to an Englishman—it being Mr. Darwin's happiest piece of subsidiary work—there has been no British text-book or popular treatise* to impart the requisite knowledge of the facts, and to

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* Except a little work by Mr. Bennett, entitled "How Flowers are Fertilized," which we have not seen.

excite and gratify the interest which these are sure to engender. There have been some excellent articles in the magazines; but Mr. Darwin's revelations seem not to have been followed up as they have been elsewhere, especially in Germany, where the literature of the subject is very considerable. In the United States it has become a part of popular botanical instruction,—not so generally, however, as it ought to be; for this, and other matters of plant-behavior, may be expected to double the charm of botanical study, by opening new fields of surpassing interest and almost endless variety. Nothing can be better adapted to sharpen the powers of observation in the young.

Sir John Lubbock's lecture before the British Association last August upon this topic attracted great attention, which his series of articles in *Nature* widened; and now these articles are collected to form this handy volume,—one of the *Nature Series*, to which the author had already contributed his more elaborate and thorough essay on "The Origin and Metamorphoses of Insects." As the author remarks, he "had made no serious study of Botany," at which we do not wonder. As it is, few indeed can be expected to give their powers, with the success he has commanded, to so many engrossing subjects, financial, ethnological, zoological and other. But as an entomologist, "the intimate relations which exist between flowers and insects," as brought to light by recent researches, came directly in his way. His observations and notes originally prepared—and as we judge very recently—"with the view of encouraging in my children that love of natural history from which I myself have derived so much happiness" (and we may add science no little advantage), naturally suggested that a wider use might be made of them; and this attractive, but rather hastily prepared volume is the result. Not much in the way of original observations is here to be expected; but there are some, and good ones. There are seven chapters. The two introductory chapters are the most important and satisfactory. They give an admirably clear account of the general subject, succinct, but sufficiently full to convey a good idea of the various ways in which most flowers are adapted to be cross-fertilized by the visits of insects, and through which these visits are secured, while others are in their way as well served by the winds. And there is an especially instructive account of the modifications of the mouth-parts and legs of bees and butterflies, for their profit in their visits to flowers, while they profit them. This is condensed from Herman Müller and the illustrations are his.

In the remaining chapters the orders of the principal British flowers are taken up seriatim, and the fertilization of a good number of them explained and illustrated,—sufficiently so in many cases, at least in the earlier orders; but, on the whole, if so wide a range and systematic treatment were undertaken, it were better to carry it out more fully. The little cuts from Bentham's Handbook, not having been prepared in view of this subject, are here of small use. They are supplemented by larger and clear illustrations of separate flowers, from Müller, Axell, Hildebrand and Darwin.

Now that we fairly well understand the meaning of these various arrangements for the fertilization of flowers, perhaps we ought to give to the first discoverer of them, Christian Conrad Sprengel, rather more credit than the present author awards him. If he did not perceive "the real significance of the facts which he had discovered," it was not because he, like his contemporaries, was "led to assume that each flower was created as we now see it." The meaning of the arrangements comes out as clearly upon that supposition as upon any other. Mr. Darwin puts the case better, when with his characteristic exactness, he says that "Sprengel failed to understand the full meaning of the structure of the flowers which he has so well described, from not always having before his mind the key to the problem, viz: the good derived from the crossing of distinct individual plants." The law of nature that organic beings shall not fertilize themselves in perpetuity, is the key to the whole; and for this key we rightly acknowledge our indebtedness to Mr. Darwin, who first turned it to full account, in a masterly way. Yet the principle involved was enunciated by Andrew Knight, in the year 1799. But even this was anticipated by Sprengel, in 1793, in the declaration to which his observations had led him, that "Nature does not wish that any complete flower should be fertilized by its own pollen." Kœlruter's equivalent suggestion is still earlier, and much antedates that of Knight (instead of following it, as Mr. Knight implies), if really to be found in his book published in 1761, as is lately stated. Like most first discoveries, these were not followed up, nor half their importance foreseen. A. G.

19. *Amphipod Crustaceans of the family Gammaridæ in Lake Baikal.*—Dr. B. N. Dybowsky has described *ninty-seven* species of Gammarids from Lake Baikal. They include one Swedish species, *G.* (*Pallassa*) *cancelloides;* and also the *G. neglectus* of the lake is hardly distinct from *G. pulex.* The species occur at all depths, the greatest depth dredged, 1373 meters, affording them as abundantly as the littoral zone, though fewer in species. The species of small depths are mostly vividly colored; those at greater depths are less bright in color, and the kinds from depths greater than 700 meters are more or less whitish in tint.—*Ann. Mag. Nat. Hist.*, IV, xv, 230.

20. *Large Cephalopod.*—A cephalopod "of great bulk," 12 feet in length, has been captured on the coast of Japan.—*Japan Mail*, Jan. 23.

21. *The Doctrine of Descent and Darwinism;* by OSCAR SCHMIDT, Prof. Univ. Strasburg. The International Scientific Series. 324 pp., 12mo, with 26 wood-cuts. New York, 1875. (D. Appleton & Co.)—This volume contains, in a brief dogmatic form, the views of Darwin on descent, combined with those of the more imaginative Hæckel. The author has nothing but matter in his philosophy, and is intolerant toward those who do not think with him.

III. ASTRONOMY.

1. *Venus.*—The Italian observers at Maddapore in Bengal, to which party the eminent spectroscopist Tacchini belonged, besides observing all four contacts, ascertained an important fact respecting the atmosphere of Venus. The ring around the planet, which in the former transits, as in the present one, was visible around Venus both on and off the sun, indicates in the spectroscope that the atmosphere contains aqueous vapor.—*Nature*, Jan. 21.

2. *A Society for Spectroscopic observations on the Sun* has been formed in Calcutta.—*Nature*, Feb. 25.

3. *Astronomical and Meteorological Observations made during the year* 1872 *at the U. S. Naval Observatory.* Rear-Admiral B. F. SANDS, U. S. N., Superintendent. Published by authority of the Hon. Secretary of the Navy. cvi, 372, 58 and 22 pp., 4to. Washington, 1874.—This new volume of the U. S. Naval Observatory opens with an account of the Observatory, the Transit Circle, with tables for correction and use, etc., and the Mural Circle, occupying over 100 pages. The following part of the volume contains the tables of the results of observations in 1872 with the Transit (160 pp.); with the Mural Circle (161–204 pp.); with the Equatorial (207–232 pp.); mean declination of stars, from individual observations with the Mural Circle (235–246 pp.); mean places of miscellaneous stars, from individual observations with the Transit Circle (249–268 pp.); Catalogue of stars observed with the Transit Circle (271–278 pp.); Right ascensions, N. Polar distances and semi-diameters of the Sun; Small Planets; Declinations of the Moon. It concludes with meteorological tables, and a report on the difference of longitude between Washington and Detroit, Michigan, Carlin, Nevada, and Austin, Nevada.

4. PROFESSOR ARGELANDER.—Friederich Wilhelm August Argelander, born at Memel in East Prussia on the 22d of March, 1799, died at Bonn on the 17th of February. He was for a while assistant in the observatory at Königsberg, under Bessel, afterward, in 1823, Director of the Finland Observatory at Abo and Helsingfors, and in 1834, Director of the Prussian Observatory at Bonn, where he continued his assiduous labors until within a short time of his death.

Professor Argelander's astronomical labors were principally in the field of fixed-star observing. In this they may be described as Herculean; his survey of the northern heavens including the observation in zones of more than *three hundred thousand* stars, the charting of which has been of the greatest service in many astronomical inquiries. Nor did he fail to compare many of his observations, being thereby led to several remarkable discoveries of proper motion and variability of light of certain stars. While in Finland, he made a determination of the motion of the solar system in space, with results nearly the same as those of Sir William Herschel. His careful and comprehensive estimation, too, of the comparative magnitudes of all the stars visible to the naked eye should be mentioned (Uranometria Nova).

The Royal Astronomical Society of London marked their sense of the value of Professor Argelander's sidereal labors by the award of their gold medal in February, 1863. His last observations, so far as we are aware, were of the bright comet of Coggia, in the summer of last year.—*Athenæum*, Feb. 27.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Engineer Department U. S. Army. Progress-Report upon Geographical and Geological Explorations and Surveys west of the* 100*th Meridian in* 1872, under the direction of Brig. Gen. A. A. Humphreys, Chief of Engineers, U. S. A.; by First Lieut. GEORGE M. WHEELER, Corps of Engineers, in charge. 56 pp., 4to. Washington. 1874.—According to this report, the work performed by the expedition under Lieut. Wheeler in 1872 extended over the astronomical, topographical, meteorological, geological and natural history departments. For the determination of latitude and longitude three parties were engaged, and for topographical observations, five parties. The area covered topographically during the year exceeded 50,000 square miles, and included part of Western and Southwestern Utah, Eastern Nevada, and Northwestern Arizona. This "Progress-Report" contains historical and descriptive accounts of various mining districts and mines within the region examined, observations on irrigation for the country, and on its agricultural resources, timber lands, routes for communication, and other points of interest.

The geologist, Mr. G. K. Gilbert, states that about White's Peak, in the Schell Creek Range, Nevada, in latitute 39° 15′ north, there are the terminal moraines of five or six glaciers, 8,000 feet above the sea-level; on Wheeler's Peak, of the Snake Range, Nevada, latitude 39° north, several moraines and an alpine lake; and on Old Baldy Peak, near Beaver, Utah, in latitude 38° 18′ north, two terminal moraines. The ancient extent of the great Salt Lake,—when it stood 900 feet above its present level as proved by its elevated beach lines,—is made 18,000 square miles, which is a little less than the area of Lake Huron. Mr. Gilbert observes that, in the plateau region of the Upper Colorado, the rock-system, ranging from the Tertiary to the Devonian, is comparatively undisturbed, and denudation has left the harder beds projecting in a succession of steps. Across these steps there is a series of parallel faults which divides the steps into limited tables, that are drained by tributaries of the Colorado and Sevier Rivers.

According to the scheme for the publication of the results of the Wheeler Expeditions, the Report will include six quarto volumes, and also one topographical and one geological atlas, each 19 by 24 inches. Volume IV of the series is to contain the geology; volume V the paleontology, which is to be illustrated by numerous plates of vertebrate and invertebrate fossils; and volume VI, the natural history of the expeditions.

Numerous excellent photographs have been taken, which will be used for illustrating the geological and other volumes. They represent grandly the scenery of the Colorado and of other parts of the region under survey.

2. *Smithsonian Contributions to Knowledge*, 1874, 4to. *Miscellaneous Collections.* Vols. xi, xii, 8vo, 1874.—The very valuable series of Smithsonian publications was enlarged by the addition of three volumes during the year 1874. The volume of Contributions contains Problems of Rotary motion presented by the Gyroscope, the Precession of the Equinoxes and the Pendulum, by Brevet Maj. Gen. J. G. Barnard; a contribution to the History of the Fresh-water Algæ of N. America, by H. C. Wood, Jr., M.D., with 21 plates, many of them colored; an investigation of the Orbit of Uranus, with general tables of its motion, by Simon Newcomb.

Vol. xi, of the Miscellaneous Collections, contains memoirs on the Arrangements of Mammals and Fishes, by Dr. T. Gill (200 pp.); monographs of N. A. Diptera, by H. Lœw (376 pp.); on collecting and preserving Insects, by A. S. Packard, Jr. (60 pp.); New N. A. Coleoptera, and classification of the Coleoptera, by J. L. LeConte (146 pp.).—Vol. xii contains a Review of American Birds in the Smithsonian Museum, by S. F. Baird (484 pp.); the Constants of Nature, Part I: Specific gravities, Boiling and Melting Points and Chemical Formulæ, by F. W. Clarke (272 pp.); on Telegraphic Announcements of Astronomical discoveries, by Joseph Henry (4 pp.).

3. *Chemical Examination of Alcoholic Liquors;* by Professor A. B. PRESCOTT. 108 pp., 12mo. New York. 1875. (Van Nostrand.)—Professor Prescott has supplied a want long acknowledged in our chemical literature by this concise and well arranged little manual. The special acts of legislation on the subject of alcoholic liquors and wines, their adulteration and falsification, which now slumber in the statute books of this country, have been framed with so little skill or with such bigotry or partizanship as to be practically inoperative. The importance to society that all articles used as foods, medicines, or beverages should be held subject to strict scientific examination by authority of law, and that the frauds now practiced should be systematically exposed and suppressed, is such as to admit of no debate. These frauds, pernicious to health or life, are by no means confined to alcoholic beverages. And the analyist will find assistance in meeting them, not only in this Manual of Dr. Prescott, but also through the use of the Outlines of Proximate Organic Analysis, by the same author, already noticed. B. S.

4. *Appleton's Cyclopedia.* —The American Cyclopædia: a Popular Dictionary of General Knowledge. Edited by GEORGE RIPLEY and CHARLES A. DANA. Vols. I—X, 1873–1875. Royal 8vo. New York.—It is now twelve years since the first edition of Appleton's American Cyclopedia was finished. It was followed in the eleven succeeding years by as many "Annual Cyclopedias," uniform with the original work, in sixteen volumes. Although much the larger part of these volumes is properly given to historical, political, civil, military and social affairs, including biography,

statistics, commerce, finance, and literature—science has by no means been neglected, as we have in successive years pointed out in our bibliographical notices.

The new edition of this work, of which ten volumes are now ready, was undertaken in 1873. No decade of this century has been marked by more rapid advances in science than that covering the interval between these two editions; and equally remarkable have been the social and political changes. To fuse all the new material, thus rapidly accumulated, with the old, into a homogeneous and compact unity, has required the recasting of the whole work. Of course a large number of writers and revisers have been busy in preparing the eight or nine thousand closely printed pages in the ten volumes already published. Many of these articles are brought down to date, with a completeness not found in the first edition; while, as far as we have examined the several volumes, the matter of former scientific and technical articles has been extensively revised with care and good editorship. The geographical maps introduced add greatly to the value of the Cyclopedia, and the wood-cut illustrations are numerous and well selected. A considerable number of names of authors, not before contributors to the Cyclopedia, now appear in connection with articles on various scientific topics. The authorship of some of the physical, chemical, geological, and technical articles is as follows: Count L. F. de Pourtalès, of the U. S. Coast Survey, Prof. J. E. Hilgard, Dr. Thomas M. Drown, Prof. Cleveland Abbé, Dr. Rossiter W. Raymond, Dr. T. S. Hunt, Dr. A. M. Mayer, Dr. Joy, and Dr. Charles L. Hogeboom. Many of the astronomical articles are from the pen of Mr. Richard A. Proctor, of England.

The tenth volume ends with "Magnet," by Cleveland Abbe.

B. S.

5. *Transactions of the American Institute of Mining Engineers.* Vol. ii. May, 1873, to Feb., 1874. 331 pp. 8vo, with 3 plates. Easton, Pa. Published by the Institute, at the office of the Secretary, Lafayette College.—This vigorous young society is doing good service in its published contributions of original memoirs on subjects appropriate to its purposes, many of them of permanent value, recording the results of methods and investigations of great interest to all concerned in such matters. B. S.

6. *U. S. Northern Boundary Commission*, A. CAMPBELL, Commissioner. *Natural History*, No. 1. *On the Muridæ*, by Dr. E. COUES, U. S. A., Surgeon and Naturalist of the Commission. Reissued with additions from the Proc. Acad. N. Sci. Philad., 1874. 28 pp., 8vo, 1874. Contains many valuable notes on the species.

Birds of the Northwest: a Handbook of the Ornithology of the region drained by the Missouri River and its Tributaries; by ELLIOTT COUES, Capt. and Assist. Surgeon U. S. A.—Miscellaneous publications 3, of U. S. Geol. Survey of the Territories, F. V. HAYDEN, U. S. Geol. in Charge. 792 pp. 8vo. Washington, 1874.

Texas Geological Report.—Mr. Buckley, in a recent letter to the editors, states that the heading of Devonian in his Report over a section treating of the Trenton formation (see p. 224 of this volume) was a printer's error. There is nothing in the Report to suggest this.—EDS.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

Art. XXXIV.—*Notices of Recent Earthquakes*, No. 5; by Professor C. G. Rockwood, Jr., Rutgers College.

Jan. 6, 1874.—A shock was felt at Wolfboro, N. H.

Jan. 14, 1874.—The village of Saru-Kamush, thirty miles east of Harpoot, Turkey, "was entirely destroyed, and a good many houses in villages near by were thrown down."

Jan. 19, 1874.—A slight shock at San Francisco.

Jan. 26, 1874.—Two shocks at Manchester, N. H., at 2 and 5 A. M., the first light, the second heavy.

Feb. 10, 1874.—There began a series of disturbances in Bald and Stone Mountains, McDowell County, N. C., which continued at intervals for several months. The phenomena appear to have been occasional earthquake shocks, at no time violent, but accompanied by explosive and rumbling noises, and occurring, sometimes two or three in a day, and again with intervals of several days. These increased in frequency and intensity until the night of February 22, when the most severe shock was felt. About March 17 and 26, the shocks were again of some intensity, as also on April 14 and 17. A correspondent of the New York Evening Post, writing from Spartanburg, S. C., under date of March 28, and having just visited the affected region, reported experiencing a decided shock, with a deep rumbling noise, about sundown of March 18, and another on March 19, these being all that he felt during a five days' visit. Another observer says "the sounds resembled the report made by blasting in a deep quarry or well, at first explosive and then reverberating." The shocks were most sensibly felt near the tops of the mountains, but were sometimes perceptible at distances of ten or fifteen miles, or even farther.

A paper by Gen. T. L. Clingman of North Carolina, on the volcanic character of this region, was read before the Washington Philosophical Society, July, 1874, and is noticed in this Journal, III, vol. ix, p. 55.

Feb. 12, 1874.—A shock at 6^h 30^m A. M. at Saco, Me.

Feb. 12, 1874.—Two shocks felt in the evening at Hilo, and also at Hamakua, Hawaii.

Feb. 15, 1874.—A shock at Copiapo, Chili.

Feb. 27, 1874.—A severe shock at 10^h 40^m P. M. at Bangor and Eastport, Me.

March 12, 1874.—A slight shock at Yarmouth, Nova Scotia.

March 12, 1874.—A slight shock at Sidney, N. S. W.

March 16, 1874. — An earthquake in Southern Mexico, stated to have been "very severe" in the province of Guerrero, but slight at the city of Mexico.

April 11, 1874.—The Japan Gazette of this date says "sharp shocks of earthquake had been felt at Hamaoa, causing great alarm."

April 18, 1874.—A shock at St. Thomas, W. I.

May 3, 1874.—Letters from the Harpoot Mission, Eastern Turkey, under date of May 8, say: "Last Sabbath morning (May 3), at seven o'clock, a severe shock of earthquake was felt here and in all the plain. Houses fell in several villages, but at Haboosi, in the eastern part of the plain, the effect was disastrous." Again, under date of May 16: "The village of Haboosi, twelve miles from here, with a population of 2,500, was reduced to a heap of ruins in one minute of time, and twenty persons were killed. Not more than three or four houses are left standing. The large flat earthen roofs were thrown upon adjoining buildings, and the ruins are so commingled that it is not easy for the inhabitants to tell where are the foundations of their own houses. In other villages the disaster has been very serious, and we fear the end is not yet. Shocks continue daily."

May 5, 1874.—A sharp shock about midnight at Callao, Peru.

May 14, 1874.—A series of severe shocks about 9 A. M. at Hilo, Hawaii, and the adjacent coast. "Simultaneously with this earthquake the crater of Kilauea became more brilliant and active."

May 24, 1874.—Two sharp shocks about 2 A. M. at San Francisco.

June 11, 1874.—Two sharp shocks at 8 P. M. at San Francisco. No damage done.

June 17, 1874.—A slight shock at 12 P. M. at Salt Lake City. "There were several vibrations, the shock continuing for about ten seconds. It was plainly felt at Alta and at Granite, where a rumbling noise was also heard."

June 18, 1874.—A shock at Guayaquil, Ecuador.

June 23, 1874.—A severe shock about $9^h\ 25^m$ A. M. at Hong Kong, China. "The rumbling and trembling of the earth lasted fully fifteen seconds." The harbor was much disturbed, bells were rung and two small houses were thrown down.

June 27, 1874.—"Strong shocks" at Constantinople, Turkey.

July 7, 1874.—A strong shock at $2^h\ 7^m$ A. M at Valparaiso, Chili.

July 9, 1874.—A slight shock about 4 P. M. at Cairo, Ill.

July 26, 1874.—A shock at Vienna, Austria.

Aug. 8, 1874.—A shock in the morning at Valparaiso.

Aug. 8, 1874.—A severe shock at noon at St. Thomas, St. Kitts and Antigua, W. I.

Aug. 17, 1874.—A slight shock at Demerara, Trinidad and Grenada.

Aug. 20, 1874.—A severe shock at Yokohama, Japan.

Aug. 26, 1874.—A severe shock in the morning at Puerto Rico. "Vibrations lasted two minutes; houses rocked."

Sept. 3, 1874.—A destructive earthquake occurred in Guatemala, centering near the Volcan del Fuego. Advices from Antigua state that, since the beginning of August, slight shocks had been felt, becoming more frequent toward the end of the month. From Aug. 27, the town of Duenos, and other places near the volcano named, felt shocks at short intervals, accompanied by subterranean noises. On the night of Sept. 3, about 9 P. M. (one account says $8^h\ 30^m$, another $9^h\ 18^m$), the most violent shock occurred. The movement was a series of vertical and horizontal impulses combined; the direction was west to east, as noted by a swinging lamp. The shock was stated by one observer as lasting twenty-five or thirty seconds; by another as continuing in its intensity during four seconds and then gradually diminishing. Sounds like thunder were heard from the earth. Many other less violent shocks were felt during the night, the principal one at 2 A. M. In Antigua the church bells were rung by the vibration, about two dozen inhabited houses were destroyed and thirty-two lives lost. The town of Duenos was entirely ruined, and some persons were killed there and in neighboring villages. An eruption of cold compact mud issued from the heights of Cerro del Tigre, a small mountain at the base of the Volcan del Fuego, and which appears to have been the center of disturbance.

Sept. 26, 1874.—In connection with the eruption of Mt. Etna, which began Aug. 29, an earthquake occurred extending to the village of Randozza, and destroying several houses.

Oct. 7 and 12, 1874.—Severe shocks and subterranean noises near Mazatlan, Mexico.

Oct. 26, 1874.—An "unusually heavy" shock in Chili about $0^h\ 12^m$ A. M. Its duration was about thirty seconds and direction east to west. The Director of the National Observatory reports that it was followed by a rise of 2°·2 of the thermometer.

Nov. 11, 1874.—Trembling and subterranean rumbling at Guanajuato, Mexico. Many houses injured.

Nov. 12, 1874.—A slight shock about 10 P. M. at Virginia City, Nev.

Nov. 13, 1874.—Two shocks, each lasting ten seconds, were felt at Vera Cruz, and along the Mexican coast. Several houses were destroyed. Shocks continued to be felt at intervals for at least ten days.

Nov. 13, 1874.—A slight shock at Guayaquil, Ecuador.

Nov. 24, 1874.—A slight shock at Salem, Newburyport and throughout Essex County, Mass. Direction west to east.

Nov. 29, 1874.—Two heavy shocks at Oreana, Nev.

Dec. 10, 1874.—A somewhat severe shock was felt throughout Westchester and Rockland Counties, N. Y., and Bergen County, N. J. It extended as far as Peekskill on the north and Norwalk, Conn., on the east. The time is stated generally as half-past ten P. M. (one observer giving $10^h\ 23^m$). The duration is variously estimated from three seconds to one minute, the majority, however, putting it at five or six seconds. The direction of the vibration appears to have been from northwest to southeast, the reports being about equally divided between west, northwest and north, only one observer giving northeast. The shock was most severe in the neighborhood of Tarrytown and Nyack, but did no damage anywhere. It was at all places accompanied by subterranean noises, which are described as a long rumble ending with a violent explosion.

Jan. 7, 1875.—A shock at Valparaiso, Chili.

Jan. 18, 1875.—A shock in Ecuador.

Feb. 7, 1875.—Three shocks at San Francisco, the first about 2 A. M., the second at $10^h\ 45^m$ A. M., lasting two seconds, with the motion vertical and vibrations north and south; the third at $11^h\ 45^m$ A. M., not so heavy as the preceding.

Feb. 9, 1875.—Three slight shocks at Preston, Conn., not felt across the river in Norwich.

Feb. 11, 1875.—An earthquake at Guadalajara, Mexico, extending to San Cristabal and destroying houses in both places.

March 10, 1875.—A shock at 12 M. in the vicinity of Issequena, Goochland County, Va.

My thanks are due to John M. Batchelder, Esq., of Boston, for information received.

New Brunswick, N. J., March 20, 1875.

ART. XXXV.—*On Dr. Koch's Evidence with regard to the Cotemporaneity of Man and the Mastodon in Missouri;* by JAMES D. DANA.

THE evidence of the cotemporaneity of Man and various extinct Quaternary Mammals in Europe and Great Britain is complete: that is, it is beyond reasonable doubt or question; for (1) it has been gathered with great care by the best of geological observers; (2) it has been verified through the re-examinations of reported cases by other able geologists; and (3) it has been further verified by the special investigations of committees of scientific societies.

The North American facts thus far announced have not, unfortunately, the same broad basis for confidence.

Among the earlier of the reported discoveries are the two in Missouri, brought out by Dr. Koch. The account of them has often been cited by writers on the subject; and Mr. J. W. Foster, in his "Pre-Historic Races of the United States of America," prefixes to the citation the remark that Dr. Koch, at an interview with him, during the last year of his life, assured him, "in the most solemn and emphatic manner, that his statement was true." Mr. Foster also observes that "to deny the accuracy of his statement is to accuse him of having attempted to perpetrate a scientific fraud,"—a decision not sustained by the ordinary rules or treatment of evidence; for Science has constantly to guard itself against the assertions of men who are honest, but are not experienced in scientific investigation, and, in all such cases, rightly asks for corroborating testimony. Moreover, Dr. Koch's statement of his facts may be true, and still his conclusion as to their proving the cotemporaneity of Man and the Mastodon in North America be wrong.

The question which American Science should carefully consider — as carefully and guardedly as has been done for similar cases in Europe — is, whether Dr. Koch was a competent observer, and whether his observations are a sufficient basis for the conclusion that has been drawn from them.

I have before me four pamphlets by Dr. Koch, dated, severally, 1841, 1843, 1845 and 1853. They relate to his discoveries in this country — the first two of them to his *Missourium*, and the others to his *Hydrarchos*, or, as these publications call it,—his *Hydrargos*, or species of *Hydrachen*. The following are copies of their title pages, commencing with the earliest:

Description of the MISSOURIUM, or MISSOURI LEVIATHAN, together with its supposed habits; Indian traditions concerning the location from whence it was exhumed; also, comparisons of the Whale, Crocodile, and Missourium with the Leviathian, as described in the 41st Chapter of the Book of Job; by Albert Koch. 16 pp. 8vo. St. Louis, 1841. [1840 on the cover, indicating that the copy is from a second edition.]

Description of the MISSOURIUM THERISTOCAULODON (Koch), or MISSOURI LEVIATHAN (Leviathan Missouriensis), together with its supposed habits and Indian traditions; also, comparisons of the Whale, Crocodile and Missourium with the Leviathan, as described in the 41st Chapter of the Book of Job; by Albert Koch. Fifth edition enlarged. 28 pp. 8vo. Dublin, 1843. [A "third edition" of 24 pages appeared in London in 1841.]

HYDRARGOS, or GREAT SEA SERPENT OF ALABAMA, 114 feet in length, 7,500 lbs. weight, now exhibiting at the Apollo Saloon, 410, Broadway. Admittance 25 cents.—Description of the HYDRARGOS SILLIMANII (Koch). A gigantic fossil Reptile, or SEA SERPENT: lately discovered by the author in the State of Alabama, March, 1845. Together with some geological observations made on different formations of the rocks during a geological tour through the Eastern, Western and Southern parts of the United States, in the years 1844–1845; by Doctor Albert C. Koch, Corresponding Member of the Societies of Halle, and of Dresden, &c. 16 pp. 8vo. New York, 1845. [Following this, Dr. Koch published at Berlin, in 1845, a book of 99 pages, with 8 plates, entitled "Die Riesenthiere d. Urwelt," giving an account of his Mastodontoid discoveries in America.]

Description of the family of Animals now extinct, but known to the scientific world under the appellation of HYDRACHEN:* these animals, when living, were the most gigantic, powerful and horrible beasts of prey that ever ruled over and spread terror through the primitive Oceans; also an account of the discovery of the ZEUGLODON MACROSPONDYLUS of Müller, and of the remains of Hydrachen in general; by Dr. Albert Koch, Corresponding Member of various Scientific societies. 12 pp. 8vo. New Orleans, 1853.

The *first* of these pamphlets was printed when the MISSOURIUM was on exhibition at St. Louis in 1840, 1841; the *second*, when the skeleton was in Ireland, it having been taken to London in 1841; the *third*, when Dr. Koch's first collection of ZEUGLODON remains was arranged and on exhibition as the "Hydrargos" in New York; the *fourth*, after this first Zeuglodon collection had been carried (in 1845) to Europe, and purchased (in 1847) for the Royal Anatomical Museum at Berlin

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* In this change of name from *Hydarchos*, the *Water-Chief* (the suggestion, no doubt, of some friend, since he never wrote it right), to *Hydrachen*, a word that looks as if made up from the Greek word for *water* and the German for *dragon*, Dr. Koch evidently intended to adopt Müller's German term for the family, *Hydrarchen*.

(where it was studied by Müller); and after another "Hydrargos" had been obtained by Dr. Koch (in 1848), in the vicinity of "Washington Old Court House, Washington Co., Alabama," and had been transported (1) to Dresden (where, through "eight months' faithful labor," it was set up by the 6th of May, 1849), and also (2) to Breslau, (3) to Vienna (1850), and (4) to Prague, and at each place put on exhibition; but not to Munich, because "the only saloon disposable was too small for the exhibition;" and, finally, had come back to its native country, "after it had established its just fame in Europe" as one of the "Hydrachen," and been put on exhibition in New Orleans.*

Still other accounts of earlier date are at hand in this Journal, vols. xxxvi and xxxvii of 1839; the *first* (vol. xxxvi, p. 198) cited from a newspaper article of January, 1839, which was evidently written by Dr. Koch (then Mr., the title of Doctor appearing first in 1845); the *second* (vol. xxxvii, p. 191), signed "A. Koch, *Proprietor of the St. Louis Museum*," and credited to the "St. Louis Com. Bulletin of June 25," 1839.

Further, a note on the bones at St. Louis collected by Mr. Koch was presented to the American Philosophical Society, in October, 1840, by Dr. W. E. Horner, and an abstract from the Proceedings of that Society is cited in vol. xl, (1841) of this Journal.

It is evident from these documents that Dr. Koch was a man of enterprise, "an indefatigable collector." The credit is also due him of having performed a great service to science by his collections; for these included one of the best skeletons of the Mastodon that has been unearthed and two nearly complete skeletons of Zeuglodon, besides portions of other Mastodon and Zeuglodon individuals. Dr. Koch's "St. Louis Museum" contained, in 1840, according to Dr Horner, "200 or more teeth of the Mastodon and American Elephant, a dozen or more lower jaws of the Mastodon, with very numerous specimens of other parts of the head and skeleton generally, though no perfect head;" "the skeleton nearly complete of a Mastodon;" and, besides, "the head of an animal which Mr. Koch calls nondescript," which Dr. Horner thought to be that of a Mastodon, and another interesting Mastodon relic, "denominated by the proprietor (Dr. Koch) *Missourium Kochii*."

The two cases of the discovery of human remains along with those of the Mastodon, mentioned by Dr. Koch, are described in the pamphlets published in London and elsewhere abroad; in the Transactions of the St. Louis Academy, vol. i, p. 61, 1857;

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* The skeleton was on exhibition in St. Louis as early as 1855 or 1856, as stated in this Journal, II, xxi, 146, 1856; was there, as I learn from Dr. Lapham, sold to the Museum (Curiosity shop); and thence, later, taken to Wood's Museum in Chicago, where it ended its remarkable career in the great fire of 1871.

and the *first* of the cases at an earlier date in a newspaper article of January, 1839, cited in vol. xxxvi of this Journal (1839). This earliest account was written by Dr. Koch himself, the discoverer, for it is all in the *first* person; and, as it appeared within a few months of the discovery, it best deserves citation. It is therefore here republished, and after it, that of the *second* case, from the pamphlet of 1843.

I. "It is with the greatest pleasure the writer of this article can state, from personal knowledge, that one of the largest of these animals has actually been stoned and buried by Indians, as appears from implements found among the ashes, cinders, and half burned wood and bones of the animal. The circumstances are as follows:

"A farmer in Gasconade County, Missouri, lat. 38°20′ N., lon. 92° W., wished to improve his spring, and in doing so, discovered, about five feet beneath the surface, a part of the back and hip bone. Of this I was informed by Mr. Wash [Walsh, in pamphlet of 1843], and not doubting but the whole, or nearly the whole skeleton might be discovered, I went there and found as had been stated, also a knife made of stone. I immediately commenced opening a much larger space; the first layer of earth was a vegetable mould, then a blue clay, then sand and blue clay. I found a large quantity of pieces of rocks, weighing from two to twenty-five pounds each, evidently thrown there with the intention of hitting some object. It is necessary to remark, that not the least sign of rocks or gravel is to be found nearer than from four to five hundred yards; and that these pieces were broken from larger rocks, and consequently carried here for some express purpose. After passing through these rocks, I came to a layer of vegetable mould; on the surface of this was found the first blue bone, with this a spear and axe; the spear corresponds precisely with our common Indian spear, the axe is different from any one I have seen. Also on this earth was ashes, nearly from six inches to one foot in depth, intermixed with burned wood, and burned bones, broken spears, axes, knives, &c. The fire appeared to have been the largest on the head and neck of the animal, as the ashes and coals were much deeper here than in the rest of the body; the skull was quite perfect, but so much burned, that it crumbled to dust on the least touch; two feet from this, was found two teeth broken off from the jaw, but mashed entirely to pieces. By putting them together, they showed the animal to have been much larger than any heretofore discovered.

"It appeared by the situation of the skeleton, that the animal had been sunk with its hind feet in the mud and water, and unable to extricate itself, had fallen on its right side, and in that situation was found and killed as above described, consequently the hind and fore foot on the right side, was sunk deeper in the mud, and thereby saved from the effects of the fire; therefore I was able to preserve the whole of the hind foot to the very last joint, and the fore foot all but some few small bones, that were too much decayed to be worth saving. Also between the rocks that had sunk through the ashes, was found large pieces of skin, that appeared like fresh tanned sole leather, strongly impregnated with the ley from the ashes, and a great many of the sinews and arteries were plain to be seen on the earth and rocks, but in such a state as not to be moved, excepting in small pieces, the size of a hand, which are now preserved in spirits.

"Should any doubts arise in the mind of the reader, of the correctness of the above statement, he can be referred to more than twenty witnesses, who were present at the time of digging."—*This Journal*, 1839, xxxvi, 198.

The statements respecting this discovery in the pamphlet of 1843 agree in the main with the above. There is the additional information that the excavation took place in October, 1838; and that the locality was within 300 yards of the Burbois (rightly Bourbeuse) River; but nothing is said of the "large pieces of skin that appeared like fresh tanned sole-leather strongly impregnated with the ley from the ashes," or of "the sinews and arteries" that "were plainly to be seen on the earth and rocks," portions of "which are now preserved in spirits."

II. "The second trace of human existence with these animals, I found during the excavation of the Missourium. There was embedded, immediately under the femur or hind leg bone of this animal, an arrow-head of rose-coloured flint, resembling those used by the American Indians, but of a larger size. This was the only arrow-head immediately with the skeleton: but in the same strata at a distance of five or six feet, in a horizontal direction, four more arrow-heads were found; three of these were of the same formation as the preceding; the fourth was of very rude workmanship. One of the last mentioned three was of agate, the others of blue flint. These arrow-heads are indisputably the work of human hands. I examined the deposit in which they were embedded, and raised them out of their embedment with my own hands.

"The original stratum on which this river flowed at the time it was inhabited by the Missourium Theristocaulodon (and up to the time of its destruction), was of the upper green sand. On the surface of this stratum, and partly mingled with it, was the deposit of the before-described skeleton. The next stratum is from three to four feet in thickness, and consisted of a brown alluvium of the *Eocene* region, and was composed of vegetable matters of a tropical production—it contained all the remainder of the skeleton.

"Most of these vegetables were in a great state of preservation, and consisted of a large quantity of cypress burs, wood and bark, tropical cane, ferns, palmetto leaves, several stumps of trees, and even the greater part of a flower of the strelitzia class, which, when destroyed, was not full blown. There was no sign or indication of any very large trees, the cypresses that were discovered being the largest that were growing here at the time. These various matters had been torn up by their roots and twisted and split into a thousand pieces, apparently by lightning, combined with a tremendous tempest or tornado; and all were involved in one common ruin. Several veins of iron pyrites ran through this stratum.

"The next over this formation was a layer of plastic clay of the Eocene region, also with iron pyrites—it was three feet in thickness. Over this was a layer of conglomerate, from nine to eighteen inches in thickness; over this a layer of marl of the Pliocene region, from three to four feet in thickness; next a second conglomerate, from nine to eighteen inches in thickness; this was succeeded by a layer of yellow clay of the Pliocene; over this a third layer of conglomerate, from nine to eighteen inches in thickness; and at last the present surface, consisting of a delta, or alluvial deposit, formed by the river, consisting of brownish clay, mingled with a few pebbles, and covered with large oak, maple and elm trees, which were, as near as I could ascertain, from 80 to 100 years old. In the center of the above-mentioned deposit was a large spring which appeared to rise from the very bowels of the earth, as it was never affected by the severest rain, nor did it become lower by the longest drought."—*Dr. Koch's Pamphlet of* 1843, pages 27, 13 and 14.

The first question before us is: Whether the observations and conclusions in the above statements may be accepted with confidence because made by a geologist, or a man of scientific training?

In the account of the second case above cited, Dr. Koch says that the Missourium was embedded in "a brown alluvium of the *Eocene* region" resting on the "upper green sand;" that next over it there was plastic clay of "the Eocene region" and beds of "the Pliocene region." He thus makes his Missourium to have come from the Lower Tertiary, and from a bed just above the upper green sand (Cretaceous), when actually from Quaternary beds; and he uses the terms *Eocene* and *Pliocene* as if he had no familiarity with geological facts or language. The earlier pamphlet of 1840 avoids this bad geology, the "upper green sand" in that being called simply quicksand, and the other beds merely beds of clay and conglomerate. All the pamphlets sustain the conclusion that Dr. Koch knew almost nothing of geology, and that what he gradually picked up from intercourse with geologists he generally made much of, but seldom was able to use rightly.

In zoölogical knowledge he was equally deficient. The account of the Missourium, in the pamphlet of 1841, recognizes a resemblance to the Mastodon and Elephant; but, notwithstanding this, it says that "his feet were webbed;" that he had "been without doubt an inhabitant of water courses such as large rivers and lakes," as his webbed feet, solid bones without marrow, short and thick legs, flat and broad tail, etc., proved; that his curving tusks, 10 feet in length, "were carried by him almost horizontally [as represented on the cover of the pamphlet of 1843], so that it would be impossible for him to exist in a timbered country;" that his food (the teeth having before been described rightly as eight in number, "four upper and four lower") "consisted as much of vegetables as flesh, although he undoubtedly consumed a great abundance of the latter;" that he "*was capable of feeding himself with his forefoot, after the manner of the beaver or otter*;" and that he "possessed, also, like the hippopotamus, the faculty of walking on the bottom of waters and rose occasionally to take air;" that "the singular position of the tusks* has been wisely adapted by the Creator for the protection of the body from the many injuries to which it would be exposed while swimming or walking under water; that it appears that the animal was covered with the same armor as the alligator, or perhaps the megatherium."

Later in the pamphlet, he goes on with his conclusions and says: "After having examined this subject in all its bearings, I have come to the conclusion that the *leviathan*—described in the 41st chapter of the book of Job—is none other than the Missourium here described, and from this time I shall call it the *Missouri Leviathan* (*Leviathan Missourii*)."

Next follows a comparison with the account of Job, taking up the several verses in order. On the first, "Canst thou draw out Leviathan with a hook?" he says: "The Missourium, as I have described, was a creature of enormous magnitude, ferocity, and strength, as well as fleetness in swimming; and by reason of his great weight and strength could attack the largest animals with impunity and overcome them with ease; nor is it probable that any combination of human force was able to draw him out of his native element." The sentence "Who can come to him with his double bridle?" has the following exposition: "The tusks coming out of the head until they arrive at a parallel with the nose, then turning suddenly back and forming a semicircle around the head (like a shield to prevent anything from approaching it), and measuring from point to point in a straight line over the head 15 feet; it can be seen at once how utterly futile would be any attempt to cast a bridle over him."

Dr. Koch closes this exposition and his pamphlet with a paragraph explaining how the "Leviathan, which is described as being an inhabitant of Asia, came to be found in the extreme west of the globe."

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* The position which one chanced to be in when the Missourium was exhumed, In the newspaper article by Mr. Koch, cited in this Journal at p. 191 of vol. xxxvii, a *Mastodon* is reported as having been found with the tusk in this position, and "Koch's Missourium" is mentioned as a nondescript animal, the head of which he found near the same place.

The Dublin pamphlet of 1843 shows some gain in knowledge; but the author still holds that the Missourium was not solely herbivorous; that its tusks curved outward horizontally; that it "waded frequently at the bottoms of the former gigantic rivers and lakes of the west;" that "the ribs resembled more those of the Reptilia than those of the quadrupeds, being situated half reversed in the body [Dr. Koch's misplacement of them]; that is, the lower edge of the rib bends in toward the intestines, and the upper edge out toward the skin" (as stated *also* in the pamphlet of 1840); and he ends off with the same detailed "comparison of the Leviathan with the Missourium," and the same explanation of how such an Asiatic inhabitant came to be found "in the extreme west of the globe."

Now this web-footed aquatic animal, capable of feeding himself with his forefoot, was no other than the *American Mastodon*, whose forefeet were as good for putting food into its mouth, or picking its teeth, as any Elephant's. The specimen taken to England, in 1841, as the Missourium, and which Professor Owen says was "well-known to the public as the *Missouri Leviathan*, when exhibited with a most grotesquely distorted and exaggerated collection of the bones in 1842 and 1843 in the Egyptian Hall, Piccadilly," is now (1846), he adds,* an almost complete skeleton of "the *Mastodon giganteus*, mounted in strict accordance with its natural proportions in the British Museum;" and a representation of it, copied from Owen, is the figure of the Mastodon on page 566 of the writer's Manual of Geology.

It is pretty plain that Dr. Koch had not been trained to scientific investigation. This is equally obvious from his two pamphlets on the "Hydrargos."

(1) The skeleton exhibited in New York in 1845, and described in the pamphlet of that date, was 114 feet long, and this was at least 35 feet longer than nature—the vertebræ gathered by him in Alabama having been all strung together into one long "Hydrargos," though belonging really, as Dr. Jeffries Wyman announced, to individuals of different ages, and according to Müller, to at least two species (*Zeuglodon macrospondylus* and *Z. brachyspondylus* of Müller).

(2) The head was in part a piece of bad patchwork; Dr. Wyman stating in his notes, made after a careful examination,† that the cranium proper (the part over the brain) was made out of a single piece of bone without sutures, leading to the supposition that it was not the true cranium, an inference sustained, he says, by there being no foramen for the passage of the spinal

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* History of British Fossil Mammals and Birds, by Richard Owen, F.R.S., etc., London, 1846, page 298.

† Proceedings Boston Nat. Hist. Soc., 1845, p. 65. In this Journal, II, ii, 129 (1846), where Dr. Wyman's article is noticed by B. Silliman, Jr., there is a figure of the remarkable head.

marrow, and no larger space for the brain than that for the spinal cord on the upper side of some of the vertebræ; and by the amount of cement, which left little or nothing of the under surface in sight.

(3) The extremities of the so-called paddles were formed, says Dr. Wyman, of *casts of* "*a species of Nautilus.*"

The Hydrargos or Hydrachen pamphlets hence do not require any modification of the opinion that Dr. Koch had not been trained to scientific investigation.

But on this point we have the opposing assertion of Dr. Koch. Being in a foreign country, where he had to make himself known, he opens his pamphlet of 1843 with the following introduction of himself to the public:

> "Previous to my commencing this Treatise, I wish particularly to mention that I have not only devoted the greater part of my life to the theoretical study of Natural History, but have also made myself intimately acquainted with the practical part of it."

Still, we are unable to set aside the facts presented in the preceding pages and the opinion to which they have led, and therefore feel forced to take this introductory paragraph differently from what the author intended, and gather from it that Dr. Koch was a man of large pretensions. This same impression is conveyed by the account of his scientific travels in North America, which occupies the following six pages. I give an example. On the third of these pages he describes the era of the "mighty Missourium" and "ponderous Mastodon" as a time when, according to "every geological research," the earth's surface was "uninterrupted by any of the rough, broken, rugged deformities which now present themselves on every side," when "the climate was free from noxious vapors," and all was "delightful," etc.; and then dwells on the sudden dreadful change when "the ground was cursed for man's sake" and "all those gigantic creatures perished," and "the garden of delicious fruit trees and blooming flowers was converted into a gloomy forest of thorns and thistles." He also gives us his idea as to the nature of the great catastrophe, as follows: "The principal instrument used to cause this change, according to my views, was a certain Comet that came in contact with our globe, which caused not only a different position of our earth, but also the interior fire and water to come into an immediate violent collision, which created a revolution, that naturally sought for vent, and therefore burst through the crust of the quivering earth, tore up countries and sank them in the sea."

This is enough to prove that these pages do not sustain his large claim.

Holding, then, to the conclusion that Dr. Koch was quite ignorant of geology and without scientific training, we are forced to doubt, to doubt strongly, his direct and definite statement that he had devoted the greater part of his life to "the theoretical study of Natural History" and had made himself "intimately acquainted with the practical part of it." It is true that he knew about the earlier part of his own life better than any other person then living. Any way, he certainly overrated almost infinitely the results on himself of so prolonged study. This much we are disposed to allow in favor of his sagacity: that Dr. Koch appreciated the absurdity of the Leviathan story, and introduced it, after some thought about the people he was among, merely to get a full house for his Missourium; and that his attempted show of scientific knowledge had the same end in view. If this supposition is unjust to him, the other alternative explanation has to stand. In his New Orleans "Hydrachen" pamphlet (1853), the inside pages of the cover contain a long cited article* which makes the Zeuglodon the Leviathan of Job—thus showing apparently that his previous convictions were not too strong for a change of opinion, especially after the Missourium had turned into a Mastodon.

The special statements respecting the mode of occurrence of the human relics cited on pages 338 to 340 remain for consideration.

In the account of the deposits which afforded the Missourium, Dr. Koch speaks of "the present surface as consisting of a delta or alluvial deposit," which suggests a doubt as to whether the overlying beds of sand and pebbles may not have been of very recent formation through river action. It is not made certain that they were true Champlain deposits. He says that one arrow-head lay "immediately under the femur or thigh-bone," and he further states, in his later article of 1857, that he "carefully thought to investigate" the point as to its having "been brought thither after the deposit of the bone," and decided against it. The observation and conclusion would have been more satisfactory had the author of them been a better observer.

The description of the deposits in Gasconade County, containing the remains of an animal "the principal part of which had been consumed by fire," is a still more unsatisfactory basis for a safe conclusion as to age. But in the article of 1857 he says that "the layer of ashes, etc., was covered by strata of alluvial deposits consisting of clay, sand and soil from eight to nine feet thick *forming the bottom of the Bourbeuse* [*River*] *in*

*** It is headed "From the New York Evangelist," and must have been written in 1845, when the skeleton was on exhibition in New York City. The author's name is not given.**

general," which seems to make it almost certain that the beds were of quite recent origin. Neither in the account of 1839, nor of 1843, is the kind of animal mentioned, that of 1843 saying that "they were the remains of an animal which had clawed feet and was of the size of an elephant," and "that the construction of the foot [fore foot] shows that it possessed much power in grasping and holding objects;" but in that of 1857 he says "the bones were sufficiently well preserved to enable one to decide positively that they belong to the *Mastodon giganteus.*"

The tragic part of the story—about the elephantine beast having been burnt alive by the Indians after they had used their bows, and also thrown more than 150 great pieces of rocks "two to twenty-five pounds in weight" at him in vain,—is an hypothesis in keeping with the rest of his documents. A fire kindled about the shoulders of a mired Mastodon would have taken long to get through the hide and muscle so as to char the bones; and an Indian's appetite would have been pretty sure to have stopped the cooking short of this charring. The charring might have been done very long after the miring and death of the animal, and the facts be all as they are reported. The remark that "the greater portion of the bones had been more or less burned by fire" favors the idea that the fire was made about the bones at some time between the era of the Mastodon and the present time, and not about the living body.

The failure to repeat, in either of the later accounts, the early statements respecting the large pieces of skin that appeared like fresh tanned sole-leather," and the "sinews and arteries plain to be seen on the earth and rocks," shows that he afterward doubted and rejected this part of his observations; and this unavoidably suggests some doubt as to the other points; even to questioning whether the charring was not in fact only a blackening in color due to burial in the marsh—a very common effect from such a cause; whether the crumbling was not a result of that natural decay which so generally befalls old bones; and whether the stone implements found were not small oblong stones of nature's chipping or polishing.

Thus stands the evidence. If the statements respecting the deposits had been published by a good geologist with no more of detail, and without any special effort afterward to make all things positive, there would be some room for doubt, considering the many chances of error that exist. But in the present case they were not made by a good geologist; they were not made by one trained to investigation, or to habits of precise statement; nor by one who had a knowledge of any department of science; nor by one whose sound common sense was so obvious a characteristic as to demand consideration for his opinions and statements; nor by one wholly free from pretense and sham.

Taking all things that have been reviewed into consideration, I think there is sufficient reason for regarding Dr. Koch's evidence of the cotemporaneity of Man and the Mastodon *very doubtful.* It is to be hoped that the geologists of the Missouri Geological Survey now in progress will succeed in settling the question positively.

The cotemporaneity claimed will probably be shown to be true for North America by future discoveries if not already so established; for Man existed in Europe long before the extinction of the American Mastodon.

Art. XXXVI.—*Bentham, On the recent Progress and present State of Systematic Botany.*

[Continued from page 294.]

As to the necessity or advantage of artificial keys to species in systematic works, when the species of a genus are brought into an order which as much as possible exhibits their natural relationships, it is seldom difficult to divide and subdivide the groups by important characters, if pains enough be taken, so that the series of subordinated divisions shall completely and practically do the work of an artificial key. The advantages are that the student's attention is fixed from the first upon the more natural characters and their combinations; also, the more thorough the subdivision, the briefer and more diagnostic may be the description of the species, since the points mentioned in the sections need not be repeated.

In this connexion we can best bring in the few remarks we have to offer upon the present tendency, which Mr. Bentham favors, to merge specific character (or full diagnosis) and detailed description into one, making of that one the latter rather than the former, and drawing it up, when Latin is used, in the nominative rather than in the ablative case. Granted that "we cannot now restrict them to the twelve-word law of Linnæus," and that "a twelve-line ablative diagnosis is an absolute nuisance," our choice is not bound to either of these extremes, and the happy medium may generally be hit. The diagnostic form is a constant enjoinder of brevity and of utmost perspicuity. The protracted specific characters which Mr. Bentham rightly deprecates generally mean slovenly work. To keep them within bounds we have only to insist upon critical grouping, and subordinated sections and subsections, such as we have just recommended. In this way we adapt the Linnæan diagnosis to our present needs, retaining all its advantages. The other course we fear will tend to prolixity and looseness, however well it may work in such judicious and practised hands as those of Mr. Bentham. Finally, if we have no diagnosis, but only a description, then the nominative case is a matter of course. If we retain the Latin diagnosis, we may well retain the ablative case, as having some advantage in succinctness, and the *prestige* of long use. A reasonable punctuation ought, however, to be settled on,—the two prevalent extremes being about equally awkward and inconvenient. A *juste milieu* is not far to seek. Even in English there is still advantage in the diagnostic form, relegating details and subsidiary or explanatory matter to a separate sentence or paragraph.

Of Linnæus's two great gifts, the binomial nomenclature and the diagnosis, we are almost as reluctant to give up the one as the other.

Upon the languages actually in use in systematic botany, Mr. Bentham has some interesting remarks, which fill three or four pages. In the preparation of this and his previous somewhat similar addresses, he had to consult botanical publications in no less than fifteen languages. Surely no other living botanist is able to do so, and to read a majority of them with facility. It is conceded that, while works intended for the beginner or amateur, or for teaching the well-known botany of a particular country, should be in the familiar language of the country, yet "purely scientific treatises, and technically descriptive works, which all botanists may have to take cognizance of, and for which the commercial demand may be too limited to ensure their translation into various languages," should be written in the one most likely to be understood by the greater number of students of all countries,—that is, in Latin; which, moreover, is "best suited for technical diagnoses and descriptions, from its concise character and from its susceptibility of being subjected to technical forms, without jarring upon the conventionalities of living languages in familiar use." To which we may add, that a sufficient, long-tried, and well-settled vocabulary has been formed from it, which cannot be readily rendered into other tongues without danger of ambiguity, except by transferring the terms themselves, as we have done, into English, the genius of our language lending itself readily to the transference. Happily, botanical English and botanical Latin are very much alike.

Modern languages are, and must needs be, more and more used for general descriptions and scientific investigations and discussions; "and of these are three which at the present day every botanist ought to understand, and in one of which [besides his vernacular] he ought to be able to write,—all three having a rich literature in every branch to repay the labor of learning them, independently of science; these are French, English, and German."

"French has long been considered the one among modern languages forming the nearest approach to a common one; it is easy, comparatively simple in construction, not overburdened with redundant words, and, above all, is readily broken up into short phrases, an invaluable qualification for clearness of methodical exposition. It has long been the recognized diplomatic language, and the first foreign one taught in most European schools; and although within my own recollection national animosities may have from time to time thrown it into disfavour in Germany and Eastern Europe, yet it always appears to recover its *prestige* there in general society. At the meetings of the botanists of various nations congregated at Florence last May, it was the general medium of intercourse, although the Frenchmen present were in a very small minority. And in every branch of science or literature to which I have paid more or less attention, it possesses more instructive elementary works, more readily intelligible treatises and clearer expositions of abstruse subjects, than any other language I am acquainted with. For the botanist, therefore, as well as for all naturalists, its study is still, and I believe will long remain, of first-rate importance.

"The English language has of late years been recommended by more than one continental naturalist for general adoption as a vehicle for international scientific intercourse. It partakes of some of the advantages of both the French and the German. Though less brilliant, it offers more variety than the former, it is less involved than the latter, and it appears to be capable of giving more precision and force to argument than either. It is now the national language of the largest proportion of the civilized population of the globe, and its use continues steadily to spread out of Europe generally, and to a certain extent among European naturalists and other educated classes, especially in eastern and northern Europe. They begin to admit the necessity of consulting our untranslated treatises and memoirs, and our German and east European botanical correspondents, at least, accept English letters as readily as French. In southern Europe French is still much more generally understood; but even there the objections to the extended use of our language for botanical works have now, I believe, lost much of their force.

"The German is a more difficult language, much more difficult, indeed, for the Latin nations of southern and western Europe than for ourselves. Its construction is involved, its extraordinary copiousness occasions a strain upon the memory; but it affords great facilities for giving expression to minutely distinguished details, whether of fact or of thought. It may thus frequently give greater solidity to their theoretical expositions than the French, but is infinitely more difficult to translate; and to those who are not thoroughly used to its intricacies it seems to foster, if not to create, confusion of ideas. Germany has now, however, so long included so many publishing centres of scientific importance, and its language has been so generally used by Scandinavian and Sclavonian, as well as by their own naturalists, that a sufficient acquaintance with it, to study the very numerous works it produces, can no longer be dispensed with by the general botanist."

Mr. Bentham goes on to consider the principal classes of works in systematic botany, recently published or in progress, under the heads: 1. of *Ordines Plantarum*, or the like,—of which Lindley's Vegetable Kingdom (very great in its merits and its faults) was the most complete and elaborate, and Le Maout and Decaisne's *Traité de Botanique* (and especially Dr. Hooker's English edition) the only modern substitute, and indeed far better in the way of structural exposition. But it covers less ground. Mr. Bentham rightly enough deprecates the proposed reproduction of Lindley's great work, as "scarcely fair to the memory of the talented author." To the bizarre classification and many strange and fanciful views of affinity not even Lindley's talents and ardor could give vitality and currency during his life-time. But there is a part of this thesaurus which might be reproduced, with needful additions, to great advantage, namely that which relates to the properties and uses of plants. This, well edited, would form a good-sized and surely a most useful volume,—a companion and supplement to the French work just mentioned, which is weak in this department.

Under the second head, of *Genera Plantarum*, a good account is given of the scope and plan of the work upon which Mr. Bentham is now engaged, in conjunction with Dr. Hooker. This publication began in 1862, and is now, it may be said, half done. We may count upon having the second volume completed before the present year is out.

Under the third head, of *Species Plantarum*, after more remarks upon De Candolle's Prodomus, and its completion, or rather its close at the end of the Dicotyledons, Mr. Bentham raises the question whether it may not still be practicable to have a condensed Species or Synopsis Plantarum for all Phænogamous plants, within a single generation. Alphonse De Candolle's recent estimate and hopeless conclusion are familiar to us. Mr. Bentham's less discouraging view, the grounds of which are disclosed in the following extract, is largely based upon his own experience.

"Alphonse De Candolle, in the above-quoted 'Reflexions,' has shown how little chance there is of a uniform 'Species Plantarum' being again undertaken with any prospect of its being brought to a successful conclusion. He calculates that it would require fifteen or sixteen years' labour of some five-and-twenty botanists, working under the direction of about eight to ten editors,—a combination which it is highly improbable will ever be practically brought to bear. His calculations may, however, be a little overcharged. He supposes that each botanist would not work up more than 300 species in a year: that may be the case in a monograph, when every detail is to be gone through from personal observation; but this would not now be necessary in a general 'Species Plantarum,' which would be most useful as a concise methodical compilation. Much of the labour expended on the 'Prodromus' and on detached monographs and floras need not be repeated. As pre-Linnæan synonyms, upon which so much time was formerly expended, have now been generally given up, so, for post-Linnæan synonyms, there would now be no use in repeating those given in the 'Prodromus' and other works compiled from, unless where errors have been detected; and this alone would save a great deal of time, labour and expense. And with regard to the greater number of the orders or genera contained in the recent volumes of the 'Prodromus' and the best modern monographs and floras, a careful and intelligent abridgment of the specific characters without reëxamination is all that would be necessary.

"It might be useful to consider what would be the requisites of any such abridged 'Species Plantarum' or 'Synopsis,' restricted within limits which should render it possible, at least as to phænogamous plants. We might expect it to follow the sequence of orders the most generally adopted, that of the 'Prodromus' and of our 'Genera Plantarum,' with such slight modifications only as the progress of science has rendered necessary, without attempting hypothetical improvements. To each order and to each genus should be given short diagnostic characters, abridged from the last 'Genera Plantarum' or other best sources, selecting chiefly those which are most essential and contrasted, but including also the most striking or the most general amongst the adaptive ones, and a general indication of geographical range, with careful reference to the works where more details are to be found. Where the orders or genera are large, a synopsis or conspectus of the principal divisions and subdivisions would be useful.

"To each species should be given:—

"(1) The name.

"(2) The diagnosis, specific character, or abridged description, which are but different names for the same thing, and which it appears to me would be always more satisfactory in the nominative than in the ablative case. After the example of Linnæus, and based upon the doctrine of the fixity of species, it has been almost universally the custom to distinguish the specific diagnosis and description, the former to contain the absolutely distinctive characters (any deviation from which would exclude a plant from the species), the latter to aid the student in identifying a plant by the enumeration of characters which, though general, might vary in the same species, or which it may possess in common with other species. In order to mark the more strongly this difference, the diagnosis, when in Latin, has been given in the form of the ablative absolute, the description in the ordinary nominative form. There is, however, nothing really absolute in nature. There is no class of characters which may not occasionally admit of exceptions: and although care should be taken to select the most important and constant ones, yet in some instances, those which are generally discarded as too variable for a diagnosis, such as dimensions, colour, &c., may yet be most useful, or even essential for the distinction of species, or even of genera. These diagnoses, moreover, to be useful should be short. We cannot now restrict them to the twelve-word law of Linnæus, but a twelve-line ablative diagnosis is an absolute nuisance.

"(3) Reference to the source whence the diagnosis is taken, to the work where a further description, the synonymy, and history of the species are to be found, and to any plates where it may be satisfactorily represented; and all further synonymy should be avoided, except where it may be necessary to refer to descriptions, names, or modifications published since the one specially abstracted from.

"(4) The habitat of the species.

"(5) Occasional notes on affinities or other points in the history of the species should be very sparingly indulged in, and only when they may assist essentially in the provisional determination and elucidation of a plant. All discussions on doubtful points and all details should be reserved for monographs or separate papers, where alone they can really tend to the advancement of the science.

"Each volume of the 'Synopsis' would of course be accompanied by a full index of genera, species, and such synonyms as it may have been found necessary to give.

"The whole work would be so indispensable to botanists of all nations, that, like the 'Genera Plantarum,' it should be entirely in botanical Latin, which, moreover, from the number of conventional expressions to which a technical meaning has been assigned, is specially suited for short diagnoses.

"No new species should be first published in this 'Synopsis.' Nothing has tended more to produce confusion in systematic botany than the publication of real or supposed new species, with short diagnoses, unattended by any full description or detailed indications of its affinities, &c. However carefully the diagnosis may be worded so as to distinguish the species from those previously published, it would be insufficient for its identification, and full descriptions would be inadmissible from the plan of the work. At the same time, it is to be expected that the author, in preparing the 'Synopsis,' should meet with new forms, which he may be desirous to make known, in order to render his work as complete as possible. But his course should be to give their full history in a separate monograph, to which, *when published*, he could refer in the 'Synopsis.' He should here not only thus avoid all addition to the numerous puzzles with which the science is overloaded from insufficient description, but strictly abstain from all mention of manuscript and other names which, according to the recognized rules of nomenclature, are not admitted as sufficiently published.

"The grade of plant-race to which the specific name and diagnosis should be attached would be the species, in the Linnæan sense, which, though not susceptible of a strict definition, is pretty generally understood amongst botanists, whether they may designate it as a true species, a Linnæan, or a compound species The 'Synopsis' might also distinguish marked varieties whose admission or rejection as species might be doubtful; but the innumerable forms variously termed varieties, subspecies, or critical species, should be passed over in silence, as their admission would simply render a general work impossible, and a more partial one comparatively useless. The enumeration and distinction of the various forms of *Brassica campestris* and *oleracea*, of *Pisum sativum*, *Viola tricolor*, &c., may be serviceable to the agriculturist or gardener, that of the forms of *Rubus fruticosus* may be interesting to the investigator of the flora of a limited district, but they are only useless encumbrances to the general systematist as well as to the naturalist in other branches who would have to make use of the 'Synopsis;' and the names and diagnoses of two hundred forms of *Draba verna* would be a simple nuisance, of no use whatever to any one.*

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* "The mode of dealing with species which, in the present state of vegetation, pass into each other through a series of intermediate forms which cannot fairly be supposed to be hybrids, is well discussed by Nägeli in a series of papers in the "Sitzungsberichte" of the Munich Academy for 1866, the result of careful observation chiefly of the genus *Hieracium*. After admitting himself to have been originally a firm believer in the fixity of species and a strong advocate of the hybrid parentage of the large number of intermediate forms observed, he acknowledges his conversion to the doctrine of evolution. "In the present state of the science" he sees "no other possibility than the assumption that the species of *Hieracium* have

"Taking the species, therefore, in the Linnæan sense, we would, with Alph. de Candolle, estimate the number of Phænogams now published, or in the course of publication, from materials already in our herbaria, at between 110,000 and 120,000. A competent botanist would readily get through three or four thousand in a year. In the 'Flora Australiensis' I had no difficulty in preparing a thousand to twelve hundred in the year, and that was all original work, entailing the personal examination of every species often in numerous specimens, and a long and tedious investigation of synonyms. Such a compilation as I have above characterized would require, it is true, a competent knowledge of plants and occasional verifications; but still the labour would be reduced by at least two-thirds; and 300 species a month, with a month or six weeks' vacation, would be no great strain upon the mind. Thus three or four botanists might complete a synopsis of ten thousand species in the year; and the general synoptical enumeration of all known Phænogams would not be beyond the range of possibility, however little chance there may be of my living to see it commenced."

The doubt which rises is, whether several or indeed any botanists can be found in our day capable of turning off good work at anything like Mr. Bentham's rate, and so exceptionably well situated in respect to herbaria and libraries, and in the command of their time. Easy as the work may seem, the number of botanists who are able to elaborate a genus and draw up fairly good botanical descriptions is wonderfully small.

arisen by transmutation either from extinct or from still surviving forms, and that there are still persistent a great number of the intermediate stages (races) formed either by the original differentiation of the extinct species, or in the course of the transformation of one yet living species into the diverging forms."—*Sitzungsber.*, 1866, i, 330.

In a subsequent paper he shows that the genus *Hieracium* affords instances of great diversity in the degree to which differentiation has attained and in the definiteness of the species established by the extinction of intermediates. He instances, amongst those to which he would in their present state assign the rank of species:—

1. Aggregate forms, such as *H. Pilosella*, which cannot as yet be separated into distinct groups. *H. Hoppeanum*, Schult., *H. Pelleterianum*, Mérat, *H. Pseudopilosella*, Jen., are not yet sufficiently isolated by the disappearance of intermediate forms to be ranked as species.

2. Forms which, by the disappearance of closely allied ones, have attained sharper and more fixed limits, and yet between which isolated intermediates may still be found, are exemplified by *H. Auricula*, *H. aurantiacum* and *H. Pilosella*, or by *H. murorum*, *H. villosum* and *H. glaucum*. On the other hand, it is uncertain whether the relations of *H. Auricula* and *H. glaciale*, or of *H. murorum* and *H. vulgatum*, should be included in this stage, or are still in the first-mentioned category.

3. Species between which no constant intermediates survive, but which still are capable of producing intermediate hybrids, are represented by *H. alpinum* and *H. villosum*, by *H. alpinum* and *H. glaucum*, by *H. murorum* and *H. umbellatum*, &c.

4. Lastly, the three sections *Pilosella*, *Archieracium* and *Stenotheca* are races which become so far-distanced from each other that hybrid fertilization no longer takes place between them.—*Sitzungsb.*, 1866, i. 4?2.

The thing is quite possible if mere literary compilation is intended; but something more than this is needed.

While it would be best, and in case of a mere compilation even necessary, that new species should be first published elsewhere than in this desired "Synopsis," yet we cannot agree that, in any proper elaboration or digest of the species of a genus, "the diagnosis would be insufficient for its identification." It is just here, where the new species is brought into line with its relatives, that the diagnosis should suffice; and if a diagnosis does not serve for the new species, it may do no better for the old ones. Upon the form in which specific characters may best be cast, in a synoptical work—whether that of diagnoses, under sections and subdivisions, or of open descriptions, led to by an artificial key—we have already offered some remarks.

Mr. Bentham proceeds to discourse of monographs of orders and genera, of floras or histories of the plants of particular countries or districts (this division ending with a loud call for the North American Flora), and, finally, of specific descriptions, detached or miscellaneous. His remarks upon the latter are important and timely.

"Had I to report only on the progress, and not on the present state also, of systematic botany, I should here stop; for the great majority of recent detached and miscellaneous descriptions are almost as much impediments as aids to the progress of the science. I have too often in my Linnean Addresses, especially in those of 1862 and 1871, animadverted on the mischief they produce, to enter now into any details; I can only lament that the practice continues, and is even rendered necessary by considerations not wholly scientific. Horticulturists must have names for their new importations. It is due to travellers who, under great perils and fatigues, have contributed largely to supplying us with specimens of the vegetation of distant regions, that the results of their labour should be speedily made known; it is even important to science that any new form influencing materially methodical arrangements should be published as soon as ascertained. But all this is very different from the barren diagnoses of garden-catalogues, and the long, uncontrasted descriptions hastily got up for the futile purpose of securing priority of name. I own that I have myself erred in the want of sufficient consideration in the publication of some of the species of Plantæ Hartwegianæ; and some descriptive miscellanea, even by men who stand very high in the science (such as Miquel's 'Prolusiones,' above referred to, and Baron von Mueller's 'Fragmenta'), are rendered comparatively useless from their utter want of method. Whilst, therefore, discouraging as much as possible all such detached publications of new species, I would admit their occasional necessity, but suggest the following rules as the result of a long practical experience:—

"No detached description of a new species should be ventured upon unless the author has ample means of reviewing the group it belongs to; and if any doubts remain of its substantive validity, he should refrain from giving it a name till those doubts are cleared up.

"The description, when given, should be full, but contrasted and accompanied by a discussion of affinities with previously known species, and an indication of the place the new one should occupy in the several monographs and floras in which it would be included.

"An illustration of the new plant, with analytical details, should never be neglected where circumstances admit of it."

We would heartily second these recommendations. They are especially applicable in this country. Time is short and the work before us is heavy. One grievous impediment to its progress and trial to patience comes from the loose and rambling descriptions of species, or even genera, hastily put forth by those who have not the means of making the proper investigations and comparisons, and in some cases not the knowledge which enables them to hit the genus or even the family correctly. The best of us commit mistakes enough, but, it is to be hoped, clear up many more than we make. The road we have to traverse, and to make smooth for those who come after us, is difficult and rugged enough as it is. Shooting rubbish into it is a serious hindrance. Real aid, however, is joyfully welcomed, and no one in our day who is able, or desirous even to attempt to do systematic botanical work, has ever received aught but help and encouragement from those who have to bear the burden and heat of the day. A. GRAY.

ART. XXXVII.—*On the Influence of Color upon Reduction of Light*; by M. CAREY LEA.

IN this Journal for March of last year, I published a train of investigations which had for their object the study of the changes produced in the sensitiveness of certain substances to particular rays of light, as caused by the presence of various colored and uncolored bodies. These results were incompatible with a law shortly before announced by Dr. Hermann Vogel, as governing the action of colored bodies on the sensitiveness of silver bromide.

Dr. Vogel's law was, that to render AgBr sensitive to any ray, or to increase its sensitiveness, it is only necessary to place in contact with it a substance capable of promoting the decomposition of AgBr, and which substance must absorb the rays in question, and not other rays.*

My own conclusion was that the power of heightening sensitiveness to particular rays was one in no way connected with the color of the sensitizing body; indeed, that perfectly colorless bodies, or bodies having very pale neutral colors might also exercise the functions of heightening sensitiveness to particular colored rays.

Since the publication of my former paper, I have made further investigations, and Dr. Vogel has published a second paper. In this second paper Dr. Vogel, from whom I have regretted to differ, has to a considerable extent adopted my own views. He reaches the same conclusion, as to the power of substances destitute of color to exalt the sensitiveness of AgBr to particular rays. As this was the main result of a long investigation, and the demonstration of it the principal object of my paper (from which paper and its conclusions Dr. Vogel strongly dissented), it would have been more agreeable to me if Dr. Vogel had acknowledged it as originating with me. This, however, is a matter of small importance. Dr. Vogel does not seem to have adverted to the fact that this conclusion is scarcely compatible with his theory, formulated in the foot-note; with the alleged function of *color* as modifying sensitiveness to particular rays.

If this theory were true, it must be easy to find very numerous instances capable of supporting it, as the number of organic coloring matters capable of taking up bromine is very large. Nevertheless, three only have been brought forward by Dr. Vogel in the two papers referred to, out of which three, two at least certainly do not seem to afford the desired proof.

Coralline and naphthaline red are substances which transmit strong red light, and consequently the sum of their absorption spectra must in each case be green: to confirm Dr. Vogel's theory, they should enhance the sensitiveness of AgBr to green light. But Dr. Vogel finds an increased sensibility to *yellow* rays only. Obviously yellow cannot represent the absorption by a red color. If Dr. Vogel's law be true, a red substance *may* heighten sensitiveness to yellow, but it *must* heighten it to green.

Now with the substance principally experimented on by Dr. Vogel, coralline, I have lately made a number of trials, and have succeeded in definitely fixing its action with respect to AgBr.

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* Wir im Stande sind Bromsilber für jede beliebige Farbe lichtempfindlich zu machen, respectiv die bereits vorhandene Empfindlichkeit für gewisse Farbe zu steigern; es ist nur nöthig einen die chemische Zersetzbarkeit des Bromsilbers befördernde Stoffe zuzusetzen, welcher die betreffenden Farbe absorbirt, die andern nicht.—*Ber. Deutsch. Chem. Ges.*, 1873, p. 1305.

To the red rays, including those whose wave-lengths are from λ605 to the end of the visible spectrum, coralline very materially increases the sensitiveness of AgBr.

To the yellow rays, taking as representatives those whose wave-lengths vary only moderately from λ570, coralline moderately increases the sensitiveness of AgBr.

To the green rays, taking principally those whose wave-lengths lie between 517 and 569, coralline gives *no increase whatever* of sensitiveness to AgBr.

So that coralline, far from being a support to the theory of Dr. Vogel, gives in fact a confirmation of my own view, viz: that substances heighten the sensitiveness of AgBr to particular rays, without the existence of any relation between the color of that ray and their own proper color.

It is a little curious that coralline acts very differently with AgI. So far from increasing the sensitiveness of that substance to red light, it actually diminishes it.

Rosaniline is another substance whose action conflicts with Dr. Vogel's law. I find that it quite destroys the sensitiveness of AgBr to yellow rays.

From all the foregoing I conclude, that there exists no relation whatever between the color of substances and the color of the ray to whose influence they modify the sensitiveness of silver bromide.

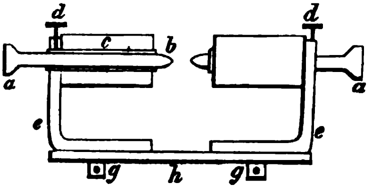
Philadelphia, March 20, 1875.

ART. XXXVIII.—*On a New Diamagnetic Attachment to the Lantern, with a Note on the Theory of the Oscillations of Inductively Magnetized Bodies*; by HENRY A. ROWLAND, Assistant Professor of Physics in the Rensselaer Polytechnic Institute.

1. *Description of Apparatus.*

SOME time ago, in thinking of the theory of diamagmetism, I came to the conclusion that apparatus of large size was by no means necessary in diamagnetic experiments, and on testing my conjectures experimentally, I was much pleased to find that they were true. So that for more than a year I have been in the habit of illustrating this subject to my classes by means of a small apparatus weighing only about a pound or two, which I place in my lantern and magnify to a large size on the screen.

The effects obtained in this way are very fine and are not surpassed by those with the largest magnets; and we are by no means confined to strongly diamagnetic substances, but, with proper care, can use anything, even the most feeble. The apparatus which I used consisted of a horseshoe electro-magnet, made of an iron bar half an inch in diameter and about ten inches long, bent into the proper form, and surrounded with four or five layers of No. 16 wire. But the following apparatus will, without doubt, be found much more convenient. It can be made of any size, though the dimensions given will probably be found convenient.



The apparatus is represented in fig. 1. To a straight bar of iron *h*, 7 in. long, $\frac{1}{2}$ in. thick, and $\frac{3}{4}$ in. wide, are attached two pieces *e e* of the same kind of iron by two set screws *g g*, which move in slots in the piece *h*. Into these pieces are screwed two tubes *c* made of iron and having an internal diameter of about $\frac{7}{16}$ in. and a thickness not to exceed $\frac{1}{16}$ in. Through these tubes the iron rods *a b* slide and are held at any point by the screws *d*. One end *b* of this rod is rounded off for diamagnetic experiments and the other enlarged and flattened at the end for magne-crystallic experiments. On the tube *c* a helix of No. 16 or No. 18 wire is wound so as to make up a thickness of .4 or .5 of an inch and having a length of $2\frac{1}{2}$ in. The object of the screws *g* is principally to allow the rods *a b* to be reversed quickly and to adjust the position of the helices. When the apparatus is to be used for only one kind of work it can be much simplified by doing away with many of the moving parts.

This instrument can be used either with the ordinary magic lantern, or better, with one having a vertical attachment. In the latter case the plane of the instrument is horizontal and the substances are suspended from a wire made quite small, so as not to cut off too much light.

The suspending thread in the case of bismuth can be quite large but for other bodies a single fiber of silk is best: these in the shape of bars half an inch long can be each attached to a fiber having a little wire hook at its upper end and hung in a cabinet until required.

The theory of feebly magnetic or diamagnetic bodies oscillating in a magnetic field is very simple and yet the results are of the greatest interest, especially the effect of the size of the apparatus, which is here given for the first time.

2. *Theory.*

Let a very small particle of a body whose coefficient of magnetization κ is very small, and either positive or negative, be placed in a magnetic field of intensity R; it will then have an induced magnetic moment of κvR, where v is the volume of the element. The force acting on this particle to cause it to go in any given direction will be equal to the product of the magnetic moment into the rate of variation of R in that direction,* and hence is $\varkappa v \mathrm{R}\frac{d\mathrm{R}}{dx}$ in the direction of x. The total force acting on the body in the direction of x is therefore

$$\mathrm{X}=\tfrac{1}{2}\iiint \varkappa \frac{d(\mathrm{R}^2)}{dx}\,dxdydz,$$

and the other components of the force are

$$\mathrm{Y}=\tfrac{1}{2}\iiint \varkappa \frac{d(\mathrm{R}^2)}{dy}\,dxdydz$$

and

$$\mathrm{Z}=\tfrac{1}{2}\iiint \varkappa \frac{d(\mathrm{R}^2)}{dz}\,dxdydz.$$

Let, now, the axis of z be vertical, the axis of x in the line of the magnetic poles of the magnet, and y at right angles to both. Then the moment of the forces acting on the body to turn it about the axis of z is

$$\mathrm{M}=\tfrac{1}{2}\iiint\left(\varkappa\frac{d(\mathrm{R}^2)}{dx}\,y-\varkappa\frac{d(\mathrm{R}^2)}{dy}\,x\right)dxdydz,$$

where the integration extends throughout the volume of the body.

If the body is suspended so as to turn freely about the axis of z it will vibrate about the position for which M is a minimum or else will remain at rest at that point. The number of single oscillations made when the angular elongation ϑ is very small, is

$$n=\frac{1}{\pi}\sqrt{\frac{\mathrm{M}}{\vartheta \mathrm{I}}},$$

in which M and ϑ must be measured simultaneously, and I is the moment of inertia of the body.

$$\therefore\quad n=\frac{1}{\pi\sqrt{2\vartheta \mathrm{I}}}\sqrt{\iiint\varkappa\left(y\frac{d(\mathrm{R}^2)}{dx}-x\frac{d(\mathrm{R}^2)}{dy}\right)dxdydz}.$$

Now let us suppose that the whole apparatus changes size, the relation between the parts remaining constant, so that the apparatus becomes m times as great as before. Then x, y, dx, dy, and dz will increase m times and I, m^5 times. To determine the changes in $\frac{d(\mathrm{R}^2)}{dx}$ and $\frac{d(\mathrm{R}^2)}{dy}$ we make use of the theorem of Sir Wm. Thomson, that "*similar bars of different dimensions, similarly rolled, with lengths of wire proportional to the squares of their linear dimensions, and carrying equal currents, cause equal forces at points similarly situated with reference to them.*"

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* Thomson, Reprint of Papers, Art. 679, Prob. vii.

But as the above only applies to equal currents, I have generalized it in the following. *In any two magnetic systems whatever, similar in all their parts and composed of any number of permanent or electro-magnets, wires carrying currents, or bodies under magnetic induction, the magnetic force at similar points of each will be the same when the following conditions are complied with; 1st. the magnetic materials at similar points in the two systems must be exactly the same in quality and temper; 2d, the permanent magnets must be magnetized to the same degree at similar points of the systems; 3d, the coils of the electro-magnets and other wires or bundles of wires carrying the current must have similar external dimensions in the two systems and must have the product of the current by the number of wires passing through similar sections of the two systems proportional to the linear dimensions of the systems.*

This will apply to the case we are considering when the product of the current by the number of turns of wire varies in direct proportion to the size of the apparatus. Hence in this case $\frac{d(\mathrm{R}^2)}{dx}$ and $\frac{d(\mathrm{R}^2)}{dy}$ will vary inversely as m. Hence we see that n will be inversely proportional to the size of the apparatus: and although we have only proved this for the case when κ is small, it is easy to see that it is perfectly general. The advantage of small diamagnetic apparatus is thus apparent, for the smaller we make it the more vibrations the bar will make in a given time and the more promptly will the results be shown.

It might be thought that by hanging a very small bar in the field of a large magnet, we might obtain just as many vibrations as by the use of a small apparatus: but this is not so, for Sir Wm. Thomson has shown* that the number of oscillations of a feebly magnetic or diamagnetic body of elongated form in a magnetic field is nearly independent of the length when that is short. So that the only way of increasing the number of vibrations is to decrease the size of the *whole* apparatus, or to increase the power of the magnets: the latter has a limit and hence we become dependent on the former.

The theory of the effect of the size of the body is very simple, and we may proceed as follows. Let the body be in the form of a small bar whose sectional area, a, is very small compared with its length, and let γ be the angle of the axis of the bar with the line joining the poles, and r the radius vector from the origin. Developing R^2 as a function of x and y by Taylor's theorem, and noting that as R is symmetrical with reference to the planes XZ and YZ, only the even powers of x and

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* **Reprint of Papers, Art. 670. Remarques sur les oscillations d'aiguilles non crystallisées.**

y can enter into the development, we have, calling R_0 the value of R at the origin,

$$R^2 = R_0^2 + \frac{1}{2}\left(\frac{d^2(R_0^2)}{dx^2}x^2 + \frac{d(R_0^2)}{dy^2}y^2\right)$$
$$+ \frac{1}{2.3.4}\left(\frac{d^4(R_0^2)}{dx^4}x^4 + 6\frac{d^4(R_0^2)}{dx^2dy^2}x^2y^2 + \frac{d^4(R_0^2)}{dy^4}y^4\right) + \&c.$$

When the vibrating body is very small the first two terms will suffice: hence we have

$$M = \frac{1}{2}a\varkappa\left(\frac{d^2(R_0^2)}{dx^2} - \frac{d^2(R_0^2)}{dy^2}\right)\sin\gamma\cos\gamma\int_{-\frac{1}{2}l}^{+\frac{1}{2}l} r^2 dr,$$

in which l is the length of the bar. If S is the density of the body (weight of a unit of volume), $I = \frac{a\,l^3\,\delta}{12}$ and n becomes

$$n = \frac{1}{\pi}\sqrt{\frac{\varkappa}{2\gamma}\left(\frac{d^2(R_0^2)}{dx^2} - \frac{d^2(R_0^2)}{dy^2}\right)},$$

in which, however, it is to be noted that $\frac{d^2(R_0^2)}{dy^2}$ is essentially negative and so the sign of the term containing it will be positive in the actual development.

This equation is independent of the dimensions of the body, and hence we conclude that when the body is small and very long as compared with its other dimensions, the number of vibrations which it will make in a given field is dependent merely on its coefficent of magnetization and on its density; a result first given by Sir Wm. Thomson, in the paper referred to. I have given it once more and put it in its present form merely to call attention to the facility with which $\varkappa$ can be obtained from it when we have measured R in different parts of the field by known methods. This could be done by means of a rotating coil as used by Verdet, or my magnetic proof plane which I will soon describe, combined with my method of using the earth inductor. This will give the best method that I know of for obtaining $\varkappa$ for diamagnetic or weak paramagnetic substances.

Troy, Jan. 15, 1875.

ART. XXXIX.—*On the Primordial Strata of Virginia;* by WM. M. FONTAINE.

MY investigations in the strata of the Primordial period in Virginia were confined to the immediate vicinity of the Blue Ridge, near Harper's Ferry, Rockfish Gap, Balcony Falls, and the Peaks of Otter. They extended to the rocks of the Calciferous epoch. These strata are well exposed along the west base of the Blue Ridge throughout the State.

The careful and accurate studies of these and the overlying formations, made by Prof. Wm. B. Rogers, have thrown much light on the complicated stratigraphy of the region. He shows that in the northern portion of the State, extending from Harper's Ferry to the south end of Page County, the Primordial slates are inverted so as to overlie the later formations on the west, and to pass with a high southeast dip under the like dipping strata of the Blue Ridge. In the middle counties, especially near Balcony Falls, these strata lie with a gentle northwest dip on the west slopes of the Blue Ridge. In some of the more northerly portions of this district, they lie immediately on the edges of the southeast dipping older slates, while at Balcony Falls and its vicinity the two systems of strata are separated by a ridge of syenitic rocks. Farther to the southwest inversion again occurs. This inverted dip is found in all the strata of Silurian age and extends into those of the Devonian. The former occupy nearly all of the "Great Valley of Virginia," while the latter appear in the Great North Mountain, and its prolongations, which bound this valley on the west. The strata of the valley lie generally in a closed synclinal, producing a continued series of southeast dips. This compressed mass of rocks, on the west side of the valley, is thrust up against, and partly over, the Devonian strata, affecting them also with a southeast dip, producing a long line of fault, and bringing Hudson River shales up so as to overlie even strata of the Subcarboniferous period.

It will thus be seen that the amount of displacement produced is enormous. Owing to the contortions thus caused, and the possibility of repetitions from closed folds, all estimates of thickness, unless based upon long-continued examinations, must be received with caution.

I shall in this paper commence with the description of the section obtained at Balcony Falls. This I do because the strata there appear to present their normal development and dip; and also for the reason that here alone has Prof. Rogers given, in any detail, a description of the Primordial strata. It will be seen that my results do not agree entirely with his, and I must confess that I cannot reconcile them. In view of this difference, I should hesitate to maintain the correctness of my observations were it not that they are substantially verified by the section made at Rockfish Gap. This last was made along the cuttings for the railroad at that point, which were not in existence when Prof. Rogers made his examinations.

Before giving my own results at Balcony Falls, I will quote Prof. Rogers's description of the strata there found. He says:

"The formation (his No. I) here rests on igneous rocks, mostly syenitic in character, which form the main axis of the Blue Ridge. These are well seen along the pass. As we approach its termination, we mark the commencement of the rocks of No. I, which are seen on the side of the canal lying on the syenitic mass, with a northwest dip.

The lowest stratum next the syenite is a brownish, decomposing slate, evidently much altered by the syenite. Next is a grayish and reddish sandstone; then a slate similar to the former; then a repetition of the sandstone; again a slate, and, at the termination of the gap, heavy beds of massive white sandstone, the typical rock of this series. The average dip of the latter is, in the cliffs at the entrance of the pass, 55° northwest.

If we now return to the east, by the road leading over the mountain, we are accompanied for a long distance by these upper sandstones, and we then pass in succession over the underlying strata, which extend with a gentle dip almost to the summit of the mountain."

I now proceed to give the result of my own observations. Referring to a former article,* the reader will remember that the last rock described in the Balcony Falls section of the crystalline strata of the Blue Ridge, was a syenite of probably later age than the mass of the ridge. Next to this occurs the first of the rocks which I hold to be of Primordial age. We have: (1.) Unbedded quartzite, with crystals of feldspar, 120 feet. (2.) Brown, crumbling, argillaceous rocks or sandy shale, conglomeritic in its upper portion, 40 feet. (3.) Brown, decomposing, thinly laminated, and contorted shale, 10 feet (4.) Conglomerate, like the upper portion of (2), 20 feet. (5.) Shales, like (3), 12 feet. (6.) Conglomerate, like (4), 15 feet. (7.) Crumbling, brownish shales, passing in the upper portion into argillaceous sandstones of the same hue and texture, 200 feet. (8.) Massively bedded, coarse white quartzite (apparently Rogers's typical No. I), 500 feet. Up to this point the rocks are well exposed in the cliffs which closely border the canal. Hence there is no danger of overlooking reduplications.

The dip of (3) is confused by contortion; that of (4) is 70°. From this point the inclination gradually declines, until in (8) it is about 55°. Proceeding west across the strike, we next encounter a series (9) of thinly laminated shales, with some beds of red and gray sandstones. This series is much more disturbed than the preceding, and, owing to its having been long subjected to the action of the river, in its efforts to cut through the massive barrier interposed by (8), it presents to the canal, slopes covered with debris. Hence details of the succession and dip of the strata cannot be given with accuracy.

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* This Journal, January and February, 1875.

A better exposure is given on the road mentioned above in Rogers's description. Here, too, however, great confusion exists. By far the greater portion of the mass is composed of thinly laminated, gray and reddish shales. Some beds of brick-red and gray sandstones of coarse texture occur, much indurated, and sometimes impregnated with epidote, chlorite, etc. Also, one or two beds of diorite apparently. Probable thickness, 600 feet.

To the west this is overlaid by a great series (10) of alternating beds of quartzite and kaolin shales. These rocks, where they join (9), present the same contortions and confused stratification, but in passing farther west, the folds open out, and are finally seen well displayed in the high mountain through which the river cuts its way at the "Cement Mills."

This mountain is the most westerly of the parallel ranges which compose the Blue Ridge at this place. At its western base the James and North Rivers meet, and force a passage, exactly at the point of junction. We thus find here the same features as those presented by the Potomac and Shenandoah at Harper's Ferry.

Standing at the Cement Mills, just where the river issues from the gorge and in this first barrier, the perpendicular walls of the opposite bank are seen to be composed of beautifully regular arches of No. (10), piled one above another to a great height. Probable thickness, 700 feet.

Both (9) and (10) seem, by a mighty thrust from the west, to have been crowded high up against the firm barrier opposed by the syenitic mass. This accounts for the gentle northwest dip which, as Prof Rogers correctly states, is shown by these upper strata in passing over the mountain to the east. In taking the section along this road, I did not find this to be true of the lower strata up to No. (8). They, even near the top of the syenitic mountain, show the same high dip that is seen along the canal. The massive, unyielding plates of No. (8) seem to have protected them to a certain extent, but have themselves been extensively fractured. Hence, in passing along the mountain road from the mills to the east, we observe the following typographical features: Looking westward, we see the strata of (10) rising and falling in rapid symmetrical undulations and forming the lofty range which fronts the valley of the river. Proceeding eastward, up the western slopes of the syenitic range, we pass first over the broken fragments of (10) and then of (9). Both, by the flattening out of the undulations, have a gentle northwest dip, and are partly crowded over the fractured outcrop of (8). Next appear the lower strata in unchanged position.

Returning to the line of section, we find the western slopes of the mountain overlooking the valley of the James to be covered high up with boulders and alluvial matter, by which the junction of (10) with the next series (11), and some adjoining space, is concealed. The river has at some time flowed several hundred feet higher than its present level.

The series of strata which I have numbered (11) in this section occupies a narrow belt between the mountain last mentioned and James River. The dip is very regular, being about 55° to the northwest. The rocks, so far as seen, next to the mountain, are more or less ferruginous, and local pockets of iron ore occur in the sandstones of (10) forming the mountain, especially near the line of junction.

The first rock seen next to the mountain is thinly laminated, fragile shale, of yellowish and reddish hues, not fully exposed. Next, to the west, we have bluish calcareous slate, and slaty limestone, 50 feet. Then very thinly laminated, firm, deep red slates, 60 feet. Next a similar slate of blue color, 15 feet. Then a bed of dark blue, hard, and dense limestone, used for cement, 13 feet. Then thinly laminated red slates, 20 feet. Then coarser, and thicker bedded, blue slates, 40 feet. Lastly, a coarse, rough, massive, siliceous limestone, 40 feet.

These strata are all well exposed in the excavations made for gaining the cement stone. This group of impure limestones is the equivalent, in Virginia, of the "Calciferous sandrock," and is present at all the sections examined by me, holding a similar position with respect to the Potsdam strata. They present, also, remarkably constant features in composition, especially the last mentioned, which, both at Rockfish Gap and Harper's Ferry, may be recognized at a glance. The cement limestone, from its physical appearance, is the purest of the group. The following is its analysis, obtained from the owner of the mills: SiO_2 34·225; Al_2O_3 with a little Fe_2O_3 7·8; CO_2 30·4; CaO 17·380; MgO 9·513; KO ·12; water and organic matter, ·5; loss, ·062. The thickness of this group, up to the last mentioned limestone, is probably 600 feet, including the concealed ground. My section did not continue beyond this point.

On the west side of the river is a valley, from half to three-quarters of a mile wide, which is bounded on the west by a range of heights called "Salling's Mountain." I observed in the western banks of the stream about eighty feet of fine-grained kaolin shales and sandstone, like those of (10), but more shaly. These have the same northwest dip; how far they extend into the valley I do not know. Prof. Rogers says that Salling's Mountain is composed of brown shales, which he makes the upper member of his No. I, or Potsdam formation. He further says that in them the dip is inverted, or to the southeast, the limestones of the Great Valley, i. e., the Trenton, Chazy, &c. (his No. II), passing under them. My section not having extended into these strata, I can say nothing concerning their age, except that they would seem to overlie the Calciferous sandrock, as the continued section above given extends through the Potsdam strata to those of the Calciferous. From this change of dip it appears that in this region the break and overturn of strata, which elsewhere extends through all the Primordial rocks, is found only in the strata overlying the Calciferous.

Some of the strata given in the above section present features of special interest, which must now be noticed.

The quartzite, No. (1), which immediately joins the syenite, is noteworthy as being the most completely metamorphosed rock seen in the entire section. This is evidently due to its contact with the crystalline rock. It is without bedding, and is affected by the same system of northeast and southwest joints seen in the syenite, and apparently forms one mass with the latter, no distinct line of junction being visible. The texture is nearly compact, from the fusion of the quartz grains. Numerous brilliant, perfectly crystallized, particles of white feldspar of small size occur. A few small, imperfectly crystallized garnets are seen in it. The next rock, No. (2), next to the quartzite, is a brown, crumbling, shaly mass, without bedding, which at its western edge terminates in a bed of feldspathic conglomerate. This has a high inclination to the northwest. The appearance of the lower portion of No. (2), and the highly altered condition of (1), seem to show that the upheaval of the adjacent portion of the syenite took place after their deposition, while the succeeding conglomerates are evidently the products of this disturbance. These conglomerates all closely resemble each other, and present some interesting features. They are composed of water-worn particles of quartz and red feldspar, of all sizes, up to that of a buckshot. The feldspar largely predominates, and is remarkable for its freshness, showing even in very small particles brilliant cleavage surfaces. Both it and the quartz are plainly derived from the neighboring syenite. The deposition must have taken place rapidly. The coarser particles are cemented together by a dark gray, very fine-grained paste, having the appearance, texture, and hardness of talc slate. It is apparently formed of finely comminuted feldspar and argillaceous matter. The entire mass weathers by exfoliation of concentric crusts, producing rounded boulders most strikingly like an igneous rock. Indeed, I was induced by this peculiarity to study closely its composition, under the impression that such was its nature.

These conglomerates crop out on the top of the mountain, and are partially exposed in the road. Here I discovered that the upper part of No. (4) was composed of a mass of angular fragments of slate cemented together by conglomeritic matter. These fragments of slate, sometimes as large as the hand, are of precisely the same character as the argillites which make up so large a part of the Blue Ridge, especially to the north. Their existence here shows not only that these argillites are older than the lowest Primordial rocks, but that they were already in a metamorphosed condition prior to the Primordial Period. The shales lying between the beds of conglomerate are very much contorted, and they, with the succeeding strata up to (8), seem to be mainly composed of finely comminuted feldspathic and argillaceous material, which had not been long exposed to decomposing and dissolving agencies.

The massive beds numbered (8) in the section differ strikingly from the underlying rocks in every respect. The material composing them is almost entirely quartz and of a white color, the grains being more or less rounded and cemented together. A few specks of yellow earthy matter occur, marking the position of decomposed particles of feldspar. With this exception, the rock is remarkably free from all minerals except quartz. If I mistake not, this is the rock mentioned by Prof. Rogers as typical No. 1, or Potsdam. But from his descriptions, given elsewhere in his reports, the strata which I have numbered (10) coincide more nearly with his sandstone, No. I. This also agrees best with the relative positions of No. (10), and of the Calciferous sandrock, which forms the upper part of No. (11).

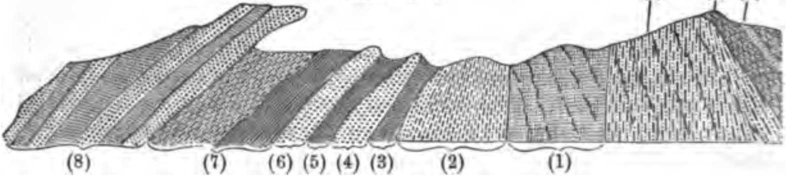
We may pass over the strata forming (9) in the section, as they lie in such confusion that it would be unsafe to attempt to give details. I may state, however, that the sandstones which occur in the shales of this series are quite thin. The coarsest and most massive layers are not more than 8 or 10 feet thick. It is worthy of note that these sandstones are much more altered than the shales by the disturbances to which they have been subjected. The shales seem hardly at all affected, while the sandstones are much indurated and often impregnated with epidotic and other metamorphic products. The material composing the shales is gray, argillaceous matter, with some beds of brown and reddish colors.

No. (10). This series, which is probably the equivalent of the Potsdam sandstone of New York, is composed of alternating strata of quartzite and kaolin shales. The quartzite layers vary in thickness from 20 feet to 3 or 4 feet. Many are quite compact, and some resemble vein quartz in fracture and other features.

The quartzites predominate in the lower beds. The kaolin shales often pass into a fine-grained sandstone, in which the grains of sand and kaolin of uniform size are equally and uniformly mixed. This latter rock, which is very common in the Potsdam of the northern part of the State, appears in great force at Rockfish Gap, and will be described in the section there more fully. The strata of (10) are remarkable for their freedom from coloring matters, and for the thorough decomposition of the feldspathic matter present. The kaolin shales and shaly sandstones occur in plates of a few inches to a foot or more in thickness. They are much fractured by the extraordinary disturbance to which they have been subjected. Notwithstanding the great amount of motion which has taken place in these strata, the shales do not seem to be metamorphosed beyond perhaps a partial loss of plasticity. Here again the sandstones show greater alteration. It is generally true, I think, that beds of pure argillaceous matter undergo far less metamorphosis in composition and texture than the siliceous layers which are interstratified with them. This at least is very generally true of the disturbed rocks of the Silurian and Devonian areas in Virginia and elsewhere, where I have observed them. The usual effect on these shales of the compression, &c., produced by disturbances is to cause them to assume more or less of the slaty cleavage, but I have never seen any approach to general crystallization of the constituents, even in the most profoundly convulsed districts. Again, the change in the beds of sandstone is usually the consolidation of the individual grains of sand. When foreign material is introduced, it is always in the form of local impregnations, plainly caused by circulating heated waters. I will return to this subject later. The strata of No. (11) do not require a further description.

The following section, No. I, represents the strata from (1) to (8) inclusive. No. (8) rises in a cliff 200–300 feet high, leaning over in an arch toward the syenite.

SECTION I.

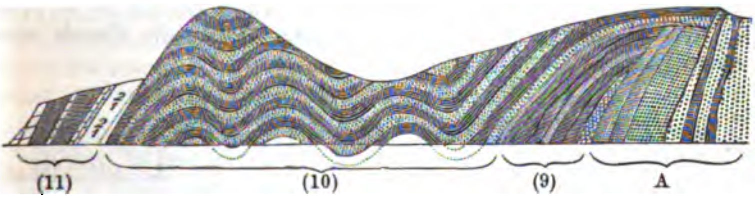


In this section, (*a*) represents the bed of quartzite mentioned in a former paper, as occurring on the east side of the syenitic mountain, while (*b*) denotes a portion of the mica slates which lean against this range on the east, with a southeast dip. The strata numbered from (1) to (8) denote those thus numbered in the descriptions, while (*c*) represents the syenites against which these lean with a high northwest dip. This section represents the stratification on the canal, but on the slopes of the mountain ascending from the west (8) and the overlying strata are crowded over to some extent on the outcrop of the subjacent rocks.

From the occurrence of the quartzite stratum at (*a*), and the fact that the Primordial strata ascend to the summit of the syenitic mountain, it might, at the first glance, be supposed that the slates at (*b*) are merely the extension of the strata on the west in a more highly altered condition. But this idea is forbidden by the great difference in the thickness of the western and eastern quartzites, and by the total change in the character of the beds, those on the east being a great mass of mica slates, quite regularly bedded, and showing none of the alternations of strata seen but a little distance away on the west. This relative position of the two systems of bedded rocks explains the fact mentioned by Prof. Rogers as occurring in the more northerly portion of the State, where Primordial strata with northwest dip lie upon the edges of the slates of the ridges which dip to the southeast. This would occur here were the syenitic mass absent, and, as we shall see, it does not form a dividing ridge farther north. The following section, No. II, gives a representation of the stratification of series (9), (10) and (11). As far as (9) and (10) are concerned, it is in part ideal, and I do not insist upon the correctness of my estimate of their thickness.

SECTION II.

A. No. (8) with subjacent strata. (9) Gray and reddish shales and sandstones.
(10) Potsdam shales and quartzites. (11) Upper shales and Calciferous strata.



PEAKS OF OTTER.

No detailed examination was made at this point of the Primordial rocks. At the west base of the syenitic range, gray shales and kaolin shales, very imperfectly exposed, were seen to pass a little distance up the mountain. They have a high southeast dip, showing the inversion of the lowest portion of the Primordial strata in this region. It is probable that some of the lower strata have been ingulfed in the overturn. Prof. Rogers states that from this point to the southwest the inversion generally prevails.

[To be continued.]

ART. XL.—*On the Silurian age of the Southern Appalachians;* by FRANK H. BRADLEY.

[Continued from page 288.]

[SINCE the previous part of this article was in type, another trip has been made from Athens to Murphy, by a road a short distance farther north than the one previously used. The section is essentially the same as that already reported, except that, after passing the soft schists of the Ducktown anticlinal, at the Hanging Dog Iron Works, the Ocoee and Chilhowee rocks, intervening between this anticlinal and the Valley River marble, have been forced past the vertical, and are exposed in inverted order, with northwesterly dips, until we have almost reached the valley at Tomotla, where southeast dips appear.

On page 284, line 13 from top, for "At Athens," read "Just south of Athens," for, about the railroad station, the Knox dolomyte shows an abundance of its characteristic chert, and the creek banks near the court-house have exposures of the shaly limestone of the Chazy epoch. The section was commenced perhaps a half mile south of the court-house.

Through inexcusable oversight, two errors have remained on page 286, after revision. As Safford's report and map both plainly show, the Knoxville fault passes a little north of the railroad at Athens, instead of two miles south of that place. The fault noticed at the latter point passes about nine miles south of Knoxville, where, though not mentioned in the section given, it separates the Cincinnati "iron-limestones" from the Knox dolomyte, both of which beds have there, locally, southeast dips. The Maryville outcrop of Knox dolomyte is thus shown to be the same one met with at Middle Creek and vicinity.]

5. *The Marble Belt.*

Three or four miles to the southwest of Dehart's, on Silver Creek, a tributary of the Nantahala, a heavy bed of marble is reported, which is supposed to carry silver ores. Farther southwest, perhaps ten miles from the Little Tennessee, its outcrop strikes the main valley of the lower Nantahala, not far below the Blowing Spring, and follows it, perhaps four miles, to Red Marble Creek, which it ascends to its head at Red Marble Gap, and thence descends Valley River. From the Blowing Spring to the Red Marble Gap the bed shows considerable quantities of a bright pink or flesh-colored marble, more or less variegated, in a patchy way, with various light shades of green and greenish-white, due mainly to the presence of more or less talc, which is sometimes in pieces large enough to make considerable flaws, or "dries," as the marble-workers call them. The value of the material as an ornamental stone is still further diminished by the frequent dissemination of small grains of quartz through the mass, which prevent a perfect polish. At some points along Red Marble Creek, especially near its mouth, we find large portions of the marble even shaly, with talc or chlorite, or both, and very much disturbed and intermingled with chlorite schists. Some of these impure portions are of a rich chocolate brown. On the mountain-side, northwest of the creek, ascending the road leading from its mouth across to the head of Tallula Fork of Cheowa, we find a second outcrop of the marble, mainly blue and bluish-white and partly chloritic, held in the trough of a small subordinate fold, as was suggested with respect to the disturbed strata between Valley River and Cheowa. Higher up, the underlying chlorite slates are so much distorted that, at one point, dips of S. 60° E. and S. 55° W. were noticed within ten feet of each other, in the solid strata.

The local fold just mentioned seems to be converted into a fault, as we pass southwestward, toward Valleytown. The main line of marble-outcrop, passing through Red Marble Gap, runs by Swepson's and Ingraham's localities, making deep and long hollows as it crosses the mountain-spurs, crosses Junaluska Creek, near Valleytown, and apparently continues nearly to Paint Creek, where the overlying pyritous and micaceous limestone outcrops, just southeast of the fault. Higher up, as on Junaluska Creek, the marble is underlaid by siliceous, chloritic and hydromica schists, of the age of the Knox shale, followed below by gneisses, partly schistose, partly quartzytic, representing the Knox and Chilhowee sandstones, which latter form a high ridge, for several miles, between this lead of marble and the upper part of the Valley River valley. On the northwest side of the fault, the marble makes its appearance about a mile above Valleytown, outcrops at several points near Whittaker's house, where a little quarrying has been done, and continues, apparently without interruption, down the valley to Murphy, where it crosses the Hiwassee. Thence, crossing the lower course of Notla River, it crosses the lower part of Hemptown Creek, then crosses the Ocoee River and continues to the White Path gold mines and Ellijoy, in Gilmer County, Ga., where we shall meet it again. The line thus traced is, in direct course, over sixty miles in length. Without more detailed surveys along the whole line, we cannot be sure that there are no other breaks in its continuity; but, as thus far known, the Valleytown fault appears to be the only very important one. Just opposite Valleytown, a partial break exposes another outcrop of the underlying gneiss, between Whittaker's and Collett's; but the two strips of the marble are united, as they pass southwestward, at perhaps ten miles below Valleytown. In this neighborhood, there are also local foldings of the overlying strata, but not such as to interfere greatly with the general regularity of the bedding.

Though the materials of the several outcrops are quite variable, yet the variation is of so constant a character in all, that one would have little hesitation in recognizing them as parts of one bed, even without the stratigraphical reasons for the conclusion. Along Valley River, and southwestward, the rock is mainly a bluish or grayish-white, fine-grained marble, the pink tinge of the Nantahala outcrops being rarely seen and then in very pale tints. One layer, from eight to twelve feet thick, is nearly black, or black-and-white striped, compact, and capable of taking a fine polish, thus forming a very beautiful ornamental stone, but still showing, under the glass, an oölitic structure like that of many of the lower layers of the unaltered Knox limestone. This was particularly noticed on J. T. Young's land, about a mile northeast of Valleytown, and at the mouth of Lenoir's shaft on lot "No. 6," near Murphy. The full thickness of the marble, near Murphy, is estimated at about four hundred feet.

Aside from disseminated grains of sand, which are sometimes very abundant, though at others entirely absent, the principal impurity of the marble is tremolite, which is sometimes present in quantity, either in separate, long, bladed crystals, or in finely radiated clusters, or in large, almost granular masses. It is said to form, at some points, considerable layers, consisting solely of it, which is certainly true at outcrops further south, near Gainesville, Hall County, Ga. As this mineral is essentially a silicate of lime and magnesia, it is natural to refer its origin to the abundant chert of the unaltered Knox dolomyte, which carries so much dolomyte in its intimate structure that it very readily disintegrates when exposed for some time to the action of percolating rain-water. At some points, as near Collett's, the marble is coarse-grained, and filled with small crystals of pyrite. At "No. 6," there is a thin layer of cellular quartz, possibly a true vein, filled with crystals of calcite, disseminated grains and masses of argentiferous galenite and some free gold.* The gravel along the outcrop, for several miles,

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* The deep weathering away of the marble has caused the accumulation, upon the outcrop, of the gold originally disseminated through a large amount of rock; and surface-washings have therefore paid enormously.

The vein-stuff, at "No. 6," as exposed by shaft and drift, about fifty feet below the outcrop, shows the following section:—

White, fine-grained marble, with galenite, calcite and thin quartz veins,	5 to 6 feet.
Coarse-granular tremolyte,	2 to 6 inches.
Flinty quartz, with little calcite,	15 inches.
Yellowish-brown and greenish slaty limestone,	1 foot.
Green and brown, compact, knotty, hydromica slate, with disseminated pyrite, including streaks and thin layers of coarse-granular marble,	4 feet.

pays well for washing; and it is therefore inferred that this vein, or others like it, must be, so far, constantly present in the marble; indeed, similar vein-stuff has been seen in the outcrop, several miles up Valley River. I have not learned of the presence of sphalerite in the marble at any point.

One of the most common accompaniments of the marble, in this neighborhood, from the Blowing Spring to the Ocoee, and perhaps farther, is a bed of very pure steatite, or "cotton-rock," as it is locally called. This has been quarried, in small quantities, at several points, partly for the lining of furnaces at the Ducktown Copper Works, partly for the making of the so-called "lava" tips for gas-burners. Mr. A. A. Campbell of Tomotla, who has quarried much of it, tells me that he has sometimes found it filling small basins in the surface of the marble; but, as I understand his statement, this may have resulted from local sharp folds of the strata, and the steatite may still be a regular overlying layer conformable with the marble, as I believe to be generally the case. The outcrops, however, are not favorable to a decision on this point. At Tomotla P. O., the steatite lies in a bed, 8 or 9 feet thick, *below* the marble, and separated from it by a bed of granular quartzyte. Below the limit of precolation and consequent decomposition, the steatite is of a rich green tint, translucent, and endures sudden heating; when partially altered, it is opaque, of a rusty yellow color, and, by reason of absorbed water, splinters when quickly heated, but gradually hardens like the unaltered rock. At one pit, on Notla River, the surface is covered with a dense blue mud, which appear to be the ultimate result of underground decomposition. The rock sometimes contains tremolite crystals.

The marble is also pretty constantly accompanied, hereabout, by thin beds of cellular chert and itacolumyte (flexible sandstone), sometimes separate, sometimes running into each other. These, I believe, mainly overlie the marble. A good point for examining them is the neighborhood of Red Marble Gap.

Heavy beds of limonite, compact, cellular and ochreous, partly pure, partly cherty, also follow the outcrops of the marble, and have been mined, or rather quarried, at several points, for the supply of small furnaces and forges. These are of essentially the same character as the limonites which are so abundant along the outcrops of the unaltered or slightly-altered Knox dolomyte throughout East Tennessee, Georgia and Alabama, and extend northeastward to New England and Canada; but some of them are considered to be exceptionally pure. They are said to carry, at most points, small amounts of gold and silver, though rarely enough to make them worth working for those metals. These ores occur at four well-marked horizons, one immediately beneath the marble, the others above it, as shown in the accompanying section of the rocks at and near Murphy:

1.	Hydromica schists, smooth and wrinkled, ferruginous, with garnets, staurolites, titanic iron and gold—some quartz veins.		
2.	Fine-grained chloritic gneiss.		
3.	Talcoid schists, with mica crystals.		
4.	Dark greenish-black hornblende rock, with garnets.		
5.	Micaceous limestone, with pyrite, weathering to *limonite*	500 +	feet.
6.	Hydromica schists	150	“
7.	*Limonites* (rarely mined)	50 to 70	“
8.	Hydromica schists	100	“
9.	Knotty quartzytes	65	“
10.	Hydromica schists	25	“
11.	Slaty steatyte,	30	“
12.	Soft schists and gneisses, with streaks of speckled mica slates,	250	“
13.	Hydromica schists, with *limonites*,	150 to 200	“
14.	Marble	400	“
15.	Limonites	100 to 200	“
16.	Quartzyte.		
17.	Heavy-bedded gneiss.		

No. 5 of this is the evident source of one of these limonite belts: and the others may very probably have similar origin; but I have not seen any exposure of the strata beneath them. This pyritous limestone was seen in the ridge between Professor Olmsted's house and Lenoir's shaft, near Murphy; also at the Parker gold mine, 11 miles northeast; also near Whittaker's (now Ross's) Mill, 14 miles northeast; also on Paint Creek, near Valleytown. The heaviest outcrop seen is at the Parker Mine. As the section shows, this bed, near Murphy, lies about 900 feet above the marble, which we suppose to represent the Knox dolomyte, though much thinner than that group is in its Tennessee exposures. This distance would not vary much from the space separating the Knox from the iron-limestone of the Cincinnati group near Knoxville, and I am strongly impressed with the belief that the pyritous and micaceous limestone is the equivalent of this latter bed, because of both its chemical composition and its position. The hornblende rock of No. 4 of the section may perhaps be included, since the dark-colored hornblendes are essentially silicates of lime, magnesia and iron; but, if so, we must also include all the overlying beds of the section, since rock of precisely the same characters occurs, in thin layers, at intervals, throughout the schists of No. 1, on the ridge back of the Parker Mine. I am at present inclined to thus include these schists, and so to refer the whole upper part of the section to the Cincinnati group.

About four miles above Murphy, on Peachtree Creek, another outcrop of marble occurs, which *appears* to be the equivalent of that already discussed, which requires the existence of a considerable fault somewhere between the two outcrops. I am not entirely certain as to the location of this fault, since the strata vary locally to such an extent, hereabout. I believe, however, that it is very close to the Peachtree outcrop. Accordingly, I would refer all the intervening strata to either the upper part of the Cincinnati group or the overlying beds of the Silurian. Ascending the Hiwassee, after leaving the auriferous hydromica schists of No. 1 of the foregoing section, which underlie most of the town of Murphy and extend nearly a half mile up the river, we find, first, a fine-grained, ferruginous micaceous, quartzytic sandstone; then, a thin bed of chlorite slate; then, a thousand feet or more of fine-grained gray sandstones, with quartz veins; then, white, gray and ferruginous quartzytes, partly heavy-bedded, with pyritiferous quartz veins, interlaminated with siliceous mica slate and hydromica schists, at least three thousand feet in all; then, several hundred feet of mottled hydromica slates, with mica crystals, a few staurolites, and some gold. This brings us to the mouth of Peachtree, on the line of strike of this marble outcrop: though the marble does not appear near and south of the crossing of the Hiwassee, yet the accompanying limonites, which are abundant on Peachtree, are also largely developed beyond the river, and a low valley, evidently eroded in soft strata, runs on for some miles toward the upper course of the Notla River. On Peachtree Creek, near M. L. Brittain's, the marble shows white, blue, gray and reddish-brown layers, the darker varieties being often quite impure with streaks of schist, partly siliceous. Beneath them, a pyritous steatite occurs at one point, which must be the equivalent of the "cotton-rock" above described; but the exposed outcrop is much more impure than that on Valley River and Notla. Above the marble, there is here a garnetiferous schistose brown limestone, which graduates into a calcareous mica schist. These beds, over 2,000 feet thick, form a high ridge, which, in its more eastern portion, between the two forks of Peachtree, is known as Breeding Mountain: an isolated portion lies between lower Peachtree and the Mission farm on Hiwassee River; and, south of the river, the line continues on between this belt of marble and Little Brasstown Creek. Just beyond this, another fault again brings up the marble, which outcrops at several points on Little Brasstown and on and near the Mission farm, being accompanied as usual by heavy beds of limonites. These limonites, both here and on Peachtree, are largely ochres, and are said to contain considerble amounts of silver, copper and lead. Above the Mission farm, the overlying calcareous mica schist forms another high ridge, which comes sharply to the river bank on both sides. Those two belts of marble are apparently broken off toward the northeast, though each has been traced some distance into the mountains, by lines of sink-holes. They probably reach the more southern branches of Vengeance's Creek, but do not appear on its more northern ones. Their southwestern continuations have not been traced continuously; but outcrops of marble, representing one at least and probably both, occur on Notla River, about at the State line. The marble of Jasper, Pickens County, Ga., is probably on the continuation of one of these belts.

In passing on up Brasstown Creek, we find principally mica and hydromica slates and schists, with many staurolites and small garnets, and increasingly numerous quartz veins which, as we approach the Georgia line, become pyritous and frequently auriferous. On both sides of the line, veins rich in gold have been opened, though none has yet been extensively worked. Two small stamp-mills are now running, in this neighborhood. In some of these veins, argentiferous galenite occurs in considerable quantities.

Somewhere near this point, both Emmons and Kerr indicate a great fault, separating the so-called "Taconic" or "Huronian," on the northwest, from the "Azoic" or "Laurentian," on the southeast. I cannot certify to the non-existence of such a fault; but I have crossed its supposed line and the succeeding belt of country for several miles, on three distinct routes, without having found any appearance of unconformability or any very marked change in rock characters, except that hornblendic strata become more numerous at some points. In ascending the Little Tennessee, from where we have supposed the main belt of marble to cross it, we find schists and gneisses dipping southeasterly, until within nine miles of Franklin, where northwesterly dips set in, continuing about five miles, until we meet with decayed, auriferous, hydromica schists dipping 45° to 60°, to N. 10°–15° E. As this has been the eastern limit of my explorations, I am not *sure* that this may not mark an approach to another series of rocks; but the character of the material does not indicate this, and I am inclined to believe that the *northeasterly* dips are only local, and that the limestone reported by Dr. Curtis as existing in considerable quantity on the western slope of the Cowee Mountains, about eight miles east of Franklin, is really the southeastern edge of the synclinal of which the Nantahala and Valley River outcrop is the northwestern. At least, I have found no definite boundary to the Silurian in this direction.

If there are *any* non-Silurian beds in this region, they are those which form the higher portions of the Valley River

Mountains and Nantahala Mountains. On the lower Nantahala, opposite the mouth of Red Marble Creek, an extensive outcrop of chloritic gneisses, dipping 55°, S. 65°–70° E., commences about two hundred yards southeast of the marble, which here dips 57°, S. 57° E. The intervening space is covered, but there is no indication of the presence of the pyritous, micaceous limestone which, farther southwest, occupies nearly the place of these gneisses. In this region of great disturbance, neither the variation in dip nor that in lithological character is sufficient to justify the inference of unconformability; yet there *may* be a fault here and displacement either up or down on its southeast side. Leaving the marble at Valleytown, and following the direct road to Franklin, we first cross a wide outcrop of hydromica schists; then, a thin bed of laminated quartzyte; then, chloritic and mica schists, gradually becoming more compact and siliceous and including some quartzytes. Until we pass the summit of the Valley River Mountains, at Morgan's Gap, the dips vary from 35° to 60°, and from S. 20° E. to S. 45° E., apparently in consequence of warpings of the strata rather than of any true unconformability; but, upon the eastern slope these are replaced by dips varying from S. 20° E. to S. 60° W., as if in the greatly disturbed strata about the axis of a very irregular synclinal. Both gneisses and schists here show more mica than chlorite. The irregularity of the dips continues until we pass the summit of the Nantahala Mountains, where the synclinal character of the region appears to be more strongly marked in sharp northwesterly dips, which continue down the southeastern slope. The rocks have become less schistose and more gneissoid, and include much hornblende. After reaching the valley, we encounter the auriferous hydromica schists, dipping N. 15° E. as before stated, which continue to Franklin. While I have not made sufficiently detailed examinations to be *certain* that these disturbed strata of the upper Nantahala are not of greater age, yet what I have seen of them has given me a very strong impression that they are *newer* than the Valley River marbles. This impression has only been strengthened by a trip from Parker's Mine to the head of Vengeance's Creek. Along this line, chloritic schists, which *must* be above the marble, gradually pass into others a little more compact and siliceous, (the precise equivalents of those first encountered on the Valleytown-Franklin road,) with laminated quartzytes. Near the summit, these include a considerable thickness of fine-grained nearly black mica slates, becoming siliceous above and striped with quartzytes.

In passing southeastward from Brasstown Creek toward the head of Hiwassee River, we find southeast dips at first; but these soon begin to alternate with northwesterly ones, showing such small local wrinkles as we should naturally expect along the somewhat irregular axis of a sharp anticlinal. Hornblendic layers here become rather more abundant, though there are still several belts of soft hydromica schists, with thin quartz bands, some of which are rich in gold. One of these latter, continued westerly, passes Blairsville, Union County, Ga., where rich ore has been found, though no systematic mining has been attempted. About two miles south of Hiwassee, Towns County, at Ivy Mount Mine, some of the hornblendic layers include copper ores, similar to those of Ducktown. though not yet fairly developed. Old works, followed on a small scale before the war, have recently been re-opened, and show very promising bodies of ore.*

Passing on southward, on the Unaka Road, we find the dividing ridge, or Blue Ridge proper, consisting of mostly heavy-bedded gneisses, partly hornblendic, with some schistose portions, all dipping northwesterly in a regular monoclinal. Descending the southern slopes of this last prominent range of the Appalachian chain, the foot slopes show a broad outcrop of the soft auriferous mica schists, with thin irregular quartz veins and occasional layers of gneiss, quartzyte and itacolumyte. These appear to be the opposing edges of the set of beds passing Hiwassee and Blairsville.† The recognized "gold belt" extends to beyond the Chattahoochee, and mining has been carried on, in the small way, at a great many points. Near Cleveland (formerly called Mount Yonah), we reach the axis of an anticlinal, along which appear heavy-bedded gneisses, which form an interrupted line of high knobs, running parallel with the main mountain. Of these, Yonah Mountain, in White County, and Walker's Mountain, in Lumpkin County, are the more prominent. These beds yield no gold; but the auriferous schists immediately recur and show almost continuously for at least sixteen miles south of Clarksville, Habersham Co. Southeasterly dips continue from the Yonah anticlinal to about ten miles south of C., where the synclinal of the Chattahoochee Ridge gives us northwesterly ones, which continue about five miles, to near Hollingsworth, where we pass another anticlinal and again find southeast dips. Besides the hydromica schists, with quartz veins and bands, which constitute the mass of the strata, we find some chlorite slates, and a few beds of gneiss, the latter partly granitoid, partly porphyritic. A few miles east of Clarksville, considerable quantities of marble have been quarried and burned into lime. The position of this outcrop indicates that the bed probably has a southeasterly dip, passes very near Clarksville (possibly along the depression of the valley of Soquee Creek), and is continuous with the outcrop at Limestone Spring, about two miles east of Gainesville. This bed must also occur on the north side of the anticlinal, along the foot of the mountain, but is not yet reported. In passing from the Yonah anticlinal direct to Gainesville, Hall Co., a second anticlinal axis is passed, near the crossing of the Chattahoochee, which is apparently not developed much farther east, as it was not seen on the Clarksville road. Near this river crossing, at the Glade Mines, a prominent bed of itacolumyte is reported; and several diamonds have been found in the gold washings. The marble, near the Limestone Spring, is essentially the same as that of the Valley River belt, but contains, perhaps, rather more impurities, heavy layers being here solid masses of tremolite. So far as seen, the immediately overlying beds were so covered with debris as to prevent comparison with those seen about Murphy. The Glade mines being very near this belt, one naturally connects the heavy limonite beds of that place with the similar ones of Murphy and vicinity. Silver ore is also reported from this vicinity.

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* The inclosing wallings are of very compact gneiss, partly hornblendic. The ore is pyritous, and carries large amounts of zinc-blende (sphalenite). The quartz gangue is at some points full of apatite, from whose alteration we have, in the crevices about the lower limit of decomposition, considerable surfaces of vivianite, in pale blue crystals.

† In mining reports upon this region, prepared in 1861, W. P. Blake described these strata as being metamorphosed Paleozoic.

Passing westward and northwestward from Gainesville, we reach the second anticlinal, just mentioned, about a mile from town, with dips at first about S. 75°–80° W., which are the first indications of approach to the irregularities which mark the *southwestern extremity of the Blue Ridge proper.* The local facts to be stated will perhaps be best understood if the general features of this southern part of the range are first briefly set forth. The Blue Ridge, which is a single range in northern Virginia, forks a few miles southwest of Lynchburg; and the two ranges thus formed gradually separate, to the southwestward, until, in middle and southern North Carolina, they are from sixty to seventy miles apart. The main strikes are S. 45°–65° W.; and, in the eastern range, which is the main watershed, these continue through Georgia into Alabama; but the western range, almost from the very point where it enters Georgia, bears more to the southward, the strikes being mostly about S. 5°–15° W., and rapidly approaches the eastern. This western range, as such, terminates before reaching the Etowah River; but the disturbances consequent upon the crowding of folds have been felt far to the southward and eastward, as we have seen.

After crossing the Shallow Ford of the Chattahoochee, we find more nearly the normal dips of this side of the anticlinal, in a heavy outcrop of thin-bedded gneisses, which dip 30°, N. 62° W., and in the overlying auriferous hydromica schists, banded with thin layers of quartz and hornblendic gneiss. As we approach the synclinal, disturbed dips again appear. We here reach nearly the horizon of the marble, but pass the synclinal axis without its appearance, about a mile west of the Chestatee River, which we cross near Robbins's mill. Dips of S. 25°–40° E., in gneisses and schists, continue until we have passed Crossville, Smithville and Barretsville, and have crossed the Etowah at Leadbetter's Ford. Somewhere in this space the marble must occur; but it was not seen in passing along the road. About twelve miles south of Jasper, we reach the line of probably the greatest crowding of folds and disturbance of dips. For the next four miles northward, the rocks are mainly hydromica schists and gneisses, with quartz and granite veins. Within this space, the following succession of dips was noted: N. 35°–70° E.; N. 20°–30° W.; S. 20°–70° E.; S. 35° W.; S. 60° E. From this latter point we find more regular southeast dips (S. 50°–85° E.) to beyond Jasper, Pickens County. As we approach the crossing of Long Swamp, at Tate's, about five miles south of Jasper, we see by the roadside a cliff of white and gray granular marble, partly quite pure, partly quartzose, dipping 32°, S. 62° E. Considerable quantities of the purer beds have been quarried near here, and at other points along the Swamp to above Jasper. From quarries now worked about a mile east of the town, I have seen specimens as pure and fine-grained as the best statuary marble, but am not informed as to the amount of such rock. This is probably the direct southern continuation of one of the Knox marble outcrops, which pass south of Murphy. The Knox shale appears to have been locally wanting, since heavy-bedded gneisses, which we must refer to the underlying sandstones, show considerable thickness along the western border of the Swamp; but fine-grained mica slates occur near this horizon, from two to four miles north of Jasper. Hence to Duckstown, we follow nearly the general line of strike, and pass over pretty continuous lines of gneisses and schists, with variable southeasterly and northeasterly dips. The marble outcrops are known, at short intervals, along to the right of the road and at gradually increasing distances from it. At Tolono Creek we cross what may be the continuation of the Valley River belt. The copper is on the left of the road, but has not been exploited, I believe, at any point south of the Mobile Mine, in Fannin County, five miles from Ducktown, where there were extensive works before the war, which are now in ruins.

To the southwestward, the copper-bearing strata appear, in Harraldson, Paulding and Carroll Counties, with nearly the regular northeast strike, but apparently broken up by faults, so as to show three approximately parallel lines of outcrop, without intervening folds. These have not been traced so closely through the intervening country that I can feel certain which, if either, is the exact equivalent of the Ducktown line. The more northern line passes through the Waldrope Mine, in lot 932 of the third district of Harraldston County, where a good ore-body, from eight to fourteen feet thick, has been exposed in a shaft about sixty feet deep. The dip is here about 45°, S. 55° E. Near the Gamble shaft, on lot 851, a heavy bed of compact red hematite runs parallel with the copper, and but a few rods from it. The second belt passes close to Draketown, Paulding County, where the McClarty shaft, 105 feet deep, showed from twelve to eighteen feet of not very rich ore. The dip is here about 80°, S. 47° E. The third belt passes about a mile north of Villa Rica, Carroll County, where several small shafts have been sunk, along the outcrop, and have shown, according to reports, from three to thirteen feet of ore, partly poor, partly rich. Near the eastern termination of the recognized outcrop of "copper gossan," large numbers of polarized fragments of magnetite are scattered over the surface; and these increase in size and number as we pass eastward; but the bed has not been traced far beyond the point where signs of copper disappear. This deposit is an interesting one as a continuation in oxyds of a belt of sulphides farther west. The iron looks rather like a titaniferous ore, but has not been analyzed. While these three belts show but slight lithological variations, the strata being mainly hydromica and talcose schists, with occasional belts of hornblendic gneiss, and are therefore probably referable to one series broken by two faults, they *may* represent two or even three horizons.

At Villa Rica, we reach a synclinal fold; and the copper-gossan appears on its southeastern side, with northwestern dip, a short distance to the southeast in Douglass County. This is probably the belt on which a little work has recently been done at several points near Carrollton, while, still farther west, Wood's Mine, in the edge of Alabama, has, for about a year, been yielding large amounts of rich ore. Along the axis of this synclinal, for a width of between one and two miles, we find a belt of hydromica schists, with several quartz bands, both thick and thin, carrying considerable gold. This has been mined at several points; and several millions are known to have been coined from this immediate neighborhood, mostly washed from the stream rubbish and decayed outcrops, but partly obtained by stamping the vein-stuff. Gold-bearing strata have been recognized near each of the two more northern belts of copper; but little work has been done upon them. The gold-bearing schists of Bonner's Mine, eight miles southeast of Carrollton, have more of the appearance of the higher strata of the upper Chattahoochee waters, and probably are their continuation, though the marble belt, which is the only horizon thus far certainly recognizable, has not yet been reported from this region, nor, indeed, from any point west of Atlanta. Dr. Stephenson reports that it passes about ten miles north of that city.

The evidence is certainly very strong that *all the strata of the region thus reconnoitred are of Silurian age.* The only exception thus far suggested is that of an area east of Marietta, Cobb County, Ga., where Dr. Little reports the surface covered with masses of partly decomposed massive granite, over an area two or three miles in diameter. This *may* be Archæan; and there may be other similar islands within the area indicated but I have not yet seen or heard of any. Tuomey's reports upon Alabama indicate that the metamorphic strata of that State are of the same character and age as their continuations in Georgia.

To the northeastward of the region visited, we have as yet nothing definite. Emmons says, however (l. c. p. 31), that "the prevailing dip of the rocks of the Blue Ridge is southeast. When, however, we have passed from the pyrocrystalline rocks to the Taconic system, the dips are changed, first to southeast and finally to northwest, which is the prevailing dip." In view of the facts observed upon the same belts in Georgia, and herein recorded, it seems fair to suggest that, in North Carolina also, the Blue Ridge is probably simply a synclinal. with local irregularities of dip along the axis; and that the "pyrocrystalline rocks," as well as the "Taconic," are *probably all Silurian*, instead of being "Azoic" or "Laurentian."

The question naturally presents itself: *When occurred the uplift and metamorphism of all this region?* We have as yet no *direct* evidence—as that of newer horizontal beds lying unconformably upon the edges of these strata—but the indirect evidence is strong. Aside from the beds recognized as genuine Archæan, none give signs of any disturbance prior to the date of the general upheaval which affected the entire series, to the top of the Coal-measures. Both the disturbances and the metamorphism, from the Cumberland Mountains to the eastern range of the Blue Ridge, at least in this lower portion of their extent, appear to belong solely to one system and time. Further, so far southeast as the foot of Chilhowee Mountain, we find Subcarboniferous strata, in regular position, involved in the disturbance; and there seems to me to be every reason for believing that the Coal-measures also once covered the entire region. The entire disturbance must then be considered *post-Carboniferous*, and is doubtless referable to the close of the Paleozoic.

Although the Smoky Mountains are as high as the Blue Ridge proper, and yet give passage to streams from the latter range, it is not necessary to infer the earlier elevation of the Blue Ridge, as Willcox has recently done (Proc. Philad. Acad., 1874, p. 165), for the more thorough metamorphism of the eastern range is sufficient to account for its greater resistance to erosion during the earlier ages. On this subject of erosion, I hope to speak further, at another time.

I must here express my acknowledgments for local information furnished me by Prof. E. B. Olmsted, of Murphy, Mr. J. Mack Whittaker, of Valleytown, Dr. Josiah Curtis, now of Washington, D. C., Messrs. J. R. Dean and R. R. Asbury, of White County, Ga., Dr. M. F. Stephenson, of Gainesville, R. J. Gaines, of Carrollton, Dr. George Little, State Geologist of Georgia, and Mr. W. L. Nicholson, Topographer of the Post Office Department, Washington, D. C.

Knoxville, Tenn., March 27th, 1875.

Art. XLI.—*Brief Contributions from the Physical Laboratory of Harvard College.* No. 15.—*On the construction of Gaugain's Galvanometer*; by John Trowbridge.

In this instrument the law of the proportionality of the strengths of the electric currents, circulating through the coil of the galvanometer, to the tangents of the deviations of the needle, is exact when the distance of the center of the needle from the center of the coil is equal to one-half the radius of the coil. The theoretical demonstration by Bravais of the truth of this law of the galvanometer leads to the expression

$$\text{Eq. (1.)} \quad i=\frac{T\rho^3}{2\pi R^2}\left[1-\frac{3l^2}{2\rho^2}+\frac{15l^2D^2}{\rho^4}+\frac{15l^2}{4\rho^4}(R^2-4D^2)\cos\delta^2\right]^{-1}\tan\delta$$

in which i=strength of current.
T=horizontal intensity of the earth's magnetism.
R=radius of coil or circular wire.
l=length of needle.
D=distance from center of needle to center of coil.
$\rho^2=l^2+D^2+R^2$.
δ=deviation of needle.

We see from eq. (1) that when $(R^2-4D^2)=0$, or $D=\frac{R}{2}$, that the terms in the parenthesis are of the order $\left(\frac{l}{\rho}\right)^4$ and when $\frac{l}{\rho}$ is small these can be neglected.

In constructing the galvanometer the serious question arises: how much will a small error in the measurement of D and of R affect the law? and which will have the greater effect, an error in the measurement of R or in that of D?

Let us suppose at first that R and D have been measured correctly. We shall then have

$$i=\frac{T}{2\pi R^2}(l^2+R^2+D^2)^3 \tan\delta, \text{ in which } D=\frac{R}{2}.$$

Suppose now that, while R remains constant D differs from its true value by αD, in which α is a very small quantity. We shall then have, neglecting small quantities in Eq. 1:

$$i=\frac{T}{2\pi R^2}(l^2+R^2+(D+\alpha D)^2)^3\tan(\delta-m)$$

in denoting the decrement of the angle of the needle. The difference in value of these two expressions for the strength of a current is

$$i-i_1=\frac{T}{2\pi R^2}\left((l^2+R^2+D^2)^3\tan\delta-(l^2+R^2+(D+\alpha D)^2)^3\tan(\delta-m.\right)$$

Suppose now that D remains constant or R varies from its true value by the small quantity βD, in order to produce the same decrement as in the angle; we shall have

$$i_2=\frac{T\,(l^2+D^2+(R+\beta R)^2)^3\ \tan(\delta-m.)}{2\pi\,(R+\beta R)^2}$$

$$i-i_2=\frac{T}{2\,\pi}\left[\frac{(l^2+R^2+D^2)^2\tan\delta}{R^2}-\frac{(l^2+D^2+(R+\beta R)^2)^3\tan(\delta-m)}{(R+\beta R)^2}\right.$$

The difference in the values of the strengths of the currents are indicated by the final terms in the values of $i-i_1$ and $i-i_2$ which are

$$\frac{(l^2+R^2+D^2\,(1+\alpha)^2)^3\tan(\delta-m)}{R^2}$$

$$\text{and}\ \frac{(l^2+D^2+R^2\,(1+\beta)^2)^3\tan(\delta-m)}{R^2(1+\beta)^2}$$

Since $D=\frac{R}{2}$ we have by developing and neglecting the squares of α and β,

$$\frac{\left(l^2+\frac{5}{4}R^2+\frac{R^2\alpha}{2}\right)^3}{R^2}\quad\text{and}\quad\frac{\left(l^2+\frac{5}{4}R^2+2\beta R^2\right)^3}{R^2(1+\beta)^2}$$

Neglecting α and β when they are additive to a large whole number, the differences in the strengths of the currents will be expressed by the terms $\frac{\alpha R^2}{2R_{\frac{2}{3}}}$ and by $\frac{2\beta R^2}{R_{\frac{2}{3}}}$, α is smaller from the nature of the case than β. Hence

$$\frac{\alpha R^{\frac{4}{3}}}{2}<2\beta R^{\overline{\frac{4}{3}}}.$$

Therefore an error in the measurement of R will not affect the law of the galvanometer so much as one in the measurement of D.

In order to obtain an idea of the magnitude of errors resulting from changes in the value of D, I arranged a galvanometer so that the coil, consisting in this case of a circle of thick brass wire, the resistance of which could be neglected, could be moved away from its true position, after the manner of Wiedeman's galvanometer. The value of R was 12 cm. and that of D 6 cm.

Value of D+X	Value of δ.
6	20°
7	19
8	18
8·5	17·5
9	16·5

A variation of 1 cm. produced a change in the deviation of the magnetic needle of 1°. (The galvanometer needle was 25 mm. long and was suspended by a single fiber of silk.)

The apparatus was also so arranged that the radius of the circle of brass could be changed, while D remained constant. It was found that the changes in the indications of the needle were inappreciable until the increase of the radius was 5 mm. when the increase amounted to 1 cm.,—a variation of half a degree was produced in the indication of the needle. The increase in resistance of the circuit was inappreciable. An error, therefore, of five millimeters in the determination of the radius of the coil was inappreciable, while the same error in the measurement of the distance of the needle from the center of the coil resulted in, comparatively, a larger error. In approaching the subject from another point of view, it can be proved* that the expression for the attraction of a single circular current on a point situated on the line passing through the center of the coil and perpendicular to its plane is

$$X=2\pi k\left\{\frac{a^2}{r^3}-\frac{3a^4}{2r^5}+\frac{15}{8}\frac{a^6}{r^7}-\ldots\ldots\right\}$$

in which *k* is a constant depending on the strength of the current, *a* is distance of point from center of coil, and *r* is the radius of the coil. By inspection it will be perceived that a small variation in the value of *a* will affect the value of X more than a similar slight variation in the value of *r*.

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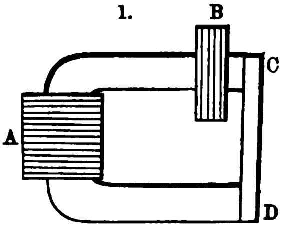
* James Stuart, M. A., Phil. Mag., 46, 1873, p. 231.

No. 16.—*Upon the induced currents produced by the application of armatures to Horseshoe magnets and a new form of Magneto-electric Engine*; by W. R. MORSE.

The apparatus consisted of cylindrical horseshoe electro-magnets, the wires of which were wound about the iron cores at the bend of the iron, so as to form practically straight electro-magnets with cores horseshoe in form. Upon one of the limbs of the horseshoe core a coil of fine wire was slipped so that the plane of its coils was at right angles to those of the electro-magnet. In fig. 1, A represents the coil of the electro-magnet; B that of the induction coil. Upon exciting the electro-magnet induction currents arose in the coil of fine wire B both at making and breaking the circuit. These currents were measured by a reflecting galvanometer placed in the circuit of the coil B; and were compared with those obtained from the same electro-magnet by placing a straight armature C D upon its poles and then exciting the electro-magnet. The following table shows the results obtained. Only the currents resulting from making the circuit are recorded. those proceeding from the breaking of the circuit being the same in value. The readings are expressed in the divisions of the scale of the reflecting galvanometer.

1.



Without armature.	With armature.	After removal of the armature. 1st deflection.	After removal of the armature. 2d deflection.
170	210	210	170
170	209	209	170
175	209	209	170
170	210	210	175

These results show that a marked increase (in these experiments, nearly twenty-five per cent) in the strength of the induction currents results from the application of an armature to the poles of the electro-magnet. The third and fourth columns of the table show that after the removal of the armature, the first induced current which results from again making the current in the electro-magnet shows the same increased effect; but that the following current resulting from breaking the circuit of the electro-magnet falls to its normal amount. This result is noteworthy, for it shows a certain molecular change in the iron which results from the application of the armature.

Although we can thus increase the strength of the induction currents produced in coils slipped upon the limbs of an electro-magnet, we diminish the lifting power of these individual limits by the employment of an armature, as the following results show:

Weight lifted without armature.	Weight lifted with armature on.
249	160
300	180

In the preceding experiments the straight iron bars forming the armatures were carefully deprived of whatever residual magnetism they might possess.

Experiments were next tried upon the effect of horseshoe electro-magnets used as armatures to electro-magnets of the same character as those employed in the preceding experiments. When two north or two south poles were opposed to each other, and the magnetic circuit, so to speak, of the two horseshoe-shaped cores was closed, very feeble indications were shown by the galvanometer. When, however, a north and a south pole were opposed, and the magnetic circuit closed, the strength of the currents obtained both on the application and the removal of the armature were very marked, as the following results show:

At contact of N. and S. poles.	On removal of N. and S. poles.
+360	−359
+362	−360
+361	−361

When the horseshoe magnet forming the armature was not used, and one of the limbs of the stationary electro-magnet was quickly slipped in and out of the induction coil, induction currents were obtained, the values of which are shown below:

Placed in.	Withdrawn.
+40	−40
+40	−40
+40	−40

In these experiments the stationary electro-magnet and the electro-magnetic armature were of the same size and the same magnetic strength.

Experiments were next made upon the influence of the mass of the iron forming the armature. This was found not to have so much influence as the residual magnetism of the iron. The results were very contradictory, as the following table shows:

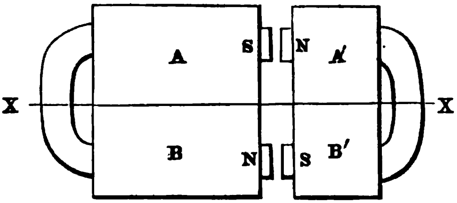
Weight of armature.	Deflection produced.
364 grams.	280
341	330
222	290
137	280
132	290
67	310

We are led to believe that the mass of the iron does not affect the results when it exceeds that of the stationary electro-magnet.

The induction currents resulting even from the employment of straight soft iron armatures which had been carefully deprived of residual magnetism are thus seen to be more than four times as strong as those obtained by merely slipping the induction coil on and off the limits of the electro-magnet; which is, practically, the method adopted in many later forms of the magneto-electric engine, particularly in that of the Gramm machine, in which different portions of a ring-shaped electro-magnet revolve toward and away from the poles of a horseshoe magnet. When electro-magnetic armatures are used the effects far surpass those obtained by non-magnetic soft iron straight armatures, as the preceding results show.

Professor Trowbridge suggests a magneto-electric engine of the following construction. The horseshoe armature is made to revolve about the line XX as an axis. By the preceding experiments it has been found that when a north and a south pole are opposed the induction currents flowing through B and A′ are in the same direction and those through B′ and A are also in one direction. By a suitable commutator the currents circulating through the coils on the stationary magnet can be sent through those on the armature, and vice versa. The residual magnetism in soft iron is sufficient to start the induced currents. Instead of one stationary electro-magnet it would probably be better to employ a number arranged about the axis XX. With projecting pieces of soft iron arranged upon the poles of the stationary magnets, the size of the horseshoe armature could be regulated to suit the varying conditions of speed.

2.



Experiments are now being made on this form of engine.

Art. XLII.—*Remarks on the Observations of the late Transit of Venus*; by Simon Newcomb.

Reports more or less detailed having been received from all the parties engaged in observing the late Transit of Venus, we are in a position to form at least a rough estimate of the measure of success which has been attained. This estimate can, however, at present, only be preliminary, because the accuracy of the individual observations and methods cannot be assured except by a critical examination, discussion, and comparison of the observations, a work which will necessarily require much time for its accomplishment.

Among the causes which affected the prosecution of the observations, the weather occupies the most prominent place. The most striking feature of the weather at the late transit, so far as concerns the American stations, is the impartiality with which clouds have been distributed among the several stations. Notwithstanding that the utmost pains were taken to choose positions where the weather record for November and December was good, the prosecution of the observations was more or less interfered with by clouds at every one of the eight stations. On the other hand, there was not a station at which the operations totally failed. At the most unfortunate one, Chatham Island, six or eight photographs of the sun with Venus on its disc were secured. At two stations, Hobarttown and Chatham Island, no contacts at all were observed. The number of good photographs of the sun with Venus completely entered upon its disc is approximately as follows:

Northern Stations.

Wladivostok, Siberia,	13
Pekin, China,	90
Nagasaki, Japan,	60
Total,	163

Southern Stations.

Kerguelen Island,	26
Hobarttown, Tasmania,	39
Campbelltown, Tasmania,	55
Queenstown, New Zealand,	59
Chatham Island,	8
Total,	187

While the small number of these photographs is extremely disappointing, their distribution among so many stations is a great advantage, through the facilities thus afforded for detecting and eliminating any constant error peculiar to any particular station.

Besides the photographs here enumerated, a number of photographs of the indentation made by the planet while entering upon the limb of the sun were obtained at some stations.

Contact observations were obtained at the American stations as follows:

First External Contact.

Northern stations: Wladivostok, Pekin, Nagasaki. ?
Southern stations: Kerguelen, Queenstown.

The difference of time between the mean of the northern stations and Kerguelen, due to parallax, is thirteen minutes.

Compared with Queenstown, the difference is only four minutes. Allowing a probable error of eight seconds in the comparison of these observations, they would alone give a value of the solar parallax with the probable error 0″·090.

First Internal Contact.

This was observed at the three northern stations, but only at Queenstown in the south. The American observations alone will therefore give us no result entitled to weight. The southern region for the observation of this contact is that extending from Kerguelen Land to Mauritius and Rodriguez, and here very little success was met with by the parties of any nation in the observation in question. It may, however, have been successfully observed by the English at Rodriguez, and by the Germans at Kerguelen, from neither of whom the writer has seen definite statements on this point. On the other hand, it was successfully observed by the English parties at the Sandwich Islands, whose observations will be available for combination with those of Dr. Peters at Queenstown, and of other observers in Australia, even if no observations were made at Kerguelen and Rodriguez.

Second Internal Contact.

This was also observed at all three northern American stations, but at only a single southern one, namely, Campbelltown. The mean difference of time due to parallax is fifteen minutes. The favorable southern region for the observation of this contact was Australia, and the islands east and south of it, and the weather was exceptionally unfavorable in most of this region. Some success was obtained at and near Melbourne, and the German expedition in the Auckland Islands was also successful, but the English and French expeditions nearly all failed in this important observation.

Second External Contact.

This was observed at only a single one of the eight American stations, Pekin. It is, however, the least fitted of the four contacts to be accurately observed, and the failure to secure it is therefore the less to be regretted.

It is my opinion that the optical observations of contacts made by the observers of all nations will, by their combination, give a value of the solar parallax of which the probable error will lie between 0″·02 and 0″·03. I also think that the American photographs alone will give a result at least as accurate as this, and probably more so. A large remaining mass of material will be the heliometer measures made by the Germans and Russians as well as Lord Lindsay, various optical measurements made near the moments of internal contacts, and photographs in which Venus was partly on the sun. The combination of all these may be expected to give a result of equal weight with either of those already mentioned. I think, therefore, that we may look forward with considerable certainty to seeing the probable error from the combination of all the observations less than 0″·02, and, perhaps, not much more than 0″·01. At the same time, it is not to be disguised that there is a possibility of unforeseen perturbing causes being brought to light by a comparison of all the observations, which will upset all our *a priori* estimates of probable error.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the Occurrence of Diphenyl in Coal Tar.*—Fittig communicates the results of an investigation made in the Tübingen laboratory by BUCHNER, upon a liquid hydrocarbon oil obtained as a by-product in the manufacture of anthracene. The portions of this especially examined were the less thoroughly known fractions boiling between 170° and 200°—i. e., between trimethylbenzol and naphthalin—and between 220° and 270°—or between naphthalin and acenaphtene. The first fraction by repeated distillation was almost entirely resolved into products boiling below 170° and others boiling above 200°; whence the conclusion that coal tar contains no considerable quantity of a hydrocarbon boiling between these limits. The second fraction gave a more satisfactory result. After several fractionings of five degrees, the portions distilling over between 242° and 263° solidified on cooling. By cooling these fractions considerably and subjecting them to pressure, a white crystalline mass was obtained, which remained solid at ordinary temperatures, and distilled completely between 245° and 250°. After two recrystallizations from alcohol, the hydrocarbon melted at 70°–71°, and exhibited all the properties of diphenyl.—*Ber. Berl. Chem. Ges.*, viii, 22, January, 1875.

G. F. B.

2. *Explosiveness of methyl nitrate.*—Methyl nitrate was discovered by Dumas and Peligot in 1835. In 1862, Carey Lea* showed that it could advantageously replace methyl iodide in most chemical reactions, especially in the preparation of the methyl bases. In 1864, as a consequence of this observation, Hugo Löwenstein applied it in the manufacture of the violets derived from rosaniline by substitution; and it has since come so extensively into use as almost to displace methyl iodide from the market. The most serious objection to its use is its explosiveness; a fact first pointed out by its discoverers, and since, unfortunately, realized on a large scale in the case of the death of the talented young chemist, E. T.

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* This Journal, II, xxxiii, 86, 227, 1862.

Chapman, and in the more recent disastrous explosions both in Germany and France. GIRARD, employing it largely for the production of colors, has made some experiments with it, with a view to obviate this difficulty. He finds, for example, that methyl nitrate, like nitro-glycerin, detonates by a blow; a drop on blotting paper producing, when placed on an anvil and struck with a hammer, an explosion quite as violent as nitro-glycerin. When mixed with pulverulent or porous substances, such as precipitated silica or infusorial earth, it yields dynamites fully as effective as those made with nitro-glycerin. By admixture with other liquids, however, such as methyl, ethyl, or amyl alcohol, acetone, benzol or toluol, Girard finds that it is no longer explosive. One part of methyl nitrate diluted with two or three parts of either of the liquids above named, does not explode either on heating its vapor, or by a blow. In such solutions, therefore, it is best kept for use. —*Bull. Soc. Ch.*, II, xxiii, 63, Jan., 1875. G. F. B.

3. *On Pure Dextrin, prepared from Malt.*—BONDONNEAU describes the properties of dextrin prepared by the action of diastase on starch. A kilogram of dry starch, diffused in two liters of water, was treated in the cold with an extract of 250 grams of bruised malt in 500 grams of water; the whole being heated on the water bath to 75° until the starch had disappeared. The liquid was then carried to boiling to destroy the diastase, filtered through animal charcoal, and concentrated to 32°–33° Baumé. To free the dextrin in this solution from the glucose (dextrose) present, it was first reprecipitated five or six times with alcohol, then treated with a copper test made with cupric chloride and sodium hydrate; this latter treatment destroying the dextrose. As thus obtained, the dextrin showed no coloration with iodine, gave only feeble indications with the copper test (equivalent to 1·85 per cent dextrose) reduced gold chloride and ammonio-silver nitrate, gave an abundant precipitate with a solution of barium hydrate and with ammoniacal lead acetate, but none with lead sub-acetate. The dextrose present, the author believes to be produced by small quantities of non-coagulable albuminoids present, acting as ferments. By care and rapidity in operating, dextrin may be obtained devoid of reducing power. The rotatory power of the dextrin thus obtained is $\alpha j = 176°$ to the right; that produced by torrefaction being $\alpha j = 183°$.—*Bull. Soc. Ch.*, II, xxiii, 98, Feb., 1875. G. F. B.

4. *On the Identity of Liebermann's Cœrulignone with Reichenbach's Cedriret.*—HOFMANN has continued his investigations upon the less volatile constituents of beech-wood tar. In a previous paper he had shown that from these could be isolated a liquid boiling at 270°, possessing a phenol-like character, and yielding, when treated with potassium dichromate, the beautifully crystallized substance described by Liebermann under the name of coerulignone, and also a magnificently crystallized compound in long yellow needles, dissolving in sulphuric acid with a carmine-red color. He has now been able by repeated fractioning and recrystalliza-

tion of the sodium salt, to isolate an oil boiling at 285°, which on oxidation is entirely converted into the yellow body, and which has the formula $C_{11}H_{16}O_3$, its bromine derivative being $C_{11}H_{14}Br_2O_3$. The yellow compound is a quinone-like body, having the composition $C_8H_8O_4$ and giving on reduction a hydro-compound $C_8H_{10}O_4$, crystallizing in white needles. Bromine gives a substitution derivative of the composition $C_8H_6Br_2O_4$. In regard to the body first named, to which Liebermann has given the name coerulignone, Hofmann concludes that it is identical with the substance long ago described by Reichenbach* under the name cedriret. The description originally given of it is quoted, and is full and complete. That a body possessed of properties so striking should have remained forgotten for thirty years, is exceedingly remarkable. In a note immediately following, Liebermann himself acknowledges the identity; though he was at first inclined to doubt it, owing to Reichenbach's having described it as a precipitate of fine *red* needles, and having obtained it from wood tar; whereas, his product was obtained from wood-vinegar and crystallized in dark steel-blue or violet needles. The production of this body by Hofmann from an oil derived from wood tar and evidently identical with Reichenbach's product, leaves no doubt of the identity.—*Ber. Berl. Chem. Ges.*, viii, 66, 69, January, 1875.

G. F. B.

5. *On a new Coloring matter, Eosin.*—A new coloring substance, introduced into commerce during the last summer under the name of Eosin, has been examined by Hofmann. It has a beautiful red color, recalling that of rosaniline, but inclining more to garnet-red. The name eosin, by which it is known to the trade, unlike most similar names, is rational, being derived from Ἕως, the red of the morning dawn. It appears as a brownish-red powder, showing here and there facets with metallic luster. It is soluble in water and in alcohol, and its solutions are strongly fluorescent. Upon examination, it was found to contain no nitrogen, and to evolve hydrogen bromide on heating, leaving a carbonaceous residue containing potassium bromide. Distilled with zinc dust it afforded benzol. Its aqueous solution treated with an acid, threw down a brick-red powder, which, recrystallized from glacial acetic acid, appeared as yellow prisms having the composition $C_{20}H_8Br_4O_5$; a formula confirmed by the analysis of the barium salt, $C_{20}H_6Br_4BaO_5$. From these data Hofmann concluded that eosin must belong to that most remarkable group of bodies with which Baeyer has enriched chemistry, being a tetrabrominated fluorescin; a derivative of a body which he obtained by fusing resorcin with phthalic oxide. To test this hypothesis, Hofmann treated the potassium compound with potassium hydrate, heated, decomposed by an acid, treated the filtrate with ether, and from the etherial solution obtained dibrom-resorcin in beautiful white needles. Eosin is therefore the phthalëin of dibrom-resorcin. Its synthesis was very easily accomplished by acting on an acetic solution of

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* Berzelius Jahresbericht, xv, 408.

fluorescin—prepared from resorcin and phthalic acid by Baeyer's method—by bromine, and then diluting with water. The red precipitate thus obtained dissolved in alkaline solutions with the characteristic color of eosin, and when crystallized from glacial acetic acid, afforded the same product as that met with in commerce.

In a paper published subsequently, BAEYER states that eosin was first produced commercially at the Baden Aniline Works by Caro, and by him introduced to the trade under that name. He suggests the following experiment to show the relations of eosin: A portion of the coloring matter is agitated with water and sodium amalgam at a gentle heat. The solution is soon decolorized, the bromine being removed and colorless fluorescin produced. If now, water be added and a few drops of potassium permanganate solution, the fluorescin changes to fluorescëin and the liquid becomes bright green and almost opake, in reflected light.—*Ber. Berl. Chem. Ges.*, viii, 62, 146, Jan., Feb., 1875. G. F. B.

6. *On the Structural Formula of Hydroxylamine.*—The body NH_3O, discovered originally by LOSSEN, and to which he gave the name hydroxylamine, possesses great theoretic interest. It may be regarded as ammonia in which hydroxyl replaces an atom of hydrogen; as a direct compound of the radicals amidogen and hydroxyl; or as a compound in which an atom of nitrogen has two of its units of attraction saturated by two hydrogen atoms while the third is united to one of the bonds of an oxygen atom, the other being saturated by hydrogen; thus $H — \overset{\overset{H}{|}}{N} — O — H$. It would seem as if there could be little question upon the rational constitution of so simple a body. The above rational formula rests, first, upon the method of its preparation and second upon its ammonia-like properties. It is prepared by the reduction of nitric acid, precisely as nitro-methane is reduced to methylamine or nitrobenzoic acid to amidobenzoic acid; the radical NO_2 in all these cases being changed to NH_2. Hence from $NO_2.OH$ comes very readily $NH_2.OH$. Again, the ammonia-like structure of hydroxylamine is shown by the fact that it unites with acids to form salts without the formation of water. The structure above given, however, involves the assumption that two of the three hydrogen atoms, being similarly situated and combined, should exhibit the same behavior; while the third, being differently united, should behave differently. LOSSEN has undertaken to test this assumption experimentally. By an easy replacement of the hydrogen atoms in hydroxylamine by benzoyl, benzhydroxamic and di-benzhydroxamic acids, and tribenzhydroxylamine, are successively produced. Entirely analogous compounds are produced by anisyl. Calling the hydrogen atoms one, two, three, in this order $N\overset{1}{H}\overset{2}{H}O\overset{3}{H}$, we have, when both radicals replace hydrogen, benzanishydroxamic acid $N(C_7\overset{1}{H}_5O)(C_8\overset{2}{H}_7O_2)O\overset{3}{H}$ and anisbenz-

hydroxamic acid $N(C_8\overset{1}{H}_7O_2)(C_7\overset{2}{H}_5O)O\overset{3}{H}$, with two replacements; and dibenzanishydroxylamine $N(C_7\overset{1}{H}_5O)(C_7\overset{2}{H}_5O)O(C_8\overset{3}{H}_7O_2)$, benzanisbenzhydroxylamine $N(C_7\overset{1}{H}_5C)(C_8\overset{2}{H}_7O_2)O(C_7\overset{3}{H}_5O)$, and anisdibenzhydroxylamine $N(C_8\overset{1}{H}_7O_2)(C_7\overset{2}{H}_5O)O(C_7\overset{3}{H}_5O)$, when three atoms are thus exchanged. Now if hydrogen atoms 1 and 2 in hydroxylamine are precise equivalents, the first two of the above named five compounds are identical; if not, they are only isomeric. If hydrogen atoms 2 and 3 are equivalents, then the third and fourth of the above compounds are identical; but they are only isomers, if they are not. Lastly, if hydrogen atoms 1 and 3 are equivalent to each other, the third and fifth of these compounds must be the same; but if these atoms are not precisely identical in function, the compounds must be isomers. The author has proved the most important point, that benzanishydroxamic and anisbenzhydroxamic acids are not identical, but are isomeric. The former fuses at 133° to 134°, the latter at 147° to 148°, and their chemical reactions as given are quite different. It seems clear therefore that hydrogen atoms 1 and 2 are not the precise equivalents of each other, and that there are at least two kinds of such atoms in hydroxylamine. Lossen assigns therefore to dibenzhydroxamic acid the formula $C_7H_5O-\overset{\overset{H}{|}}{N}-O-C_7H_5O$ and to dianishydroxamic acid that of $C_8H_7O_2-\overset{\overset{H}{|}}{N}-O-C_8H_7O_2$. The author promises further researches to fix the position of the replacing radical in the monohydroxamic acids.—*Liebig's Ann.*, clxxv, 271, Jan., 1875. G. F B.

7. *Influence of Pressure on Combustion.*—M. L. CAILLETET has studied the effect of a pressure of 30 to 35 atmospheres on the luminous, calorific and chemical rays, emitted by a burning body. The air was compressed by pumps in which the pistons were fixed and the cylinders movable, a layer of water or glycerine at the same time cooling the gases so as to protect the packing from the heat, and preventing leakage. The reservoir consisted of a hollow cylinder with four apertures; the first admitted the gas, the second allowed it to escape, the third admitted the manometer-tube, and the fourth was closed by a thick glass plate to allow of observation of the interior. The latter had a diameter of 10 cms. and a capacity of about 4 liters. Placing a lamp in this space, the brightness increased with the pressure of the air. The base of the flame, which under the ordinary pressure is blue and transparent, became white and very bright; but soon the appearance changed and thick clouds of smoke circulated through the apparatus and escaped by the stopcock closing the outlet. The flame seen through this smoke is reddish and the wick is found to be charred and much soot deposited, doubtless owing to the dissociation of

the gases containing carbon. The heat increases but not enough to burn an iron wire heated to redness in the common flame.

The brightness of the flame of burning phosphorus does not seem to increase, but that of sulphur is brighter and yellowish-red on the borders.

Potassium burns with a very bright violet flame. The combustion in a charcoal furnace did not seem more rapid under 25 atmospheres than at the usual pressure. An alcohol lamp with a wick formed of a single thread, and giving in the open air a scarcely visible flame, rapidly increased in brightness with the pressure. At 20 atmospheres, the flame is white and bright as that of an oil-lamp. Its spectrum is continuous and more extended than at ordinary pressures; the ray *D* is alone visible, apparently enlarged. Bisulphide of carbon also burns more brightly than usual. Neither it nor sulphur gives a perceptible quantity of sulphuric acid.

The chemical rays, as well as those of light and heat, seem to have an increased activity. A number of flattened tubes containing phosphorescent substances were exposed to the rays of these flames and shone with a much brighter light with the increased pressure.—*Comptes Rendus*, lxxx, 487. E. C. P.

8. *Magnetism.*—M. Bouty has studied the laws of magnetic action by means of a new and simple device for measuring magnetic moments. A small magnet attached to a mirror is fastened to the lower end of a rod of sealing wax and suspended by a filament of silk. In the upper part of the rod a small glass tube is inserted at right angles to the magnet. If now a magnet is inserted in the tube, the system will assume a position of equilibrium under the action of the two magnets, from which their relative magnetic moment is readily determined. The angular deflection is measured by means of a telescope and scale. The lower magnet has a length of 6 cms. and a diameter of ·2 mms; the tube a length of 2 cms. and a diameter of ·2 mms. By this arrangement the moment of magnets could be determined whose length was but 2 mms. and their diameter ·2 mms. With this apparatus the magnetization of a bar was measured when it was inserted a number of times into a helix, and was found to be well represented by the empirical formula $Y = A - \frac{B}{X}$, in which Y is the moment and X the number of insertions.

The effect of breaking a needle was next studied. The needle was heated to redness and plunged instantly into water, and the middle portion, about 150 mms. in length, alone employed. Green has deduced a formula for the magnetic moment of saturated bars of various lengths. But the primitive needle possesses in every point a higher degree of magnetization than corresponds to the saturation of the fragment which belongs to it after breaking. Two instruments are employed, one of great delicacy, for the short fragments, a second less sensitive for the longer pieces. The agreement of observation with theory is all that could be desired until the length becomes small compared with the diameter. The law for changes in the diameter and the effect of a fracture parallel to the axis were also studied. Finally, on the ground of the peculiar facts examined in this memoir, as well as the whole of the known facts, he concludes that the present theories of magnetism are insufficient to explain the peculiarities of the temporary magnetization of steel, and suggests that in regard to the magnetic properties of its elements this substance must be considered a heterogeneous mass.—*Phil. Mag.*, xlix, 81, 186. E. C. P.

9. *Frictional Electricity.*—M. Rossetti has made a careful study of the current developed by a Holtz machine and shown that it follows the laws of a galvanic battery. The machine was turned by a weight, and the work done measured when charged and discharged. The current was determined by a galvanometer and the resistance by four long fine tubes filled with distilled water. The current is nearly proportional to the speed, but increases a little more rapidly than the latter. The ratio of the two alters with the amount of moisture in the air, so that to produce a given current the machine must be driven faster in wet weather, although the work required is less than when the air is dry. The economy, therefore, is greater on wet than on dry days. A Holtz machine resembles a galvanic battery, having an electromotive force and interior resistance which are constant as long as the velocity and moisture are constant. The electromotive force is invariable for a given degree of moisture whatever the velocity, while with a given velocity it diminishes as the moisture increases. The interior resistance on the other hand is independent of the moisture and diminishes more rapidly than the velocity increases. In the actual instrument employed, when the moisture was ·69 the electromotive force equalled 41,000 volts, while with the moisture ·35 it equaled 57,000 volts. The resistance with a velocity of eight turns per second was 540,000,000 ohms, and with a velocity of two turns 2,680,000,000 ohms. The current follows the law of Ohm, consequently if very large exterior resistances are employed, the strength of the current gives the mechanical equivalent of heat. The mean of seventeen experiments gave as a result the number 428, which agrees very closely with the commonly received value.—*Ann. de Chim. et Phys.*, iv, 214. E. C. P.

10. *Chemical Sub-section in the American Association for the Advancement of Science.*—This sub-section of the American Association was organized at the Hartford meeting in August last. It is called the "Sub-section of Chemistry, Chemical Physics, Chemical Technology, Mineralogy and Metallurgy," and all persons interested in applied as well as in pure chemistry will be welcome at Detroit, where the next session is to be held, in August of this year. We make this statement very willingly at the request of Prof. F. W. Clarke, of Cincinnati University, who is the secretary of this section, and who is making active exertions to secure a large gathering of chemists on that occasion. Prof. S. W. Johnson, of Yale College, is chairman-elect of this section.

II. GEOLOGY AND NATURAL HISTORY.

1. *Supplement to the Article on Dr. Koch's evidence with regard to the cotemporaneity of Man and the Mastodon; by the Author.*—Since the article on pages 335 to 346 was printed, I have come across another of Dr. Koch's pamphlets. It is a "second edition" of the New York pamphlet of 1845. In the main it is the same with the earlier one of that year. The most important difference is in the first half of the title page, which reads as follows:

"Description of the HYDRARCHOS HARLANI (Koch). (The name SILLIMANII is changed to HARLANI, by the particular desire of Professor Silliman.) A gigantic FOSSIL REPTILE, lately discovered by the author, in the State of Alabama, March, 1845."

A *second* difference is in the appended matter of nearly 10 pages, which extends the pamphlet to 24 pages. This matter consists of (1) an extravagant article from "The New York Dissector;" (2) the article from the "New York Evangelist" about the Hydrarchos and Leviathan, alluded to on page 344, as occupying the inside pages of the pamphlet of 1853; and (3) a puff from the "New York Morning News."

A *third* novelty is a large wood-cut of the "*Hydrarchos Harlani*," covering the last page of the cover. The body of the pamphlet contains only some verbal changes.

These New York pamphlets of 1845 contain one significant discovery of Dr. Koch's, made during his "geological tour," which is worth citing. He says: When at Golconda, Illinois, "I discovered a large deposit of old red Sandstone or Devonian system, in which I found a great variety of non-described fossil fish of most wonderful forms, the spiral columns of many of them bearing a striking resemblance to a screw, so that they are called by the inhabitants of the country *petrified screws*."

The Doctor's "spiral columns" of "fossil fish" are the common Bryozoan corals of the genus *Archimedipora*, found there, and elsewhere, only in Subcarboniferous rocks.

2. *Cold of the Glacial and other Geological epochs.*—The idea that a high-latitude elevation making a partial barrier across the shallower part of the Atlantic Ocean from Scandinavia to Greenland, would produce a change of temperature in the North Atlantic, is brought out by Mr. Prestwitch in his Presidential Address before the Geological Society of London, in 1871, to account for changes in the life of the ocean, during the latter part and close of the Cretaceous period; and the principle is recognized by Dr. W. B. Carpenter in his elaborate paper on Ocean Currents in connection with his Researches on board the "Shearwater" in 1871, read before the Royal Society in June, 1872, and published in No. 138 of the Society's Proceedings, xx, 535–644. J. D. D.

3. *Geological Survey of Wisconsin.*—The present geological survey of Wisconsin was organized in the spring of 1873, with Dr. I. A. LAPHAM as chief geologist, and Professor R. D. IRVING,

of the State University, Professor T. C. Chamberlin of Beloit College, and Mr. Moses Strong, Mining Engineer, as assistant geolgists; and Professor W. W. Daniels, of the State University, as chemist. The appropriation was $13,000 annually for four years.

The name of the Chief Geologist was a guarantee that the work would be faithful and exact.

Two seasons have been occupied in office and field work. During the first season there were three parties in the field, and during the second. four, a special contract having been made for a survey, during the latter season, of the Menominee River Huronian region, with Major T. B. Brooks, of the Michigan Survey, the work being really but an extension across the State line of his previous labors in Michigan. By the close of the last season one-third of the State had been examined in detail, and very many interesting facts developed. Reports have been made sufficient to fill a large quarto volume, accompanied by hundreds of illustrations and over a hundred detailed maps. These maps embrace all the results possible to place upon them; it having been the aim of the corps to put as much of the work as possible in this permanent form. They are complete for all portions of the State examined, and include geological, topographical, agricultural and other maps, accompanied by large general sections, all drawn on a scale of two inches to the mile.

The entire lead region has been topograpically surveyed and mapped with contour lines at a distance of fifty feet vertical, as recommended by Whitney in his report on the same region. Careful determinations of dips were made at the same time, so that the exact position of the mining ground—confined to certain strata—is made known at each locality.

Northern Wisconsin, bordering on Lake Superior, has been examined in detail, and some interesting facts bearing on the ages of the Lake Superior sandstone and of the copper rocks ascertained. The main work in northern Wisconsin consisted in an investigation of the Huronian rocks and ores of the Penokie Iron Range of Ashland County. The range is thirty miles in length in Wisconsin and about a mile in width at base. Several streams break through it from the southward, affording magnificent sections of its rocks, and on its flanks the siliceous ores that form its mass everywhere outcrop in precipitous exposures. These outcrops have been measured and the ore sampled and analyzed. The report on this range alone would form a volume of considerable size, with some scores of illustrations.

Eastern Wisconsin has been nearly or entirely surveyed by Prof. Chamberlin's party. He has been able to divide the Nia ara limestone into several subordinate formations. He has also obtained many new and interesting facts bearing on the drift phenomena of a region most remarkable for its glaciated surfaces, giant kettles, boulder clays and moraine heaps. He has also prepared a series of soil and timber maps of this region, and has collected no less than 14,000 fossils, many of them new forms.

Central Wisconsin has been examined in detail by Professor Irving, where he has made observations on the Lower Silurian of the Four Lake country; the Archæan quartzyte range of Sauk and Columbia Counties; the extensive Potsdam sandstone region of the center of the State, with its remarkable castellated outliers of sandstone, and numerous Archæan islands; the boundary between the Archæan and Potsdam; the rocks of the main Archæan body along the valleys of the Wisconsin, Yellow, and Black Rivers; and on the drift phenomena of the entire region, which are especially interesting because the area is just on the edge of the driftless region of the western half of the State.

While the members of the corps were just now expecting authority from the legislature for the printing of these reports, they were greeted, we learn, by the announcement that the governor had appointed a new chief geologist, and one whose sole recommendation for the position was political services, no one having ever heard of him before as acquainted with geology or any other science. It appeared that Dr. Lapham had been appointed under the law subject to the confirmation of the senate at its next session. At that time a new administration had come in and Dr. Lapham's name was never sent in, though he continued to perform his duties, and to be recognized by the State officials. This fact was taken advantage of to oust him, the appointment being defended in the senate by parties from the northern portion of the State, who had got the idea that the reports on the mineral regions of that section were not as favorable as interested parties desired. The whole thing was a total surprise to Dr. Lapham and other members of the corps, so that there was no time for any opposition on their part, or that of other friends of the survey and of good honest scientific work.

A great wrong has been done to Dr. Lapham, and a greater to the State. But we cannot believe that the State of Wisconsin will be satisfied to thus stultify itself before the world by sustaining the appointment to a scientific position of one who confessedly knows nothing of its duties.

4. *Geological Survey of Alabama. Report of Progress for* 1874; by EUGENE A. SMITH, Ph.D., State Geologist. 140 pp. 8vo. Montgomery, Alabama, 1875.—Prof. Smith, after a history of the Geological Survey of the State hitherto undertaken, gives in a brief and systematic form the results of his geological work carried on during the past year. His labors were confined mainly to the portions of the State occupied by crystalline rocks, and especially to Chilton, Talladega, Cleburne, Randolph, Clay, Coosa, Tallapoosa, Chambers, Lee, and Elmore Counties. The rocks, and the mines or valuable minerals they contain, are described with care. The crystalline rocks include granites, gneiss, hornblendic rocks, mica schist, hydromica slate, chlorite slate; and also hypersthenyte or noryte, which occurs near Columbus; with crystalline limestones, which are partly dolomitic. Among the materials of economical importance occurring in these counties

are included marbles; soapstone at many localities in Clay, Tallapoosa, Chambers and other counties; white porcelain clay; asbestus; mica; corundum, in Tallapoosa County; graphite; gold; copper, in Wood's Mine in Cleburne County; iron ores, which are associated usually with the hornblendic rocks. The Report closes with chemical analyses of various iron ores, coals and limestones.

5. *Geological Survey of New Jersey. Annual Report of the State Geologist*, Prof. GEORGE H. COOK, for the year 1874. 116 pp. 8vo. Trenton, N. J., 1874.—Prof. Cook gives, in his Report for 1874, new information on the remarkable series of Archæan iron mines in New Jersey, and also facts respecting mines of limonite, copper, zinc, and the quarryings of pottery clay, etc. He states that 700 to 1000 tons of metallic zinc are now made annually from New Jersey ores, 850 tons from Pennsylvania ores, and more than 3000 tons from Western ores. With regard to the white clays of New Jersey, he remarks that 265,000 tons of fire clay are annually dug in Middlesex County, about Woodbridge, Perth Amboy and South Amboy, and sold at an average price of $3.50 per ton. Besides, there are 20,000 tons of stoneware clay dug at South Amboy, which averages $4 per ton, and is shipped to all parts of the United States.

Prof. Cook states that the *geological position of the clay deposits is in the Cretaceous formation*, and that the stratum constitutes its lowest member. They occur in a belt of country stretching across the State from the northeast, on Staten Island Sound and Raritan Bay, to the southwest, ending on the Delaware in Gloucester County. Its northwest edge adjoins the Triassic red sandstone from Woodbridge to near Trenton, where for five or six miles it borders on the gneiss; thence to the southwest end, it follows along or near the Delaware River.

6. *Preliminary Report of the State Geologist of Oregon;* Rev. THOMAS CONDON. 22 pp. 8vo. Salem, Oregon. 1874.—Mr. Condon, who has in past years done much in the collection of the mammalian and other fossils of the interior of Oregon, was appointed State Geologist in October, 1872, but upon a salary of only $1000 per annum, and under the idea of his devoting but part of his time to the object. This preliminary report gives an interesting review of the topography of the State, with important facts respecting the great lake basins of the Tertiary, and the relations of the Tertiary sandstones containing silicified and coaly trunks of trees, to the overlying igneous rocks in the line of the Cascade Range. He speaks of the latter rocks as having a thickness exceeding 4000 feet, and as consisting of porphyry below, trachyte over the porphyry, and heavy dark basalt above.

7. *Prof. O. Heer's Arctic Flora.*—The first two volumes of Prof. Heer's Arctic Flora are as widely known as the name of the celebrated author. A third volume has been published recently*

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* By Wurtzer & Co., of Zurich.

from materials collected by the Swedish Polar expeditions under the direction of Prof. Nordenskiöld. This volume admirably completes the work, by the superior character of its execution and by the interesting facts which it exposes in regard to the geological floras of the Arctic and Polar regions. It contains, 1st, a paper on the Carboniferous flora of the Arctic zone (eleven pages and six plates); 2d, the Cretaceous flora of the Arctic zone (one hundred and forty pages with thirty-eight plates); 3d, an appendix to the Miocene flora of Greenland (three pages and five plates); and 4th, a general revision of the Miocene flora of the arctic zone (twenty-four pages).

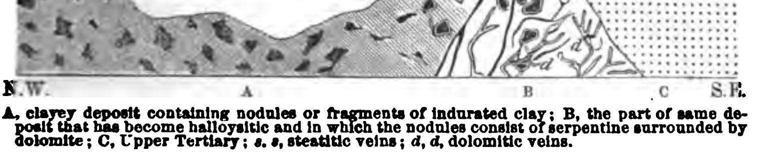
The most important part of this publication is that on the Cretaceous floras, not merely for the reason that the vegetation of the Cretaceous land was till now scarcely known, but because its exposition brings out documents related to a number of interesting problems which are masterly presented and discussed by the author. This part has a peculiar interest for the naturalists of this country, on account of the discovery in Nebraska and Kansas of a terrestrial formation closely related in time to that of the Upper Cretaceous of Greenland, in which a large number of fossil plants have been discovered and recently described by Lesquereux in a volume published by the Department of the Interior, as noticed on page 227. The fossil plants of Greenland are from two divisions of the Cretaceous; the lower one, which, from the character of its flora, is intimately allied to the Upper Jurassic, the flora being mostly composed of Ferns, Conifers, Cycadeæ, with a few monocotyledonous and a single dicotyledonous species, representing apparently a kind of *Populus*. The Upper Cretaceous division has in its flora, with a large proportion of Ferns and Conifers, two species of Cycas only, and thirty-four dicotyledonous species out of sixty-two which compose the whole flora. Among these there are species of the Genera *Myrica*, *Ficus*, *Sassafras* (one species), *Andromeda*, *Diospyros*, *Magnolia*, etc., which are also represented in the Cretaceous of Nebraska, either by identical or related forms. This indicates an evident relation, which, however, is far less marked than would be expected between two groups of plants from formations where synchronism is probable. For among the one hundred and thirty species described from the Dakota group of Nebraska, one hundred and thirteen are dicotyledonous and many of the genera, *Liquidambar*, *Salix*, *Betula*, *Alnus*, *Quercus*, *Fagus*, *Platanus*, *Laurus*, *Liriodendron*, *Menispermum*, are not represented in the flora of Greenland. In Nebraska also there is a preponderance of species referable to *Sassafras*, while from Greenland one leaf only is described as of this genus. As all these genera are now widely distributed in the North American flora, this preponderance of American types leads us to refer the origin of our present flora to the American Cretaceous.

An intermediate division, that of the Middle Cretaceous, is represented in the volume of Heer by a small number of plants from Spitzbergen, in all sixteen species of Ferns and Conifers with only one *Equisetum*.

8. *On Serpentine Pseudomorphs after Monticellite, a Lime-magnesia Chrysolite;* by G. vom Rath. (Monatsb. k. Akad. d. Wiss., Berlin, Nov. 19, 1874.)—The pseudomorphs described by Vom Rath are from the Pesmeda Alp, on Mt. Monzoni in the Tyrol. The syenyte, dioryte and "augitic greenstone," which constitute the Monzoni peak, come up through limestone [of the Triassic formation] which is in part crystalline; and this limestone contains many crystallized silicates near its junction with the other rocks, viz., fassaite, vesuvianite, gehlenite, garnet, spinel, etc. In a high ridge adjoining the Pesmeda Alp, at a height of about 2500 yards, the limestone, near its contact with "augitic greenstone," affords crystals of the form of monticellite, along with others of anorthite, garnet and spinel. The monticellite crystals, some of which are two inches long, are all changed to serpentine. They occur mixed with fassaite, and with a blackish green spinel which is also in part serpentine. The color of the pseudomorphs is light brown, yellowish and occasionally white. The crystals within are irregular in texture and color, as is well represented on a plate showing magnified sections. Vom Rath gives several excellent figures of the crystals. Unaltered monticellite has not been found at the locality, but it occurs massive (Breithaupt's batrachite) to the west of Pesmeda and to the southeast of Mt. Monzoni, near the junction of the limestone and syenyte. This massive kind is externally altered.

Vom Rath also states that the locality of serpentine-pseudomorphs affords others of *monticellite altered to fassaite.* The crystals are an inch and less in size. They have sometimes a nucleus of serpentine or calcite. The fassaite pseudomorphism in all cases preceded the serpentine.

9. *On the conversion of an argillaceous rock to Serpentine;* by A. d'Achiardi. (Bolletino R. Com. Geol. d'Italia, 1874, p. 336.)—In Montajone, Italy, south of San Miniato, there is connected with the Upper Tertiary an unconsolidated earthy mass, greenish white in color, and partly halloysite-like in composition (B in the figure below), which is intersected by numerous dolomitic (*d*, *d*) and steatitic (*s*, *s*) veins, and contains besides isolated nodules of a fine green, compact serpentine, more or less translucent, which are often coated with dolomite. The bed or mass followed to the northwest loses its steatitic and dolomitic veins, and graduates into



A, clayey deposit containing nodules or fragments of indurated clay; **B**, the part of same deposit that has become halloysitic and in which the nodules consist of serpentine surrounded by dolomite; **C**, Upper Tertiary; *s. s*, steatitic veins; *d*, *d*, dolomitic veins.

a rough, dark clay bed (A), containing nodules that are like the serpentine nodules in form, size and position, but which consist of indurated clay, reddish gray or grayish yellow in color. From the unaltered clayey nodules there is a gradual passage, through others partly altered, to those consisting wholly of serpentine. D'Achiardi concludes that the serpentine nodules were produced through the alterations of the clay nodules. He refers their formation to the action of hot magnesian waters on a hydrated aluminous silicate, and the replacement thereby of the alumina by magnesia. The halloysite-like material he regards as containing the alumina which was removed from the nodules in the process of alteration.

10. *On the formation of Mountains and the hypothesis of a liquid substratum beneath the Earth's crust;* by Rev. O. Fisher, (Proc. Cambridge Phil. Soc., Feb. 22.)—This paper was a sequel to one read in Dec., 1873, in which it had been shown that, upon the supposition that the inequalities of the earth's surface have been formed by contraction of its volume through cooling, they are too great to be so accounted for if the earth has cooled as a *solid* body. In the present communication it was therefore assumed that there is a *liquid* layer beneath the cooled crust; and an approximate calculation was made of the form which the corrugations of a flexible crust would take if so supported. It was shown that their lower surface would consist of a series of equal circular arcs arranged in a festoon-like manner, and having a radius $2\frac{\rho}{\sigma}c$, where ρ, σ are the densities of the crust and liquid respectively, and c the thickness of the crust. It was argued that the consequences of this form of corrugation agree fairly well with some of the phenomena of mountain elevation, but that it does not suffice to explain the ocean-basins and the continental plateaux.—*Nature*, March 18.

11. *Botany of the Island of Amsterdam.*—It is a curious fact that the little island of Amsterdam, in the South Indian Ocean, is known to be covered with trees, whilst the island of St. Paul's, only fifty miles to the south, is destitute of even a shrub. Botanists have long been anxious to determine the character of the Amsterdam forest, but the difficulty of effecting a landing on the island has generally prevented the collection of specimens. In the last part of the *Journal of the Linnean Society*, Dr. Hooker announces that at length he has received the desired specimens, these having been collected by Commodore Goodenough, who states that they represent the only species of tree growing on the island. Dr. Hooker identifies this with the *Phylica arborea* of Thouars, a tree which, strangely enough, is found in the remote Island of Tristan d'Acunha. It is a curious problem for those who study insular Floras to suggest how the same plant can have established itself on these two little specks of land, separated from each other by about five thousand miles of ocean.—*Athenæum*, March 6.

12. *Amphioxus lanceolatus.*—Prof. Huxley has shown, in a paper read before the Royal Society (Ann. Mag. Nat. Hist., IV, xv, 225),

that the anomalous *Amphioxus*, which Hæckel refused to admit into the class of Fishes, is closely related in some important characters to the Marsipobranch fishes. In the mouth with its tentacles, and in some other respects, *Amphioxus* is much like *Ammocœtes* (the young of *Petromyzon*). "The several pairs of nerves in *Amphioxus* which leave the cerebro-spinal axis between those which answer to the *portio dura*, and the optic nerve are represented by the third, fourth, fifth and sixth pairs of cranial nerves of the higher Vertebrates. The cranium of the latter is represented by those segments of the body of *Amphioxus* which lie in front of the fifteenth, counting from before backward, and their cranial nerves by the corresponding anterior pairs of nerves. No auditory apparatus was distinguished; but "in all other respects *Amphioxus* conforms to the Vertebrate type;" and, considering its resemblance to the early stages of Petromyzon (described by Schultze), there is no reason for removing it from the class *Pisces*. On account of its permanently segmented skull and its many other peculiarities, Prof. Huxley regards it as the type of a primary division which he names *Entomocrania*, other fishes being *Holocrania*.

13. *Zoological Station at Naples.*—Dr. Anton Dohrn has issued a Catalogue* of the Library of the Zoological station at Naples. Nearly one half of the volumes belong to Dr. Dohrn, and formed his private library at the time he established the zoological station; the other half consists of works presented either by the publishers or by the authors to the station. As a library of embryological and anatomical works on marine animals, it forms an admirable nucleus for the building up of a great zoological library. It only needs the continued interest which has been shown by all working naturalists to supply the station with everything published needed to carry on investigations in every department of zoology. Dr. Dohrn is anxious to secure the co-operation of American naturalists and hopes that they will remember the zoological station in the distribution of their papers. A. A.

14. *Birds of the Northwest:* a hand-book of the Ornithology of the Region drained by the Missouri River and its tributaries; by Elliott Coues. 8vo, 791 pages.—This volume forms part of the miscellaneous publications of the United States Geological survey of the Territories, under Dr. Hayden. It includes quite full synonyms of all the species and varieties belonging to the region, with many valuable descriptions of the habits, geographical distribution and variations of the species. The families *Laridæ*, *Colymbidæ*, and *Podicipidæ* are treated monographically, all the North American species being fully described. V.

15. *Geographical Distribution of Animals.*—Mr. A. R. Wallace has in the press a work on this subject, which is to be illustrated with elaborate maps, and woodcuts of animals. It will be published by Messrs. Macmillan.—*Athenæum*, Feb. 27.

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* Die Bibliothek der zoologischen Station zu Neapel, Verzeichniss der daselbst bis zum Ende des Jahres 1873 vorhandenen Bücher. Leipzig. 8vo., pp. iv, 791. 1874. Wilhelm Engelmann.

III. ASTRONOMY.

1. *On the Total Eclipse of the Sun of April* 16*th, as observed by Mr. Stone at Klipfontein in Namaqualand, South Africa*, 55 miles from the sea and 20 miles from the central line of shadow.—The general results which Mr. Stone considers he has obtained from the observations are summarized as follow: "1. A confirmation of Young's observations of the general, or nearly general, reversion of Fraunhofer's lines in the spectrum of the corona near the photosphere. 2. A spectroscopic examination of the outer corona, in contra-distinction to the inner corona, carried to the extent of rather more than a degree from the sun's center, which has proved that the spectrum of the outer corona consists of a linear spectrum of one bright line, either exclusively or sensibly, whose wave-length is 5,312, and of an ordinary sunlight spectrum with absorption lines. The spectrum of the outer corona has been shown to fade gradually away as the extreme visible limit of the corona is approached, and not to disappear sharply as if the extreme limit of the corona had been reached. 3. This spectroscopic examination of the outer corona, combined with the unchanged character of its principal features as seen at Namaqualand, Griqualand, and Basutoland, at intervals of absolute time extending to 10^m, and at distances of more than 500 miles, proves, I venture to think, the solar origin and cosmical character of the outer corona. The want of coincidence in the positions of the general extensions of the inner corona with the main branches of the outer corona is an additional argument against the atmospheric origin of the outer corona. 4. A comparison of the drawing by Mr. Hall, made at Namaqualand, and the photographs obtained in 1869 and 1871 shows the permanent character of the contraction of the inner corona in a direction parallel to, or nearly parallel to, the sun's axis of rotation. The strongly marked character of the contraction of the outer corona in the same direction, as seen in the eclipse of 1874, may not improbably ultimately lead to a similar inference in the case of the outer corona also."—*Monthly Notices*, February, 1875.

2. *Schiaparelli's Observations of Comet III* 1862.—The careful observations made by Professor Schiaparelli of Comet III 1862, which, among the large comets, possesses a peculiar interest from its connection with the August meteors (the *Perseids*), have been lately published, the delay having been occasioned by the difficulty of reproducing faithfully the original drawings, which have now been drawn on stone by Signor Tempel of the Milan Observatory. Prof. Schiaparelli, in his *Memoir*, first gives the original observations, and then discusses them under various heads. The apparent brightness of the coma increased from 6 mag. on July 24, to 1·7 mag. on August 31, and the intrinsic brightness, calculated by the usual formula, $\frac{1}{r^2\Delta^2}$, also increased from 0·210 to 1·017 within the

same period, but not uniformly; increasing for ten days, then remaining stationary for a like period, after which there was again an increase for ten days, followed by a second stationary period; these changes being unaccompanied by any marked variation in the size of the head. Corresponding to the increase in brightness of the head, there was a marked decrease in brightness of the nucleus, which could not, Signor Schiaparelli considers, have exceeded 350 miles in diameter when nearest to us, from which he concludes that its density must then have been considerable, as it alone contained the materials for the coma and tail in all succeeding apparitions of this comet. The appearance of the luminous jets is then discussed, and is connected with a general want of symmetry in the head of the comet, the left side of which (supposing the tail to be below the head) was more developed, so that the comet resembled a stick with a knob on one side. The most remarkable feature in this comet was the large inclination of the tails (of which there were three on August 21) to the plane of the orbit, a fact which was established by the circumstance that the apparent angle between the tail and the prolongation of the radius vector did not change sensibly on the passage of the earth through the plane of the orbit on August 10. The lateral deviation of the tail, which was very considerable, Prof. Schiaparelli refers to an explosive force from the left of the nucleus, from the effect of which the particles projected from the head would, under the action of a repulsive force from the sun, describe parabolas, having the head as vertex and the radius vector as axis, the tail being curved back till, near its extremity, it became parallel to the radius vector. This would require the nucleus (which was apparently unsymmetrical) to preserve a nearly constant direction in space, which the author considers might result either from a polar force residing in the sun, or from the absence of rotation of the nucleus. Another imporant conclusion drawn by Signor Schiaparelli is that the particles of the tail exercise a mutual repulsion on each other, causing the anterior boundary to curve forward till it cuts the prolongation of the radius vector, the axis of the parabolas which would be described by the particles of the tail if only under the sun's repulsive force.—*Monthly Notices*, February, 1875.

3. *The Gold Medal of the Astronomical Society.*—The Council have awarded the gold medal to Prof. D'Arrest, for his work entitled "Siderum Nebulosorum Observationes Havnienses institutæ in Specula Universitatis per Tubum Sedecimpedalem Merzianum ab Anno 1861 ad Annum 1867," and his other astronomical works. The President will explain to the meeting the grounds of this award after the Annual Report.—*Monthly Notices*, February, 1875.

4. *Fall of a meteor in Iowa.*—A meteor fell in Iowa on the night of Feb. 12th with loud detonations. The principal portion of the fragments from the stone have been secured for the Iowa State University by Prof. Leonard.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Anderson School of Natural History.*—The experience of the past two years has shown that it will be impossible to carry on the School of Natural History at Penikese on the same terms as formerly. At the close of the last session the Trustees had exhausted their resources. They propose to charge a fee of fifty dollars for the season of 1875, and to carry on the school during the coming summer if a sufficient number of applications are received in time to make the necessary arrangements. Even with the full complement of students, there will be a considerable deficit (as was the case last year) to be met by the friends of the school; the position of Penikese necessitating many expenses which need not be incurred in a more favored locality. Applications should be sent at once to the Director at Cambridge, Mass.; preference will be given to teachers and to those who intend becoming teachers. ALEXANDER AGASSIZ, *Director*.

2. *Note on a supposed change in the Climate of Scotland;* by A. BUCHAN.—Mr. Buchan concludes a paper in *Nature* of February 25, as follows: "The general result of the inquiry then is, that though large annual fluctuations of temperature have occurred, yet the warm and the old cycles, extending over longer or shorter periods, are so distributed over these long intervals as to give no indication that there has been any tendency toward a steady increase or decrease in the temperature, or that any permanent change has taken place in the climate of Scotland. And since the same remark applies with equal force to the observations of the separate months, it follows that meteorological records give no countenance to the idea of a permanent change having occurred in the climate of Scotland either as regards summer heat or winter cold. It may be added that during the past seven years the temperature of July has been above its average respectively 2·8,° 1·7,° 2·0,° 0·2,° 1·7,° 1·0,° and 1·8,° and that of December, as compared with its average +1·5,° —4·2,° −5·6,° −1·1,° −0·8,° +3.4,° and −7 4;° results quite in the opposite direction of the popularly entertained belief that the summers are colder and the winters milder than formerly."

3. *On the Periodicity of Thunder-storms (Ueber gesetzmässige Schwankungen in der Häufigkeit der Gewitter während langjähriger Zeiträume);* by W. v. BEZOLD.—Von Bezold, in his study of the periodicity of thunder-storms, made use of a series of observations that extended over a period of 105 years (1764–1869) at one locality (Kremsmünster, Bavaria) almost without a break, and of others less complete at other places.

His conclusions he expresses as follows. In years when the temperature is high and the sun's surface relatively free from spots, thunder-storms are abundant. Since, moreover, the maxima of the sun-spots coincide with the greatest intensity of auroral displays, it follows that both groups of phenomena, thunder-storms and auroras, to a certain extent supplement each other so that years of frequent storms correspond to those auroras, and *vice versa*. He observes that such a connection between sun-spots and storms does not by any means sanction the supposition of a direct electrical interaction between the earth and sun, but may be simply a consequence of a degree of insolation dependent upon the sun-spots.

These changes in the insolation, according to Köppen, manifest themselves in different latitudes not cotemporaneously but successively. The phenomena of thunder-storms, on the other hand, do not depend alone upon the condition at the place in question with respect to temperature, but also on the condition of the atmosphere at points far distant and belonging to another zone. This appears most distinctly in the storms which accompany electrical displays. The peculiar intermediate position which the weather curve takes between the curves of sun-spots and temperature may possibly find its explanation in this fact.

Von Bezold closes with showing that observations recently published in Saxony confirm in a striking manner the conclusions he had reached.—*Ber. Ak. München*, Nov. 7, 1874. E. S. D.

4. *The Blind Fish and some of the associated species of the Mammoth Cave, Kentucky, probably of Marine origin.*—Mr. F. W. PUTNAM, in an artical published in the Bulletin of the Essex Institute, vol. vi, No. 12, 1874, remarks as follows on the origin of some of the present inhabitants.

That many, or, with two or three exceptions, nearly all of the thirty or forty species of vertebrates, articulates, mollusks and still lower forms, including a few plants, now discovered in the caves of Kentucky, are of comparatively late introduction, is probable from the fact that they are so closely allied to forms living in the vicinity of the caves. But that the blind fishes, the Chologaster and a few of the lower forms of articulates, as the Lernæan, parasitic on the blind fish, may have been inhabitants of the subterranean streams for a much longer period, is worthy of consideration on the following grounds:

First, the blind fish family has no immediate allies existing in the interior waters,* the only species of the family, in addition to the three found in the Mammoth Cave, being known at present only from the rice ditches of the low coast of South Carolina.

Second, the Lernæan parasite is much more common on marine fishes than on strictly fluviatile species, and is more decidedly a marine than a fresh water form. These facts may therefore be taken as at least indicating the probability of the early origin of some part of the great cave system of the region of the Ohio Valley, and while there may be nothing in the present structure of the caves to indicate their having been formed in part while in contact with salt water, the supposed erosion of the limestone and the modification of the early formed chambers by later action should be carefully considered before it can be denied that the caves were not, in some slight part, for a time, supplied with marine life. Until a specimen of Chologaster, or some other member of the family, has been obtained in the external waters of the Ohio Valley, it is hardly logical to regard the family to which the blind fishes belong as one originally distributed in the rivers of the Ohio Valley, and afterward becoming exterminated in the rivers and only existing in two such widely different localities as the coast of South Carolina and the subterranean streams of the southwestern States. That marine forms of life are found in our fresh water lakes and rivers is known to be the case. The specimen of a shrimp exhibited was secured in the Green River, near one of the outlets of the Mammoth Cave. The fact that in some of the waters of Florida fishes once marine are now confined to the fresh water lakes of comparatively recent formation, and that in the St. John's River, and others of that State, many marine and fresh water species are found associated, are evidence of the change that may take place in the habits of some marine animals, while a recent announcement of the Gobiosoma found in the Ohio River* is another instance of a marine fish living in fresh waters.

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* In common with others I have considered the Heteropygii as belonging to the same order with the Cyprinodontes, but I now have, from further information of their structure, doubts as to their close association with that group. This subject will be presented on another occasion.

5. *Proceedings of the Cleveland Academy of Natural Sciences*, 1845 to 1859. 296 pp. 8vo. Cleveland, Ohio. Published by a gentleman of Cleveland.—The valuable papers which were read from time to time before the Cleveland Academy, previous to 1860, by Dr. J. P. Kirtland, Dr. J. S. Newberry, Col. C. Whittlesey, and others, and which have hitherto been published only in part in any scientific journal, are now for the first time gathered into a volume by the Society. The volume contains among its articles descriptions of species of fossil coal plants by Dr. Newberry, and of recent fishes by Dr. Kirtland, with important notes on the distribution and habits of birds, fishes, butterflies and other species by Dr. Kirtland; a paper on the Allegheny Coal field by Col. C. Whittlesey; notes on the Drift by Dr. Newberry and Col. Whittlesey, and short contributions on other topics of interest, including several valuable letters by Agassiz, Harris, and Bachman.

6. *Annual Record of Science and Industry for* 1874. Edited by SPENCER F. BAIRD, with the assistance of eminent men of science. 666 pp. 12mo. New York, 1875. (Harper & Bros.).—The Annual Record for 1874 by Prof. Baird comes well laden with the scientific news of the year past. A long introductory chapter contains a brief review of the chief stages of progress in the various sciences, theoretical and industrial; and then follow abstracts of various papers in science, announcements of new discoveries, statements of new facts about old discoveries and new illustrations of principles, accounts of scientific expeditions and institutions recently established, an obituary chapter, and a list of new works. The book has facts of interest for all classes of readers.

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* Putnam, notice of *Gobiosoma molestum* from the Ohio. Amer. Nat., viii, Feb., 1874.

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[THIRD SERIES.]

ART. XLIII.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXXII.—*Results of Dredging Expeditions off the New England Coast in* 1874; by A. E. VERRILL.

DURING the summer of 1874 Prof. S. F. Baird, U. S. Commissioner of Fish and Fisheries, established the headquarters of the Commission at Noank, Conn., a village situated on Fisher's Island Sound, a few miles east of New London. A large party of naturalists, who were invited to take part in the investigations of the commission, availed themselves of the unusual facilities there offered for the study of marine life. The investigations of the invertebrate animals, in general, were placed in charge of the writer, but several others, especially Mr. S. I. Smith, Prof. A. Hyatt, and Mr. S. F. Clark, took a prominent part in this work. Extensive dredging operations were carried on from this station, by means of the U. S. steamer "Bluelight," under Commander L. C. Beardslee, U. S. N. These dredgings extended from 30 to 40 miles from Noank, in different directions; westward to the mouth of the Connecticut River; southward to Gardiner's and Peconic Bays and the waters south of Montauk Point; and eastward to the banks several miles south and east of Block Island, so as to connect with the dredgings of 1871. Temperatures of the surface and bottom waters were taken at more than ninety stations, and dredgings were made in a still larger number of localities.

A very large and interesting collection of invertebrate animals was secured. Among these are over 100 species new to the fauna of southern New England. Most of these are northern species, but many are undescribed. A large collection of algæ was made by Prof. D. C. Eaton and others. Prof. Baird, with several assistants, took direct charge of the fishes and fisheries, and made many interesting discoveries. He also obtained a valuable collection. A more detailed account of these investigations will be given in a future article.

During part of the month of September the Superintendent of the U. S. Coast Survey offered Prof. Baird the use of the steamer Bache, Capt. Platt commanding, to continue the dredging operations off the coast of Maine. This work was put in charge of Dr. A. S. Packard, as in 1873, and he was assisted by Mr. C. Cooke and Mr. Robert Rathburn. They made dredgings at about forty stations in the Gulf of Maine, off the coasts of Maine and New Hampshire, at various depths down to 125 fathoms. These localities may be conveniently grouped in five series.

a. Several dredgings on hard bottoms, near the Isles of Shoals and on Jeffrey's Ledge, in 25–51 fathoms (see Nos. 44, 46, 48, 77, 78).

b. An interesting series of dredgings on Cashe's Ledge, about 90 miles off Mt. Desert I., in 27 to 39 fathoms, hard and rocky bottoms.

c. One dredging on a new bank, discovered by Capt. Platt, in 32 fathoms, sandy bottom (No. 69).

d. Several dredgings in 36–48 fathoms, muddy bottoms, between Cape Ann and the Isles of Shoals (Nos. 38–41, and 78 in part).

e. Numerous localities in 50 to 125 fathoms, muddy bottoms, including most of the localities not already mentioned, over a wide area, both east and west of Jeffrey's Ledge, and extending from No. 62, off Pemaquid, Me., to the deeper parts of the Gulf of Maine, south of Cashe's Ledge.

Hard bottoms.—The collections from the hard bottoms (included under *a*, *b*, *c*) are much like those from similar and adjacent localities explored in 1873, of which nearly complete lists were published in this Journal (vol. vii, p. 502, May, 1874). Cashe's Ledge, as before, proved to be a rich dredging ground, remarkable for large numbers of rare northern species. Locality 78, in 35 fathoms, near Jeffrey's Ledge, was, properly speaking, a mixed bottom, mud predominating; but the dredge brought up some stones and large quantities of masses of firmly consolidated ferruginous mud and sand, most of which were irregularly broken and curved pieces, but some had the form of large, slightly conical tubes, 3 to 6 inches in diameter, and 12 to 15 inches long, the walls often being an inch or more thick. These are probably old uninhabited tubes of *Cerianthus borealis*, which have become firmly consolidated by some chemical action. Upon these fragments of tubes numerous species of Bryozoa, Ascidians and Sponges had established themselves, causing the collection from this place to resemble that of certain rocky bottoms.

Table of Stations in the Gulf of Maine, where dredgings and temperature determinations were made, in 1874.

Current No.	Date. Sept.	Hour.	Locality.	Nature of bottom.	Depth in fathoms.	Temperature.*		
						Air.	Surface.	Bottom.
38	2	12 M.	Thatcher's I. Light, about 10 miles south	Soft blue mud.	41	70° F.	66°	45°
39		1.38 P.M.	Do., about 13 miles south	Mud,	48	70	69	45·5
40		2.23 "	Do., 16 miles south	Blue mud,	43	70	69	47
41		3.00 "	Do., 18¾ miles south	Mud—rocks,	36–27	70·5	69	45·5
42	3	11.35 A.M.	Boon I. Lt., 6 m. N.W. by W. ½ W.	Brown mud,	68	69·5	67	52·5
43		2.32 P.M.	Boon I., N. by E.; hotel on shoal S.W. by W. ½ W.	Brown mud,	43	75	65	47
44		5.00 P.M.	Star I., S.W.; Duck I., W.	Rocky,	25	75	67·5	51
45	4	0.50 "	Boon I., 12½ miles W.N.W.	Soft mud,	88	65	67	40
46		2.28 "	Agamenticus Mt., N.W. by W. ½ W.	Hard, sandy mud,	51	65	69·5	42
47		4.00 P.M.	Jeffrey's Ledge, near last,	Sand & gravel,	25	64	58·5	45·5
48		4.30 "	Near last, Agamenticus N.W. ½ W.	Gravel,	36	64	56·5	47·5
49		--------	Boon I., W.N.W. 27 miles,	--------	113	64	65	40
50		7.00 P.M.	Lat. 43° 01′ 20″, lon. 69° 45′,	--------	100	60	56	40
51	5	2.15 A.M.	Lat. 42° 56′, lon. 69° 08′,	Mud & gravel,	105	60	55	41
52		5.10 "	Cashe's Ledge	Rocks & grav.	27	60	55	42
53		7.00 "	Cashe's Ledge	Soft mud,	73	63	55	42
54		8.24 "	Do. 3¾ miles N.W.	Mud,	110	61	61	42
55		2.00 P.M.	Do. 2½ miles S.	Gravel,	40	60·5	65	43
56		2.50 "	Do. 1½ miles S.S.E.	Gravel,	30	62	59	46
57		3.15 "	Near last	Rocky,	37	----	---	---
57a		4.00 "	Near last	Rocky,	39	----	---	---
58		7.00 "	Lat. 42° 03′, lon. 69° 05′,	Mud & gravel,	65	60·5	57	40
59		12.00 "	Lat. 43° 22′, lon. 69° 17′,	Mud,	92	61	55	41
60	6	4.00 A.M.	Lat. 43° 17′, lon. 69° 24′,	Mud and sand,	65	61	57	41
61		7.30 "	Boothbay, Me., harbor,	--------	5	64	58	51
62	7	--------	Pemaquid Pt., 10 miles N.	Mud,	48	62	62·5	47
63		0.40 P.M.	Pumkin I., 4 miles N.E.	Brown mud,	42	68	65	48
64		2.45 "	Pemaquid, 12 miles N.	Mud,	58	61·5	57	42·5
65		4.28 "	Monhegan I., 2 miles E by N.	Soft mud,	47	64·5	59	44
66	8	1.20 "	Do., 14 miles N.E. ¾ E.	Brown mud & gravel,	65	66	64	40
67		3.30 "	Seguin Lt., 19 miles N. by W.	Brown mud,	86	68	64	40
68		6.00 "	Do. N.W.; noar last,	Mud,	91	64·5	59	40
69		8.30 "	Lat. 43° 11′, lon. 69° 35′,	Sand,	32	64	60	46
70		11.00 "	Lat. 43° 03′, lon. 69° 36′,	Mud,	91	61·5	58	40
71	9	1.00 A.M.	Lat. 42° 55′, lon. 69° 36′,	Brown mud,	96	61	58	40
72		4.03 "	Lat. 42° 57′, lon. 69° 50′,	" "	125	61	57	39·5
73		6.30 "	Lat. 42° 58′ 30″, lon. 70° 00′,	" "	102	62	59	40
74		8.00 "	Lat. 43° 01′, lon. 70° 09′,	" "	88	62	60	39
75		9.30 "	Lat. 43° 02′, lon. 70° 15′,	" "	92	64	64	40
76		11.10 "	Lat. 43° 03′, lon. 70° 25′,	Mud & gravel,	51	64	63	42
77		1.30 P.M.	White I. Lt., 3½ miles E. ¾ S.	Rocky,	33	69	65	44
78	12	12.00 M.	Agamenticus Mt., N.W. by N. ½ N.	Blue clay, mud and sand,	35	61·5	60	43

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* I am informed by Dr. Packard that the surface temperatures cannot be regarded as perfectly reliable, for they were taken in a bucket of water in which the bulb of the thermometer was not always submerged. All the water-temperatures were taken with a Miller-Casella thermometer (numbered 18491), which was left down from 5 to 10 minutes for bottom temperatures. Our experience shows that these instruments should be down 10 to 20 minutes to insure perfect accuracy.

The following list includes the species additional to those enumerated last year. Those from Cashe's Ledge are marked C; those from Jeffrey's Ledge, J; those from Capt. Platt's new bank (loc. 69), P; those from locality 78 are marked loc. 78.

Additions to the list of hard-bottom species.

Crustacea.

Hippolyte Phippsii.	C.	loc. 78.
Diastylis quadrispinosa.	J.	
Pardalisca cuspidata.		loc. 78.
Stegocephalus ampulla.	J.	
Epimera cornigera.		loc. 78.
Monoculodes, sp.	C.	
Melphidippa, sp.	C.	
Melita, two sp.	C.	

Annelida.

Lagisca rarispina.	J.	
Nephthys circinata V.	J.	
Grymæa spiralis V.	J.	
Sabella neglecta?	J.	
Spirorbis valida V.		loc. 78.

Gastropoda.

Bela cancellata.	J.	
B. violacea.	C.	
Aporrhais occidentalis.	J.	
Trichotropis borealis.	C. J.	
Menestho albula.	C.	
Adeorbis costulata.	J.	loc. 41.
Diodora noachina, var. princeps.	C.	
Onchidoris pallida.	C. J. J.	
Dendronotus robustus V.		
Philine quadrata.	C. J.	

Lamellibranchiata.

Neæra arctica.	P.	loc. 69.
Thracia truncata.	C.	
Cardium Islandicum.	J.	
Nucula tenuis.	C. J.	

Bryozoa.

Discofascigera lucernaria.	C. J.	loc. 78.
Tubulipora serpens.	J.	loc. 78.
T. hispida (=T. crates St.)	J.	loc. 78.
T. incrassata.	J.	loc. 78.
Diastopora hyalina.	C.	loc. 78.
D. hyalina, var. simplex.		loc. 78.
Discoporella verrucaria.	J.	loc. 78.
Cellularia scabra.	P.	loc. 78.
C. Peachii.	C.	loc. 54.
Bugula avicularia.	J.	
Flustra papyracea.	J.	
Membranipora unicornis.	C. J.	
M. unicornis, var. Americana.	J.	
Lepralia spathulifera.		loc 78.
Eschara verrucosa.	C. J.	
E. verrucosa, v. propinqua.	C. J.	
E. verrucosa, var. patens.	C. J.	
E. elegantula.	C. J.	
E. lævis (=*Porella l* Sm.)	C. J.	
Hippothoa vulgaris.	C.	
H. divaricata.	J.	
Escharella porifera.	C. J.	loc. 78.
E. auriculata.	C. J.	
E. Landsborovii.	C. J.	loc. 78.
E. candida (St. sp.).	C. J.	
E. solida. =*Flustra solida* St., 1853. =*Eschara palmata* Sars, 1862.	C. J.	
Discopora scabra, v. plicata.	C.	loc. 78.
D. scabra, var. ovata.		loc. 78.
D. coccinea.	C. J.	
D. coccinea, var. ovalis.	C. J.	loc. 78.
D. Jacontini (=*Escharella J.* Sm.).	C. J.	

Echinodermata.

Crossaster papposus. C

Hydroida.

Gonothyræa hyalina.	C.	loc. 69.
Clytia Johnstoni.	C. J.	loc. 69.
Calycella plicatilis (Sars, sp.)	J.	loc. 46.
Lafoëa gracillima.	C.	
Halecium tenellum.	C.	
H. sessile.		loc. 69.

Muddy bottoms.—The dredgings on muddy bottoms agree closely with those of the previous year, as might be expected from the similarity of the localities, and their proximity. Most of the rare species previously obtained were again met with, so that for many species additional specimens of great interest were obtained. Thus a second specimen of *Pleurotomella Packardii* V. was dredged at loc. 54; and several of *Anachis Haliœeti* at loc. 58 and 60; *Caridion Gordoni* and large specimens of *Stegocephalus ampulla* at loc. 54 and 58, etc. A fine new species of *Asterina*, with dark dorsal spots, occurred at locality 54.

The following list includes most of the additional species, not enumerated in the lists published last year in this Journal (vol. vii, p. 411), though quite a number found on some of the muddy bottoms, but belonging properly on hard ones, are here omitted.

List of additions to the fauna of the muddy bottoms.

Crustacea.

Hippolyte polaris.	loc. 54.	Œdiceros lynceus.	loc. 41.
H. Phippsii.	loc. 54.	Syrrhoë crenulata.	loc. 41.
Crangon boreas.	loc. 41.	Metopa, sp.	loc. 54.
Stenothoë peltata.	loc. 54.	Byblis Gaimardii.	loc. 38–40.
Tritropis aculeata.	loc. 58.	Ampelisca macrocephala.	

Annelida.

Eunoa nodosa.		Amphitrite Grayi.	loc. 72.
Euphrosyne borealis.		A. intermedia.	
Ancistria capillaris V.			

Gastropoda and Lamellibranchiata.

Astyris rosacea.	loc. 63.	Glycimeris siliqua.	loc. 72.
Diodora noachina, var. princeps.	loc. 51.		

Bryozoa.

Diastopora hyalina.	loc. 38–40.	Bugula flexilis V., sp. nov.*	loc. 54.
Membranipora unicornis.	loc. 40.	Eschara elegantula.	loc. 62–65.

* *Bugula flexilis*, sp. nov. Plate VII, figures 1, 2.

Several rather long, slender, flexible, dichotomously divided branches radiate from close to the point of attachment, making a stellate cluster. Zoœcia in two alternating rows, smooth, oblong, slightly swollen in the middle, with a short tooth or spine on the outer angle; aperture terminal, oblique, rounded or oval. Avicularia, on the front of the zoœcia, remarkably large, nearly as broad as the zoœcia and more than half their length, compressed, fusiform, tapering gradually to the point of attachment. East of St. George's Bank, 430 fathoms, 1872; off Casco Bay, 95 fathoms, 1873; Gulf of Maine, 110 fathoms, 1874.

Discopora nitida, sp. nov. Plate VII, figure 3. From Vineyard Sound and Long Island Sound.

Easily distinguished by the very small apertures with elongated processes projecting inward from the sides; and by the acute lateral avicularia.

Lepralia Americana, sp. nov. Plate VII, figures 4, 5. =*L. Pallasiana?* V., in former papers. Long Island Sound to Beverly, Mass., low-water to 30 fathoms.

ART. XLIV.—*On the Primordial Strata of Virginia;* by WM. M. FONTAINE.

[Concluded from page 369.]

Rockfish Gap.

THE railroad cuts at this place afford some good exposures of the lowest Primordial strata. In the deep cut made for the west entrance to the tunnel, the junction of these rocks with the massive chloritic argillites, which here form the mass of the Blue Ridge, is well shown. The strata are here all inverted, and dip southeast, but at a much higher angle than the Blue Ridge slates. These latter lie in heavy plates, produced by the consolidation of numerous laminæ, with a dip of about 40° southeast. As described in a previous paper, they are of a dark greenish-gray color, the green being caused by films of chlorite. They are firm and hard, resisting decomposition and degradation quite well. The first stratum of the Primordial rocks which adjoins these slates presents a strong contrast with them, the line of junction being strongly defined. This stratum, No. (1) of the section here, is composed of very thinly laminated slates or shales, much compacted by pressure. They are not crystalline and hence not true slates. They are pale yellowish-gray, and are of the variety called by the Professors Rogers, "feldspathic slates with talcose matter." They decompose, or rather lose their coherence, much more easily than the argillites. Hence at their junction with these rocks, the height of the walls of the cut suddenly diminishes from twenty to five or six feet. Thickness 300 feet. (2.) On the west of these is a band of more siliceous composition. On the side next to (1), the layers of this rock, in color, do not differ much from (1), but are thicker than the preceding slates, and more sandy. The bedding thickens, and the amount of siliceous matter increases to the west, until we have a fine-grained white kaolin sandstone in pretty thick layers. Thickness 75 feet. (3.) The last rocks are suddenly succeeded by a conglomerate of brownish red color, indicating a sudden change in the conditions of sedimentation at this point, while the two preceding rocks graduate into each other, showing a gradual change of the material deposited, which was plainly the product of slow decomposition and accumulation. Nos. (1) and (2) are the equivalents of the highly metamorphosed sandstone, No. (1), of the Balcony Falls section, while (3) is the representative of the feldspathic conglomerates and their included shales seen there. The conglomerate here presents some very interesting points of difference.

This rock is well exposed in a quarry near the railroad, from which a large amount of stone has been removed. Its structure and composition is thus fully exposed. It lies in massive plates five to six feet thick, with thin seams of shaly matter between several of the plates. These thin seams are the diminished representatives of the shales which separate the beds of conglomerate at Balcony Falls. Some small veins of quartz, the result of metamorphic action, fill cracks in the mass. The coarser materials are rounded grains of quartz of the size of a garden-pea and under. Rarely is a particle of fresh feldspar seen. This feldspar has the same character with that found in the equivalent rock at Balcony Falls. On the other hand, numerous grains and lumps of this feldspar, in a condition of almost complete decomposition, are seen. These larger particles are imbedded in a slaty cement of highly ferruginous, decomposed, felsitic matter, which when scratched with a knife gives a decided cherry-red streak. Besides, distinct particles of hematite appear. Enclosures of angular fragments of the slates of the Blue Ridge are not rare.

The more decomposed condition of this material in this vicinity is explained by the fact that it was probably brought by currents from the southwest, and thus for a considerable time subjected to agencies tending to disintegrate it. At Rockfish Gap the shores of the Primordial sea were probably formed by argillites, while farther southwest, coarse syenites containing red feldspar and quartz were washed by the waters. The fresh state of the feldspar at Balcony Falls shows that this sediment must have been rapidly formed and poured into the sea. I have already given some account of an eruptive syenite which, in the vicinity of the Peaks of Otter, has penetrated the older metamorphic syenites, and have stated also that the lowest Primordial rock at Balcony Falls has been highly altered by contact action of a similar rock. It is probable that this rock is the product of deep-seated metamorphic action, produced by the sinking, in its earliest stages, of the bottom of the Primordial sea. This would cause great pressure against the resisting syenitic border, and might fuse a portion of it and squeeze this out, shattering the upper and firmer mass. An igneous rock capable of delivering feldspathic and quartzose material exists also at Rockfish Gap, as is disclosed in the tunnel. It is, however, small in amount, and being entirely enclosed in slates, was probably not exposed to any extent to the action of the waves. There is no evidence to fix its age, but if it was formed at this period, it probably caused a fracturing of the slates, which was the source of the fragments found in the conglomerate. The thickness of the conglomerate rock is 60 feet, and like the two preceding, it has a dip of 70° to the southeast. The strata so far form the western base of the Blue Ridge. The succeeding rocks form a series of low hills, which for a space of 2½ miles at this point lie between the mountain and the valley of South River, a branch of the Shenandoah.

(4.) We have next to the conglomerate No. (3), a partially concealed interval of 200 feet, in which the rocks are, when seen, kaolin shales and kaolin sandstone, principally the former. Hence from their softness, they occupy a valley. To the west of this is a band of kaolin sandstones, with layers of cellular, much indurated quartzite, and some subordinate beds of an indurated, gray, coarse sandstone, to be described farther on. This series, in which the quartzites predominate, has undergone much local metamorphism, apparently from hot siliceous solutions, which has rendered the material more resisting to denuding agencies. It occupies a range of hills of considerable height. It contains one bed of the gray sandstone 20 feet thick. Thickness 420 feet, or including the partly concealed band, 620 feet. This series of strata is the equivalent of No. (8) in the Balcony Falls section. The dip is high to the southeast; it is somewhat confused by the consolidation of the beds.

(5.) The first rock in this series is a highly indurated gray sandstone, the type of the beds above mentioned as forming occasional layers in No. (4). It is of dark gray color, and is composed of coarse grains which are by metamorphic action changed nearly to a compact texture. Specks of decayed feldspar occur, also numerous seams of quartz, and occasional impregnations of chlorite and epidote. The bedding is almost obliterated. This is the next most highly altered rock seen by me in the Primordial area. Next to this occurs a bed of diorite 20 feet wide, which has aided in producing the metamorphosis of the sandstone and the strata of (4). The thickness of the sandstone is 50 feet. The diorite is composed of hornblende principally, with a rather scanty amount of feldspar, apparently albite. The hornblende occurs in pretty large particles, while the albite cements them together. Next is a band of bluish-gray, coarse shales, 75 feet wide; then a partially concealed interval of 300 feet, occupied by similar rocks; then for 200 feet, several alternations of the same shales with bluish argillaceous sandstones. In these a layer 6 feet wide of red argillaceous hematite is found. Then a second bed of diorite 12 feet wide, resembling in all respects the first. All of these rocks have a comparatively low dip of from 30° to 40° to the southeast.

(6.) The above beds, which are mostly sandy shales, and are rather thickly-bedded in layers of from one to several feet, are succeeded by a band of very thinly-laminated, firm, olive slates or shales, in which the dip rises to 75° to the southeast. These slates, which in fact are highly compressed shales, are in places much crushed, and show on their faces fine wrinkles. They are 300 feet thick. We then have 30 feet of indurated brown sandstone, with some chlorite, and a good deal of partly decomposed feldspar interspersed in it. Then an interval of concealed rock occurs for 100 feet. We then have for 150 feet several alternations of brownish argillaceous sandstones, with greenish shaly beds, in which the dip comes down to 50° southeast. Then very finely fissile, pink-colored slaty shales, which weather purplish red. Thickness 200 feet. About 50 feet (on the east side) of these shales differ from the rest in color alone, being greenish when fresh, and taking a yellow tint on weathering. Then for 100 feet a bluish shale is found. The two last series of beds, (5) and (6), appear to be the equivalent of No. (9) at Balcony Falls. Their combined thickness is about 1450 feet, that is, provided no reduplication from folding occurs. This is possible, though not probable; I think. It will be seen that these rocks are characterized by the large amount of ferruginous matter which they contain, by which they are sharply distinguished from those which precede and follow them.

(7.) The dip in the preceding series gradually declines until it attains on the west side 40° S.E., which is still more decreased in No. (7), where it varies from 30° to 40° S.E. The strata now to be described are the equivalent of the Potsdam sandstone, and resemble No. (10) at Balcony Falls in their almost total freedom from iron and other coloring matters, also in the great amount of siliceous matter and kaolin present in them. But while at Balcony Falls we find many layers of very considerable thickness to be pure quartzite, all the strata here have some kaolin, and this substance forms by far the greater part of the rock. The system here consists of a vast number of thin layers, in which we may distinguish three classes of rock. *a.* A shale, pale gray to bluish-gray when fresh, composed almost entirely of kaolin, and occurring in thin plates. *b.* This is associated with a very fine-grained kaolin sandstone, the quartz grains being now perceptible to the naked eye. The kaolin still predominates, but this rock occurs in slabs four to eight inches thick, and with *a*, forms layers or beds interstratified with *c*, which is a kaolin sandstone of moderately fine grain, and has often a thickness of several feet in the individual layers. The quartz grains are mixed with an equal amount of kaolin, which here serves as a cement for them, and forms a peculiar, mealy-looking rock, which can be easily crushed and crumbled to a loose grit. All of these varieties assumed a drab or pale yellow color on weathering, from the oxidation of the minute amount of iron present. *c* is almost always present in some portion of the Potsdam strata, and may be considered in this part of Virginia to be characteristic of this formation. So clearly marked are the features of this material that a fragment of it may be recognized at a glance wherever found. *b* and *c* are, when fresh, very light gray in color.

The entire mass of this material is so shattered and broken that it lies in angular fragments in the beds, so that it may be removed with the pick and shovel. The Messrs. Rogers, in their descriptions of the composition of the lowest Silurian strata, often speak of their containing talc and imperfectly developed feldspar, which they regard as the product of incomplete metamorphism. While it may be true that such partial regeneration of minerals may occur on the large scale in these strata, I have seen no instance of it. This "imperfectly developed feldspar" is simply kaolin, which has lost more or less of its plasticity. A qualitative analysis of a fragment of *c* gave me as essential components: 1, Uncombined silica (sand); 2, combined silica; 3, alumina; 4, a little water, and no potash. The slaty shales of No. (1) in this section, which agree with the rocks called by Prof. Rogers talcose slates, have no magnesia, but are mainly composed of combined silica and alumina, with a marked amount of potash and iron, and a small amount of water and lime. The rock, *c*, contains also a trace of iron, and a little mica in fine scales. The conglomerate near the base of the system does not contain crystalline feldspar, because of the greater metamorphic action to which it has been subjected. The finding of feldspar in all stages of decay shows that this mineral is undergoing a process just the reverse of regeneration, and that strata composed of the fresh particles of degraded crystalline rocks have been mistaken for rocks partially crystallized by metamorphic action.

The beds last described have a thickness of 300 feet, and are succeeded by a partially-concealed interval, in which 50 feet of a similar rock are shown. From this point, for the space of half a mile no cuttings exist.* The space outside of the valley in which the road runs is occupied by low rounded hills, covered to a great depth by fragments of the rock last described. So great is this mass of matter that in some places where it is removed to be used as ballast for the railroad, excavations to the depth of thirty feet do not penetrate through it. This interval up to the valley of the South Branch of the Shenandoah, so far as can be judged from the partial exposures, is occupied by No. (7). Much of the loose matter on the surface seems to have been subjected to the action of some transporting agent, since it is mixed with more or less clayey matters. Its sharp angular condition shows that it has not been moved far. Large boulders of the metamorphosed quartzite occurring in No. (4) are found in it.

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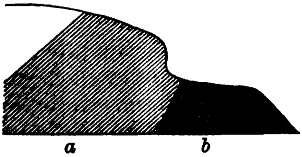
* No reliable estimate of the thickness of (7) can be given. It is perhaps 800–1000 feet.

Next to the hills occupied by (7) occurs the valley of the South Branch, about 500 yards wide. Here the strata are not exposed. This space was occupied entirely, or in great part, by the shales overlying the Potsdam strata. No very reliable estimate can be given of their thickness, but it cannot have been under 700 to 800 feet. They seem to have been of the same character, except the calcareous matter, with those associated with the Calciferous strata, which are found immediately on the west side of the stream. Here, at the town of Waynesboro, we have the following succession of strata, in which the dip is again high, viz: 60° to the southeast: first, purplish slaty shales, 50 feet; then dark gray, argillaceous limestone of earthy texture, 5 feet; next, greenish slaty shales, 15 feet; then dull purple, calcareous shales, 8 feet; next, very fissile, yellowish, calcareous shales, 40 feet. Argillaceous limestone follows, 5 feet thick; then very fissile, dark gray shales, 15 feet; then 10 feet of dark gray, thin-bedded limestone; then dark gray, very fissile shales again, 12 feet; lastly, slabby, argillaceous limestone, passing into the coarse, massive siliceous limestone which usually closes this series, 60 feet. It will be seen that this series is closely like that of similar age at Balcony Falls. To these rocks succeeds a wide belt of purple, greenish and gray shales, belonging probably to the same epoch. Then follows the Great Limestone of the Valley, the Auroral, or No. II, of the Messrs. Rogers.

It will be seen by a comparison of this section with that given at Balcony Falls, that the thickness of Nos. (5) and (6) in the section here is much greater than that estimated for their equivalent, No. (9), in that section. A disparity, no doubt, exists, though probably not to such an extent as it thus appears to do, since I have under-estimated rather than over-estimated the thickness of (9). Still there remains the fact that the proportion of shaly matter in the entire group at Rockfish Gap has greatly increased. This may be due to the nature of the shores of the Primordial sea, which, as we have seen, were probably composed of the fine chloritic argillites of the Blue Ridge. What could have been the source of the immense quantity of kaolin found here and everywhere in the Primordial strata, and what caused the almost total freedom of such kaolin rocks from iron, forms an interesting problem.

The following section represents the junction of No. (1) of the series with the argillites of the Blue Ridge, as seen in the west approach to the tunnel at Rockfish Gap. At *a* are the heavily-bedded slates of the mountain, while at *b* the thinly-laminated slaty shales, the lowest of the Primordial strata, are represented.



In the two detailed sections given above at Balcony Falls and Rockfish Gap I have been thus minute in my descriptions, in order that the reader may determine for himself how far the one confirms the other. At the same time, the strata at the two places may be taken as types which best represent the normal and inverted positions, and also the changes which result from the different development of certain members of the series.

Harper's Ferry.

The exposures at this place are not sufficient to admit of a detailed section. Enough, however, may be seen to show considerable changes.

The argillites, described in a former paper, extend to the west about a mile and a half from the railroad bridge. They are then succeeded by the lower Primordial strata, the change being quite abrupt. The line of junction is well shown on the Virginia side of the Potomac, on the railroad. The massive dark gray beds of argillite are seen here, with a dip of about 40° to the southeast, to abut against the highly inclined, more fragile lower Primordial strata. The change in the character of the rock is at once seen in the altered topography. The hills no longer present abrupt faces of firm rock close to the road, but recede with rounded slopes, which are so covered with earth and fragments of stone that it is difficult to find the strata *in situ.* The interval between the limestones of the Calciferous and the argillites is about 1,100 feet. These crushed rocks are succeeded immediately by a high cliff of Calciferous limestone, between whose massive walls and those of the argillites the more fragile lower Primordial rocks seem to have been compressed and crushed.

On the Maryland side the exposures are better. The rock forming the lower strata next to the argillites seems to be a variety of the lowest slates seen at Rockfish Gap. It differs from them in containing a greater proportion of siliceous matter in the form of very fine sand, intimately mixed with argillaceous matter and kaolin. This material produces a fine-grained, slaty sandstone and slate, which, when fresh, has a pale greenish-gray color, but weathers with a gray or yellowish-gray color. The dip of these strata is about 75° to the east-southeast. They, as far as seen, occupy most of the space below the Calciferous. These rocks, in their upper or more westerly portion, are succeeded by a highly-altered sandstone of brownish color and rather coarse grain. About forty feet of this are shown. It lies in rather thin laminæ, and has the same dip with the preceding. No other strata were seen until the Calciferous limestone, distant one or two hundred feet, was reached. This latter series also shows important modifications. The entire thickness of the strata lying below the Calciferous at Harper's Ferry cannot be much over 1,000 feet. So far as seen, only two varieties of rock occupy this space, viz: the slaty rocks above mentioned, which form by far the largest portion, and the sandstone, which much resembles some of the quartzites of the lower strata at Rockfish Gap. It will thus be seen that, along with the greatly diminished thickness of the formation, the disappearance of most of the coarser material seen to the southwest is to be noted here. It is not impossible that some of the beds have been engulfed in their upheaval, since on the Virginia side the manner in which the argillites abut against the Primordial indicates that the former were crowded in a solid mass over against the latter.

The Calciferous rocks at this place are almost entirely limestones. Instead of the numerous alternations of shale and impure limestone, which elsewhere form the lower beds, we have here only limestone. This is well shown in the high cliff on the Virginia side, which presents a vertical wall close to the railroad. This rock is of dark blue color, and is quite thinly stratified. The laminæ stand at an angle of about 80° E.S.E. It is much metamorphosed, with crypto-crystalline texture, and has numerous small seams of calc-spar penetrating it in every direction. Numerous small particles of fluor-spar occur interspersed in the mass. About 120 feet were seen. The only other member of the Calciferous seen here is the curious, rough, massive limestone which forms the upper member of the series everywhere, and which at every exposure presents the same physical features. This rock at Harper's Ferry contains workable deposits of iron ore. The excavations made on the Maryland side of the Potomac for the procuring of the ore have well exposed its character. It lies immediately to the west of the rock last described. From the examination made in the open cuts in the iron mine about two miles above the bridge, its character seems to be as follows: color dark gray, fracture rough and glistening, from its sub-crystalline texture. The hardness and weight are much above those of ordinary pure limestone. It lies in masses of the thickness of thirty feet and more, without bedding. Without close examination, it would be mistaken for a variety of igneous rock. The ore is an argillaceous limonite, occupying a seam varying in width from six to ten feet. This seam is the decayed outcrop of an argillaceous layer highly charged with pyrites, for at the depth of forty feet the ore is cut off by pyrites. This, and the other features of the rock, indicate that the entire mass of limestone has been metamorphosed by the action of water holding mineral matters in solution.

To the west of this limestone the usual formation of variegated shales occurs. The thickness of the limestone seen was about ninety feet.

The section at Harper's Ferry thus indicates an increase in the proportion of fine material over that shown to the southwest, along with a great diminution in the total amount of sedimentary deposits. This fact explains the absence in Pennsylvania (as noted by H. D. Rogers) of the conglomerates which occur in Virginia, and, according to Safford, reach their greatest development in Tennessee.

I have given the above sections in greater detail than I would have otherwise done, from the fact that Prof. Wm. B. Rogers, in his Virginia reports, does not (with the exception of the strata at Balcony Falls) give detailed descriptions of the individual members, and the order of succession of the lowest Primordial strata, the nature of his report forbidding such details. With the single exception of the *Scolithus*, there are no fossils to be seen in these lower rocks which can indicate their age. It will be seen from the above notes that in Virginia we have below the Calciferous limestones a great development of sandstones, shales and conglomerates, which attain in the middle portion of the State a thickness of over 2,000 feet, and increase in the proportion of coarse materials to the southwest. They probably attain greater thickness in that quarter, while to the northeast the amount of sediment diminishes, and the proportion of fine matter increases. This change is plainly due to the increasing development to the southwest of the syenitic rocks which formed the shores of the ancient seas, and to the greater violence in that direction of disturbing forces. The Potsdam sandstone forms one of the upper members of this group. Much further study of these strata is required to settle the question whether the entire series is a great expansion of the Potsdam, or whether divisions may be made corresponding to other epochs. The fact that at Rockfish Gap, and to the southwest, a great body of ferruginous slaty shales separates the lower, highly siliceous and altered, sandstones from the upper kaolin sandstones of probable Potsdam age, seems to indicate a change in the conditions of sedimentation sufficient to justify such a division, in which the Acadian strata may be found.

General Remarks and Conclusions.

Having given the above brief indication of the possible relations of the lower Primordial strata to each other, I will devote a small space to pointing out the apparent relations of these strata to the metamorphic crystalline rocks of the Blue Ridge. I am aware of the fact that not enough has been done to justify in all cases positive conclusions.

In my paper on the Blue Ridge of Virginia, published in the January and February Nos. of this Journal, I gave a description of certain coarse syenites and granites, which, in the vicinity of Balcony Falls and the Peaks of Otter, compose a large portion of the Blue Ridge, and which appear farther to the east in the short ranges of Tobacco Row and No Business Mountains. From the stratigraphical relations and composition of these rocks, it is plain that they are of Laurentian age. Whether the gneisses along the east foot of the Blue Ridge, and the syenites near Lynchburg, which appear to break through the mass of slaty rocks which occupy most of the country, belong to this system or to the slates, remains to be determined. These Laurentian rocks evidently increase in their development from the northeast portion of the State to the southwest.

Lying along and upon the eastern slopes of the syenites of the Blue Ridge, in the region of Balcony Falls, a formation of argillites occurs, which, in the northeast, occupy the entire space up to the Primordial strata. These rocks are covered to the east by a series of mica slates, schists, gneisses, etc., disposed in a great synclinorium which has its axis in the vicinity of the Catoctin Mountains. This axis is occupied by talcose limestones, quartzites, mica slates, hydromica slates, etc., standing nearly vertical. These latter strata in every respect bear a most striking resemblance to the rocks described by Prof. Dana as found in Berkshire Co., Mass. This entire belt of slaty, semi-metamorphic strata, is bounded on the east by a line drawn from northeast to southwest, and passing through a point four or five miles west of Alexandria, the eastern part of Louisa Co., and by Columbia, on James River. It will be readily seen that these rocks have all the characteristics of the Green Mountain series. I think that in this broad belt at least two systems exist, one older than the Primordial strata, and the other composed of metamorphosed Silurian. The argillites of the Blue Ridge belong to the former. Whether these are of Laurentian or Huronian age, I cannot undertake, in the present lack of detailed examinations, to decide. Their unconformability with the coarse syenites apparently shows them to be of later formation. How much of the mica slates and schists, if any, is of the same age with the argillites remains to be seen. I need not repeat here the evidence given elsewhere in this paper, proving that the argillites are more ancient than the Primordial rocks. The evidence for the supposed fact that the limestones and associated rocks found in the axis of the synclinorium are metamorphosed Silurian strata, is by no means so positive. The following points lead me to such a conclusion: 1. The position of the strata, since they occupy the center of the synclinorium. 2. Their nature, associations and dip. The rocks here found associated do not exist in other parts of the belt. Stated briefly, they consist of two ledges of limestone, 80 to 100 feet thick, enclosed in a vast mass of mica slate, and separated from each other by an interval of from two to three miles, principally occupied by the mica slates, in which occur two ledges of quartzite, each about 100 feet thick. This mode of occurrence indicates reduplication by folding. The dip is nearly vertical, while on each side the strata dip toward them. 3. Prof. Rogers states that in certain parts of the quartzites of this series he saw enclosed fragments of mica slate. 4. At a slate quarry in the rocks of this series, occurring in Buckingham Co., on the east of the Catoctin Mountains, Credner states that he found undoubted specimens of a cyathophylloid coral. He does not give a more particular description. He also states that they were badly preserved. I have not visited this quarry, but propose to do so. Should Credner not have been mistaken, his discovery would of course indicate the Silurian age of the slates of the quarry.

Before closing this paper, I will add a few remarks on metamorphism, suggested by my studies of the Virginia rocks. Some writers on the subject attribute regional metamorphism mainly to three agencies, viz: 1. Increased heat and pressure, caused by thick deposits. 2. The saturation of the strata with moisture. 3. The change of motion into heat, which involves a considerable disturbance of the metamorphosed region. I have not observed that close connection between the first and third of the above-mentioned agents, and the degree of metamorphism in a given region, which should exist if these bear to each other the relation of cause and effect. In order to show this, I must compare the condition of the strata in certain parts of the State, which, in the amount of metamorphism possessed by them, stand in strong contrast. For the purpose of this comparison I shall call the belt of country lying between the western edge of the great valley of Virginia and the Blue Ridge Mountains, a portion of which is described in this paper, the Primordial belt. The region extending from the Blue Ridge eastward, and bounded by the northeast and southwest line given above, may be styled the Middle belt. On the east of this belt, and between it and the Tertiary strata which extend some distance inland from the Atlantic, is a third belt, occupied by rocks of very different character from any occurring on the first and second belts. This region may be called the Eastern belt. The general character of the strata occupying it closely resembles that of the rocks of the "White Mountain system." The structure of this belt, stated briefly, seems to be as follows: The lowest strata are certain heavily-bedded granitoid gneisses, composed of a little quartz, black mica, and the feldspars, albite and orthoclase. These show themselves abundantly near Richmond and Petersburg. Passing to the west, these strata seem to be disposed in broad undulations, which sometimes bring them up through the overlying schistose strata, especially along certain lines where true igneous eruptive rocks penetrate them. These massive beds are covered by a comparatively thin formation of typical gneisses and mica schists, abounding in quartz and mica. The overlying rocks are gneisses in the eastern portion of the belt and mica schists in the center, which become hornblendic toward the western edge. These schistose rocks are disposed for much of the distance in broad anticlinals and synclinals, the strata over considerable spaces having generally a dip of 40°–50°, except where it is steepened by the protrusion of the underlying rocks, caused by local disturbances. This entire system is remarkable for the thorough metamorphism of the rocks, all the constituents being well segregated in large particles.

Now if we compare the amount of metamorphism existing in the strata of the several belts, we shall find it increasing in a remarkable manner as we proceed from the Primordial, through the Middle, to the Eastern belt.

I have already in this paper pointed out the difference in the amount of alteration shown in associated argillaceous and siliceous Primordial beds, and stated that I have seen nowhere in the *Primordial* belt the production over extensive areas of anything more than the first stages of metamorphism. It is true that certain strata exhibit a higher degree of change, but this is always due to intensified local action. If we pass from the lowest Primordial strata into those of the Middle belt, we note an abrupt change. The metamorphism of this belt is universal, but still not complete. The rocks are all crystalline, but the individual particles are small, and the masses which they form show more or less want of cohesion. In the thin quartzites of the east slopes of the Blue Ridge the texture is compact, and impregnations of metamorphic products occur. But in the quartzites along the Catoctin Mountains we may easily detect the separate grains of sand. The mica schists and gneisses of the entire belt rarely show scales of mica more than four or five millimeters long, and quartz in free particles is not common.

The disturbance of the strata in this area is much less than that which is found in the Primordial belt. The strata over considerable spaces are disposed in wide arches and extensive rolls, lying at a comparatively low angle. Yet in such regions they are as fully changed as where they stand on end, and, indeed, are often more so. From many indications, I am convinced that the most disturbed portions were thus affected, *after* the regional metamorphism had been produced, and that these later convulsions produced only intense local change. In the absence of measurements, no positive conclusions can be drawn as to the comparative thickness of these semi-metamorphic strata, but the indications are that it does not surpass, if it equals, the united thickness of the more highly convulsed rocks which lie to the west of them.

In the Eastern belt the strata are thoroughly altered, and in the coarseness of the crystallization, and firm, unyielding character of the rock, contrast strongly with those of the Middle belt. The massively bedded, underlying granitoid rocks are apparently of different age from the overlying schistose strata, being probably Laurentian. Still, in the amount of metamorphism exhibited, they do not surpass the latter. These schistose strata, which, as previously stated, have the general character of the "White Mountain series," do not exhibit that increased amount of disturbance and of thickness that should exist if these were the principal causes of their highly altered condition. On the contrary, these deposits appear to be thinner than those of either of the other belts, and are plainly less disturbed than the Primordial strata. As to the presence of moisture sufficient to produce metamorphism, the most favorable conditions must have existed in the Primordial belt, as this is known to have been covered with the water of the ancient seas certainly to as late a period as either of the other belts.

In examining the geological structure of the State from east to west, I have not found any such connection between the flexures as would justify the conclusion of Prof. H. D. Rogers, that the Appalachian system of folds extends to the Atlantic, and that they are more compressed next to the ocean, while they open out in proceeding west. Indeed, I do not see how he himself could have come to any such conclusion, after an inspection of his "Susquehanna Section" alone.

Such a connection between the flexures of the Silurian, Devonian, and Carboniferous areas does exist, but the Blue Ridge is the initial point on the east, and it may be accounted for by the fact that their flexures were mainly produced by a common cause acting in one period.

On the contrary, the folds of the eastern belt are mostly wider than those found in the other belts, and independent of them.

ART. XLV.—*Preliminary Inquiry into the Existence of Elements in the Sun not previously traced;*[*] by J. NORMAN LOCKYER.

IN a paper communicated to the Royal Society on December 12, 1872 (Phil. Trans. 1873, p. 253), I have shown that the test formerly relied on to decide the presence or absence of a metal in the sun, namely, the presence or absence of the brightest and strongest lines of the metal in question in the average solar spectrum, was not a final one, and that the true test was the presence or absence of the longest lines of the metal: this longest line being that which remains longest in the spectrum when the pressure of the vapor is reduced.

Of the test in question I have said in the paper already mentioned, "It is one, doubtless, which will shortly enable us to determine the presence of new materials in the solar atmosphere, and it is seen at once that to the last published table of solar elements—that of Thalén—must be added zinc, aluminium, and possibly strontium, as a result of the new method."

In order to pursue the inquiry under the best conditions, complete maps of the long and short lines of all the elements are necessary. It is, however, not absolutely necessary for the purposes of a preliminary inquiry to wait for such a complete set of maps, for the lists of lines given by the various observers may be made to serve as a means of differentiating between the longest and shortest lines, because I have also shown that the lines given at a low temperature, by a feeble percentage composition, or by a chemical combination of the vapor to be observed, are precisely those lines which appear longest when the complete spectrum of the pure dense vapor is studied.

Now with regard to the various lists and maps published by various observers, it is known (1) that very different temperatures were employed to produce the spectra, some investigators using the electric arc with great battery power, others the induction spark with and without the jar; (2) that some observers employed in certain cases the chlorides of the metals the spectra of which they were investigating,—others used specimens of the metals themselves.

It is obvious, then, that these differences of method could not fail to produce differences of result; and accordingly, in referring to various maps and tables of spectra, we find that some include large numbers of lines omitted by others. A reference to these tables in connection with the methods employed shows at once that the large lists are those of observers using great battery power or metallic electrodes, the small ones those of observers using small battery power, or the chlorides. If the lists of the latter class of observers be taken, we shall have only the longest lines, while those omitted by them and given by the former class will be the shortest lines.

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[*] **Extract from a memoir presented to the Royal Society in November, 1873, which has just been printed in the "Philosophical Transactions;" here cited from Nature of Feb. 25, 1875.**

In cases therefore in which I had not mapped the spectrum by the new method of observation referred to in my paper, I have taken the longest lines as thus approximately determined; for it seemed desirable, in view of the very large number of unnamed lines, to search at once for the longest elemental lines in the solar spectrum without waiting for a complete set of maps.

A preliminary search having been determined on, I endeavoured to get some guidance by seeing if there was any quality which differentiated the elements already traced in the sun from those not traced; and to this end I requested my assistant, Mr. R. J. Friswell, to prepare two lists showing broadly the chief chemical characteristics of the elements traced and not traced. This was done by taking a number of the best known compounds of each element (such, for instance, as those formed with oxygen, sulphur, chlorine, bromine, or hydrogen), stating after each whether the compounds in question were unstable or stable. Where any compound was known not to exist, that fact was indicated.

Two tables were thus prepared, one containing the solar, the other the more important non-solar elements (according to our knowledge at the time).

These tables gave me, as the differentiation sought, the fact that in the main the known solar elements formed stable oxygen-compounds.

I have said in the main, because the differentiation was not absolute, but it was sufficiently strong to make me commence operations by searching for the outstanding strong oxide-forming elements in the sun.

The result up to the present time has been that *strontium*, *cadmium*, *lead*, *copper*, *cerium*, and *uranium*,* in addition to those elements in Thalén's last list, would seem with considerable probability to exist in the solar reversing layer. Should the presence of *cerium* and *uranium* be subsequently confirmed, most of the iron group of metals will thus have been found in the sun.

As another test, certain of those elements which form unstable compounds with oxygen were also sought for, gold, silver, mercury being examples. None of these were found.

The same result occurred when the lines due to the jar-spark taken in chlorine, bromine, iodine, and those of some of the other non-metals were sought, these being distinguishable as a group by formation of compounds with hydrogen.

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* Potassium has since been added.

Now other researches, not yet completely ready for publication, have led me to the following conclusions:

I. The absorption of some elementary and compound gases is limited to the most refrangible part of the spectrum when the gases are rare, and creeps gradually into the visible violet part, and finally to the red end of the spectrum, as the pressure is increased.

II. Both the general and selective absorption of the photospheric light are greater (and therefore the temperature of the photosphere of the sun is higher) than has been supposed.

III. The lines of compounds of a metal and iodine, bromine, &c., are observed generally in the red end of the spectrum, and this holds good for absorption in the case of aqueous vapor.

Such spectra, like those of the metalloids, are separated spectroscopically from those of the metallic elements by their columnar or banded structure.

IV. There are in all probability no compounds ordinarily present in the sun's reversing layer.

V. When a metallic compound vapor, such as is referred to in III, is dissociated by the spark, the band spectrum dies out, and the elemental lines come in, according to the degree of temperature employed.

Again, although our knowledge of the spectra of stars is lamentably incomplete, I gather the following facts from the work already accomplished with marvelous skill and industry by Secchi of Rome.

VI. The sun, so far as the spectrum goes, may be regarded as a representative of class (β) intermediate between stars (α) with much simpler spectra of the same kind, and stars (γ) with much more complex spectra of a different kind.

VII. Sirius, as a type of α, is (1) the brightest (and therefore hottest?) star in our northern sky; (2) the blue end of its spectrum is open; it is only certainly known to contain hydrogen, the other metallic lines being exceedingly thin, thus indicating a small proportion of metallic vapors; while (3) *the hydrogen lines in this star are enormously distended*, showing that the chromosphere is largely composed of that element.

There are other bright stars of this class.

VIII. As types of γ the red stars may be quoted, the spectra of which are composed of channelled spaces and bands. Hence the reversing layers of these stars probably contain metalloids, or compounds, or both, in great quantity; and in their spectra not only is hydrogen absent, but the metallic lines are reduced in thickness and intensity, which in the light of V, *ante*, may indicate that the metallic vapors are being *associated*. It is fair to assume that these stars are of a lower temperature than our sun.

I have asked myself whether all the above facts cannot be grouped together in a working hypothesis which assumes that in the reversing layers of the sun and stars various degrees of "celestial dissociation" are at work, which dissociation prevents the coming together of the atoms which, at the temperature of the earth and all artificial temperatures yet attained here, compose the metals, the metalloids, and compounds.

On this working hypothesis, the so-called elements not present in the reversing layer of a star will be in course of formation in the coronal atmosphere and in course of destruction as their vapor-densities carry them down; and their absorption will not only be small in consequence of the reduced pressure of that region, but what absorption there is will probably be limited wholly or in great part to the invisible violet end of the spectrum in the case of such bodies as the pure gases and their combinations, and chlorine. (See I, *ante.*)

The spectroscopic evidence as to what may be called the plasticity of the molecules of the metalloids, including of course oxygen and nitrogen, but excluding hydrogen, is so overwhelming, that even the absorption of iodine, although generally it is transparent to violet light, may (as I have found in a repetition of Dr. Andrews' experiments on the dichroism of iodine, in which I observed the spectrum) in part be driven into the violet end of the spectrum, for iodine in a solution in water or alcohol at once gives up its ordinary absorption properties, and stops violet light.*

A preliminary comparison of the ordinary absorption spectrum of a stratum of 6 ft. of chlorine renders it not improbable that chlorine at a low temperature is the cause of some of the Fraunhofer lines in the violet, although, as said before, I have not yet obtained certain evidence as to the reversal of the bright lines of chlorine seen in the jar-spark.

There is also an apparent coincidence between some of the faint Fraunhofer lines and some of the lines of the low temperature absorption-spectrum of iodine.

Should subsequent researches strengthen the probability of this working hypothesis, it seems possible that iron meteorites will be associated with the metallic stars and stony meteorites with metalloidal and compound stars. Of the iron group of metals in the sun, iron and nickel are those which exist in greatest quantity, as I have determined from the number of lines reversed. Other striking facts, such as the presence of hydrogen in meteorites, might also be referred to.

An interesting physical speculation connected with this working hypothesis is the effect on the period of duration of a star's

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*** I have since obtained the same result by observing the absorption of I. vapor in a white-hot tube.**

heat which would be brought about by assuming that the original atoms of which a star is composed are possessed with the increased potential energy of combination which this hypothesis endows them with. From the earliest phase of a star's life the dissipation of energy would, as it were, bring into play a new supply of heat, and so prolong the star's light.

May it not also be that if chemists take up this question which has arisen from the spectroscopic evidence of what I have before termed the plasticity of the molecules of the metalloids taken as a whole, much of the power of variation which is at present accorded to metals may be traced home to the metalloids? I need only refer to the fact that, so far as I can learn, all so-called changes of atomicity take place when metalloids are involved, and not when metals alone are in question.

As instances of these, I may refer to the triatomic combinations formed with chlorine, oxygen, sulphur, &c., in the case of tetrad or hexad metals.

May we not from these ideas be justified in defining a metal, provisionally, as a substance, the absorption-spectrum of which is generally the same as the radiation-spectrum, while the metalloids are substances the absorption-spectrum of which, generally, is not the same? In other words, in passing from a cold to a comparatively hot state, the plasticity of these latter comes into play, and we get a new molecular arrangement. Hence are we not justified in asking whether the change from oxygen to ozone is but a type of what takes place in all metalloids?

My best thanks are due to Mr. R. J. Friswell for the valuable aid he has afforded me in these investigations.

ART. XLVI.—*On the occurrence of the Brown Hematite Deposits of the Great Valley*; by FREDERICK PRIME, Jr.*

THE Great or Cumberland Valley, which (under a variety of names) extends from Canada, through Vermont, Massachusetts, Connecticut, New York, Pennsylvania, Maryland, Virginia and East Tennessee to Alabama, is composed in part of limestones lying immediately above the Potsdam sandstone. Geologically, they belong to the era between the Potsdam and Trenton, and are commonly termed Calciferous. A peculiarity of these limestones is their great richness in limonite or brown hematite ores, which form a large part of the charges of the many blast-furnaces situated on their outcrops. The mines from which the ores are obtained are (properly speaking) only pits or quarries, from which the ore is extracted by means of picks or shovels, powder never being employed.

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* A paper read before the Amer. Inst. of Mining Engineers at New Haven, Feb. 25th, 1875.

The ore does not occur casually at any point in the limestone, but forms regular lines, following apparently the outcrops of certain beds. In Lehigh County, Penn., where I have been engaged on the geological survey of the State during the past season, there are three or four of these lines. During the progress of the survey I was much struck by the fact that the two most important and promising lines of outcrop were—one, at the base of the crystalline schists (forming the South or Lehigh Mountain), and overlying the Potsdam sandstone conformably; the other, near the line of junction of the Calciferous limestone with the Hudson River slates. The mines along these two lines—following the topography of the country—were in place, richer and more permanent than those of the belts between, which had been more decomposed *in situ* and were generally leaner.

A peculiarity of these two lines of outcrop, and one to which I particularly desire to draw attention, is the occurrence of what I at first mistook for a highly altered slaty limestone, but which on subsequent analysis proved to consist in great part of damourite (hydrated potash-mica). The following are analyses of this damourite-slate:

I. From Fogelsville, Lehigh County, within a few hundred feet of the contact of limestone and slate, by Dr. F. A. Genth.

II. From Hensingerville, Lehigh County, within 300 feet of gneiss, by Mr. Sydney Castle of the University of Pennsylvania.

III. From Allentown, Lehigh County, within 150 feet of contact of the limestone with Potsdam sandstone: by Mr. Pedro G. Salom of University of Pennsylvania.

IV. From another quarry close to III; also by Mr. Salom.

	I.	II.	III.	IV.
SiO_2	49·92	45·40	59·30	39·80
CO_2	----	----	----	14.40
Fe_2O_3	0·91	5·06	30·30	2·40
Al_2O_3	34·06	24·69		23·95
FeO	----	----	----	*trace*
MgO	1·77	13·56	*trace*	1·94
CaO	0·11	*trace*	*trace*	9·85
Na_2O	0·74	0·27	1·51	0·52
K_2O	6·94	5·85	6·24	3·34
H_2O	6·52	4·80	4·70	6·00
	100·97	99·63	102·05	102·20

As typical damourite contains 11·77 p. c. of potash, it is evident from the above analyses that I. contains 55·40 p. c. of damourite; II, 49·70 p. c.; III, 53·02 p. c.; and IV, 28·30 p. c.

The remainder of the slate consists of ferruginous clay, quartz, and carbonates of lime and magnesia.

I have noticed that as a rule the fresh damourite-slate can only be observed where a mine is being worked; when fresh it is white to straw-yellow in color, has a greasy feel and very slaty texture, being composed of minute crystalline plates. As soon as work is stopped in a mine the slate commences to decompose and becomes rapidly converted to a white or yellow clay, the latter color being due to oxide of iron. Indeed, in many mines no slate can be observed at all, the whole of it being changed to clay prior to the opening of the pits.

I have made a qualitative examination of both white and yellow clays from the same mine as I, and close to the spot where the slate was obtained. I found them to contain the same ingredients as I.

That the damourite-slate belongs to the Calciferous and not to the Laurentian or Huronian periods, is evident from the fact that it is found forming a bed in the limestone immediately overlying the Potsdam sandstone and conformably to the latter. The ore is always found above the clay or damourite-slate, or at least in the upper portions of it, *never* below it; and usually in streaks or masses precisely in the manner noticed by Prof. W. P. Blake* at Ocoya Creek, California; and from the manner in which the ore occurs it appears to have been deposited by the percolation of waters containing iron in solution. The ore also occurs as so-called "bombshell ore," and this ore when hollow either contains water or the clay resulting from the decomposition of the damourite-slate. The interior surface of this bombshell ore is frequently glazed, apparently by a thin coating of manganese oxides, and often contains small stalactites, the ore clearly indicating its aqueous origin, and its formation *subsequent* to the rock or clay inclosing it.

So-called pipe ore is also found,—though more rarely in this district—but under different conditions, being always found underneath a bed of limestone, and evidently formed by the percolation of ferruginous waters through this bed. I have never seen the pipe ore in large quantities at any one point or associated with the slate or clay.

If this occurrence of the damourite-slate or clay and ore was merely local, it would be scarcely worth noticing; but in fact we find that the slates or clays are almost universally associated with the iron ores of the Calciferous epoch. They are therefore of the greatest importance.

Professor Dana, in a private letter to me, mentions the occurrence of slates with the brown hematite deposit of Richmond, Berkshire County, Massachusetts. Professor Shepard,†

*** See Pacific R. R. Reports, vol. v, p. 168.**

† A Report on the Geol. Survey of Connecticut, by C. U. Shepard, p. 20.

in writing of the ore bed at Kent, Connecticut, speaks of decomposed micaceous gneiss (?) called by the workmen "grey fuller's earth," and also of "decomposing quartzy mica-slate." We meet with this association of slate or clay and ore in Lehigh County, Penn.

Professor Lesley also speaks of this occurrence in his "Report on the Brown Hematite Deposits of the Nittany Valley, Penn." Here there are apparently *three* lines of mines, to judge from the map accompanying the report. Professor Persifor Frazer, Jr., informs me that he found the same association of white clay with ore in York County, Penn.

Professor Lesley [†] gives a sketch of what he calls "ore-bearing slates" lying between the Potsdam and Calciferous of the Cumberland Valley. These are evidently damourite-slates, for he proceeds in the article to speak of white clays formed by their decomposition. The same association is found in Maryland and Virginia in the valleys forming the Great Valley.

Professor Lesley[*] speaks of the occurrence of white clay with ore in the same formation at Embreeville, East Tennessee. It also occurs at Shelbyville. Alabama.

It is therefore evident that the occurrence of this slate or clay with the brown hematite ores of the Calciferous epoch is the rule, its absence the exception. As Professor Lesley[†] says: "They (the ores) never occur in any other relationships. They are the same ores, in the same rocks, arranged in the same way, the whole distance from Massachusetts to Alabama. Consequently what is geologically true of the ore beds at Salisbury and Amenia, east of the Hudson, is true all the way to Alabama."

Formerly the slates probably covered the sides of the mountain ranges now forming their south boundary, resting on crystalline limestone, which separated them from Potsdam sandstone. On account of the large amount of carbonate of lime which the slates probably contained and the ease with which they were converted to clay, they have suffered the most in the erosion which has taken place. My own experience has been that the slate and clay occur under the ore, and *not* the reverse.

From this constant occurrence of the slate with the ore it is evident that the former has exerted an important influence on the deposition of the ores; and I think that the presence of the slate was necessary to such deposition, in that it formed an *impervious bed through which the chalybeate waters could not percolate.* Whatever may have been the manner in which the ore originally existed, it was deposited in and above the slates from an aqueous solution. Under these circumstances, it will be highly improbable that we shall find continuous deposits of the brown hematite ores when the slate or clay does not occur with them.

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[*] Lesley, "The Iron Ores of South Mountain in Cumberland Co., Penn.," in Proc. Amer. Phil. Soc., Jan., 1873.

[†] Proc. Amer. Phil. Soc., May, 1872.

With respect to the quantity of ore, Professor Lesley, in his paper on the Cumberland Valley, says it will depend: "First, on the original charge of iron in the strata; second, on the dip of these strata; and third, on the depth beneath water-level to which the mouldering decomposition of the strata and the peroxidation and concentration of the iron has extended."

There is much difference of opinion as to how these deposits of brown hematite ores can have been formed. Dr. Hunt, in a paper read before the National Academy of Sciences in November, 1874, gives it as his opinion that they were once beds of pyrites in *Huronian* schists, now decayed. I have shown in the commencement of this paper that they are *Calciferous* slates or schists; and were the ores formed by the alteration of pyrites *in situ*, we should find kernels of iron-pyrites which had escaped alteration, especially in the deepest mines. This is not, however, the case. Iron pyrites are found very rarely; as, for example, in Breinig's mine near Allentown, and one of the Glendon Iron Company's mines near Easton; and these mines are not the deepest we have, the former being quite shallow.

Professor Lesley* thinks the ores "were deposits *in loco originali* of the iron (as hydrated peroxide) set free from the limestone or dolomite rocks during their gradual erosion and dissolution;" agreeing in this respect with Dr. R. M. S. Jackson, who made the first geological survey of the Nittany Valley ore beds in 1838 or 1839. Of course, only the carbonates of lime and magnesia would be dissolved; the clay, iron and silica would remain behind. Were this, however, the case, should we find *fresh*, *undecomposed* damourite-slate conformably in position with limestones above and below it, as can be seen at the Lehigh Iron Company's limestone quarry at Allentown, Penn.? From what I have seen in Lehigh, Northampton and Berks Counties, Penn., I have been led to the following conclusions, which I advance with considerable reluctance, as I consider it extremely dangerous to argue from observations within a limited area to general conclusions.†

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* Lesley, Report on the Brown Hematite deposits of the Nittany Valley.

† Prof. C. U. Shepard, in his Report on the Geological Survey of Connecticut, published in 1837, says: "The origin of limonite in these rocks ['mica slate, micaceous gneiss or quartz-rock'] may be attributed to the decomposition of the sulphuret of iron and other ferruginous minerals with which they are known to abound. It is obvious also that, in a majority of instances, this change took place in the original repositories of these minerals; since no perceptible derangement is discoverable in the layers of the ore-bed, or want of conformity in them to the adjacent rock." This view, adding limestone to the rocks, has been found by Dana to accord fully with the various facts in the limonite region of Western Connecticut and Massachusetts.—EDS.]

The damourite-slates, being the products of the decomposition of primary rocks containing orthoclase and probably oligoclase, or albite, were deposited in the Silurian sea during the Calciferous epoch and near its commencement. At Allentown and Bethlehem, Penn., the slates are but 50 to 100 feet above the Potsdam sandstone, with a blue massive limestone, generally crystalline, between the two. Limestone was again deposited above the slate. I am not yet prepared to say whether more than one bed of damourite-slate was formed, and reserve an opinion on this point until I have made further explorations.

The brown hematites were probably formed by the oxidation of iron pyrites, but the former are not in the same place that the latter were. My reasons for this assertion are twofold. In the first place, the animal carbon of the organisms, whose shells formed the enormous quantity of dolomite or limestone, exists in such quantities throughout the limestone as to color the rock, the zinc blende (of Friedensville) and the carbonate of iron a bluish-grey. This same carbon would readily reduce the sulphate of iron carried into the ocean to iron pyrites. As a proof of this reduction I need only mention that iron pyrites have been found in the mud* of a pond. Secondly, the great majority of the brown hematites which I have had analysed, and they are many, contain a *trace* of sulphur, usually not more than a few hundredths of a per cent. I have also found minute (almost microscopic) crystals of iron pyrites in much of the limestone, where it is opened in quarries; and I have also seen in much of this same limestone, where weathered, minute cavities, which I have ascribed (perhaps erroneously), from their general appearance, to the decomposition of the pyrites.

It is at present impossible to say whether the pyrites from which the brown hematite ores were and are forming were thus minutely disseminated through the limestone, or whether there was a bed of the limestone—now decomposed,—especially rich in the pyrites.

In either case it would seem that the pyrites above water-level—and we must bear in mind the great erosion which the surface of the country has undergone since it was formed—would oxidize from the action of the water and the air carried in by it, forming protosulphate of iron. This being readily soluble in water, was carried down through the limestone, forming sulphate of lime (gypsum) and carbonate of iron wherever the solution came in contact with the fresh limestone. This reaction was, however, probably slight, owing to the rapid descent of the solution in seeking the water-level. It experienced no difficulty in its descent until it came in contact with the damourite-slate, when, meeting with an impervious bed, it could not descend any farther and was then obliged to follow the slates. This considerably retarded the rapidity of its flow, permitting the reaction between the limestone (and the carbonates of lime and magnesia in the damourite-slate) and protosulphate of iron to take place more completely. By this means carbonate of iron was deposited, being but slightly soluble; while the sulphate of lime (gypsum) formed, being very soluble, was carried off in solution.

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* Gmelin—Kraut's Handbuch der Chemie, 6th ed., vol. iii, p. 333.

No doubt many of the limestone caves—so common in the Calciferous limestones—were formed by the dissolving action of the water containing protosulphate of iron. The subsequent formation of the limonite is easily explained, being merely the oxidized and hydrated product of the carbonate of iron.

In fact, a bed of blue carbonate of iron, identical in appearance with the ordinary limestone and Friedensville zinc-blende, occurs with the brown hematite ores at Balliet's mine near Allentown and at one of the Hellertown mines near Bethlehem. It does *not* occur at many of the mines in the Lehigh Valley; those being the only mines where I know of its occurrence. The very fact of this occurrence would tend to show that the above-mentioned reaction has taken place and was the one by which the ore was formed. This reaction would also explain why the brown hematites which occur in limestone are almost always free from sulphur, merely containing a trace; and yet almost always containing *this trace.* As a proof of this, I give the analysis of a brown hematite which occurs at Katahdin Furnace, Piscataquis County, Maine. The analysis was made by Professor T. M. Drown of Lafayette College. It afforded

Fe_2O_3	H_2O and organic matter	SiO_2	H_2SO_4	P_2O_5
76·87	19·25	0·71	3·10	0·10=100·03

Professor Drown informs me that the rock underlying the ore was siliceous in character, thus giving the sulphuric acid no opportunity to combine with lime and be carried away.

The objection may be raised to this view of the case, that we do not anywhere find any deposits of gypsum. It must be borne in mind that gypsum is readily soluble, and would be carried a considerable distance in solution before being precipitated. I can also point to one very considerable deposit of gypsum in this formation; that of Saltville in Russell County, Virginia.

It may be urged—and I confess with much show of reason—that the iron was first present in the limestone as carbonate. As one example of this may be adduced the following instance, for which I am indebted to Professor Dana. "In one of the brown hematite mines at Richmond, Berkshire County, Massachusetts, there was exposed, about fifteen months ago, a great mass of massive carbonate of iron, which was once a part of the limestone of the region. It appeared to have resisted decomposition in consequence of its compactness and purity. There is limonite of great thickness all about it, which has been made by the decomposition of the limestone and its included carbonate of iron, or its carbonate of iron and lime; for an extensive ledge of limestone rises above the great pit or excavation on its north side, whose layers are conformable to that of the massive carbonate of iron in the bottom of the mine. The material between the two must have originally been calcareous: it is now gone and there is limonite in its stead."

The fact here mentioned tends to prove that the limonite was probably originally present as carbonate of iron and that the outer and possibly less compact portion of the deposit became changed.

It seems to me that in this instance, as in those at Balliet's mine and Hellertown, we have a case of alteration of the limestone to carbonate of iron particle by particle, or, so to term it, "a pseudomorph by replacement."

My objection to the original deposition of the carbonate of iron, as such, in the Calciferous epoch is threefold. In the first place, it should be uniformly distributed through the limestone and not confined merely to the horizon just above the damourite-slates. Then there should not be even the *trace* (the few hundredths of a per cent) of sulphur in the brown hematites, which we so uniformly find. And finally, there seems to be a lack of cause, such as exists in the case of the carbonates of lime and magnesia. For these were secreted by the marine animals to form their shells, &c., which would not be the case with carbonate of iron.

In conclusion, I would once more point to the fact that whatever the origin of brown hematites may have been, the cause of their deposition, where they are now found, must have been the damourite-slates.

Lafayette College, Easton, Pa., Feb. 22, 1875.

ART. XLVII.—*Note on some New Points in the Elementary Stratification of the Primordial and Canadian Rocks of South Central Wisconsin;* by ROLAND IRVING.

THE order always hitherto accepted for the Lower Silurian strata of Wisconsin has been as follows, beginning below:

I. *The Potsdam Sandstone*—500–700 feet in thickness; in its upper portions somewhat dolomitic; fossiliferous in localities near the Upper Mississippi and St. Croix Rivers.

II. *The Lower Magnesian Limestone*—200–250 feet in thickness; chiefly a pure dolomite, and almost without fossils.

III. *The St. Peter's Sandstone*—80–100 feet in thickness; for the most part a coarse quartzose sandstone: at times with ferruginous cement, at others loose and incoherent.

IV. *The Blue and Buff Limestones.*

V. *The Galena Limestone.*

VI. *The Cincinnati Group.*

During the past season I have been able to make some modifications in this scheme, as far as the Potsdam, Lower Magnesian, and St. Peter's are concerned, for the region of south central Wisconsin, especially in Dane and Columbia Counties. The succession now made out is as follows, beginning below:

I.	The Lower or Potsdam Sandstone	800	feet.
II.	The Mendota Limestone	30	"
III.	The Madison Sandstone	35	"
IV.	The Main Body of Limestone	80–120	"
V.	The St. Peter's Sandstone	80–100	"
VI.	The Buff and Blue Limestones.		

I. *The Potsdam Sandstone.*—In the region of the "Four Lake Country," about Madison, in Dane County, only the upper layers of this great formation are to be seen. Farther north, in southern Columbia County, the northerly rise of the rocks brings lower layers to view. In all this region the upper 50–100 feet of the formation are decidedly dolomitic, but nevertheless are very different and easily distinguished from the overlying *Mendota limestone.* Borings from artesian wells at Madison and Janesville go to show that some dolomitic admixture continues in the sandstone to within 200 to 300 feet of the Archæan, which is entered by two borings at Madison. The lowest layers seem, however, to be a coarse quartzose, non-dolomitic material. The dolomitic admixture in the upper layers amounts to 15 or 20 per cent. In the Janesville well there was some indication of the existence of a distinct layer of limestone at a very considerable depth in the Potsdam. *Scolithi* are the only indications of life.

II. *The Mendota Limestone.*—This limestone has everywhere in the region around Madison, and in fact wherever recognized, very marked characters. It has always a peculiar yellowish, dirty appearance, and is frequently stained by reddish patches and seams of non-hydrated oxide of iron. Layers of green sand occur, especially about the junction with the underlying sandstone. This limestone is usually heavy-bedded below and thin-bedded to shaly above. It is always to some extent arenaceous. In localities it is fossiliferous, carrying trilobites especially, and most common among them the *Dikellocephalus Minnesotensis,* or some very similar form. The fossil locality at Mazomanie, on the west side of Dane County, which is alluded to by Hall in his monograph on the Fauna of the Potsdam Sandstone,* is doubtless at the horizon of the Mendota limestone. The name *Mendota* is given from the exposures on Lake Mendota in Dane County, where the rock was first recognized as a separate stratum.

III. *The Madison Sandstone.*—This sandstone having been first recognized in the vicinity of the city of Madison, where it is largely used as a building stone, I have thought proper to give it this name provisionally. It is in its lower portions always, and in its upper most commonly, a very coarse quartzose sandstone, consisting of very much rolled grains of limpid quartz, without admixture of dolomitic material, or with only a very slight amount. Frequently the rock is an incoherent mass of white sand. In other cases there is an admixture, or cement, of hydrated oxide of iron, the rock having a brownish hue, the oxide staining the grains of limpid quartz superficially only. In some localities, again, the upper layers are decidedly dolomitic, and then the rock becomes a building stone of considerable value. This layer of sandstone has been recognized over a wide area south and east of the Wisconsin River. The dolomitic varieties contain from ten to twenty per cent of calcium and magnesium carbonates.

IV. *The Main Body of Limestone.*—This limestone is never more than 85 feet in thickness in the region around Madison. Farther north, in Columbia County, it reaches an extreme of 120 feet. It consists of three portions, not always well defined from one another, but still distinct. These are, beginning below: (1) thin, often irregular layers, carrying near the base persistent layers of green sand, and immediately at the junction with the Madison sandstone a very peculiar siliceous oolite layer, a few inches in thickness. This oolite consists of rounded granules $\frac{1}{40}''$ to $\frac{1}{20}''$ in diameter, closely packed in a pulverulent siliceous matrix, the whole having a milky white color. The smaller granules are seen under the microscope to be single grains of limpid quartz, the larger ones being aggregations of quartz grains. This layer is astonishingly persistent, having been seen everywhere where the base of this limestone is exposed, and at points many miles apart The whole thickness of this portion of the formation is about 20 feet. (2) Very heavily and indistinctly bedded light buff limestones, breaking with a conchoidal fracture and carrying considerable chert in nodules, in thickness about 20 feet; (3) the upper layers of the formation, in general consisting of (*a*) thin-bedded layers, (*b*) very heavily-bedded layers, carrying very numerous large chert concretions and continuous chert layers, and (*c*) concretionary and brecciated layers. In none of these subdivisions have any fossils been seen.

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* 16th Annual Report, Regents of the University of the State of New York, on the condition of the State Cabinet of Natural History, Appendix D, 1863.

V. *St. Peter's Sandstone.*—This formation appears with its usual characters, except that east of Madison it presents a very marked deviation from its usual very uniform thickness of 100 feet, being reduced as low as 40 feet.

On the Mississippi River bluffs, in the vicinity of Winona and La Crosse, and again farther south, there exists, so far as now known, between the Potsdam and St. Peter's sandstone, 200–250 feet of dolomite without sandstone layers. Farther northwest, there occurs in the valley of the Minnesota River—according to Professor N. H. Winchell, State geologist of Minnesota—the following succession of strata, beginning below:

I.	The Lower or Potsdam Sandstone.	
II.	The St. Lawrence Limestone.	
III.	The Jordan Sandstone........................	50 feet.
IV.	The Shakopee Limestone	70 "
V.	The St. Peter's Sandstone.	

His description of the characters of the Shakopee, Jordan, and St. Lawrence beds shows that these strata are respectively very strikingly like my Main Body of Limestone, Madison Sandstone, and Mendota Limestone. Future investigations may show that the Jordan and Madison sandstones are somewhere continuous, in which case Mr. Winchell's names would appropriately be extended to the Wisconsin strata. So far as now known, however, the two regions in which these similar alternations occur are separated by an area where all merge into a mass of limestone whose thickness is much greater than their combined thickness.

The following schedule serves to show the probable equivalence of the strata in these different regions:

	South Central Wisconsin.	Mississippi Bluffs.	Minnesota River.
Canadian.	St. Peter's Sandstone.	St. Peter's Sandstone.	St. Peter's Sandstone.
	Main Body of Limestone, 80 to 120 feet,	Lower Magnesian Limestone, 200 to 250 feet.	Shakopee Sandstone. 70 feet.
	Madison Sandstone, 35 ft.		Jordan Sandstone, 50 ft.
	Mendota Limestone. 30 feet.		St. Lawrence Limestone.
Primordial.	Lower or Potsdam Sandstone.	Lower or Potsdam Sandstone.	Lower or Potsdam Sandstone.

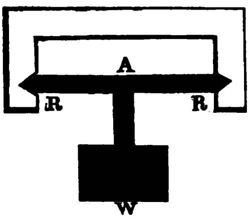
University of Wisconsin, February 13, 1875.

ART. XLVIII. – *On the application of the Horizontal Pendulum to the measurement of minute changes in the dimensions of Solid Bodies;* by O. N. ROOD, Professor of Physics in Columbia College.

[Read before the National Academy of Sciences, Nov. 4th, 1874.]

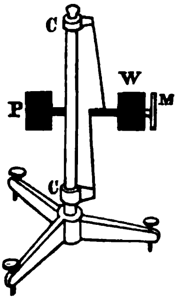
IN figure 1, let RR represent an inflexible rod of steel placed horizontally, and supported at its extremities by pivots on which it turns freely, and let W be a weight inflexibly attached to the rod as indicated. It is evident that WA when left to itself will assume a vertical position, and that the whole apparatus will constitute essentially an ordinary pendulum. If now an attractive or repulsive force be made to act on W, the pendulum will tend to be deflected from its vertical position, and if the force is sufficiently powerful a sensible deflection will be observed. In an arrangement of this kind, aside from friction, the opposing force to be overcome will of course be the attraction of gravitation; if, however, we gradually elevate the rod RR, the gravity component will diminish, and become zero when RR is vertical, and consequently AW horizontal. In his articles on the Horizontal Pendulum, Zöllner[*] has shown experimentally how this may be accomplished to an almost incredible extent, so that an apparatus of this general nature in his hands became capable of obeying even the feeble attractive force of the moon!

1.



I give here a diagram of Zöllner's horizontal pendulum; at W is the weight, the inflexible rod being replaced by fine steel wire or watch-spring, stretched as shown on the vertical column CC, the whole being supported by a tripod with leveling screws. P is a counterpoise; M a mirror for reflecting the divisions of a scale to a telescope. The height of this apparatus was about 30 inches; it was mounted on a pier like an astronomical instrument, and enclosed in a small building by itself, the observations being made from without.

2.



In the remarkable papers above referred to, Zöllner also pointed out the extreme sensitiveness of this apparatus to changes in level, and it at once occurred to me that by suitable modifications and additions, it could be converted into an instrument of unprecedented delicacy for studying minute changes in the dimensions of solid bodies.

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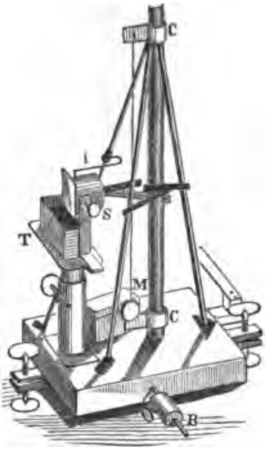
[*] Pogg. Ann. for 1873, cl, 131, 134.

Arrangement of a Horizontal Pendulum for measuring minute distances.

The apparatus constructed by me was almost entirely of brass; the dimensions were as follows: the height of the tubular column CC, fig. 3, was 350 mm.: distance *ab*, fig. 4, 5 mm.; distance *a*W, 58 mm.; weight of pendulum, 137 grams, nearly. The whole was supported on a well braced and firmly constructed wooden stand, which was hung by its upper end on the brick walls of the college-building, and again attached to them below, so as to prevent vibrations independent of the walls of the building itself.


3.

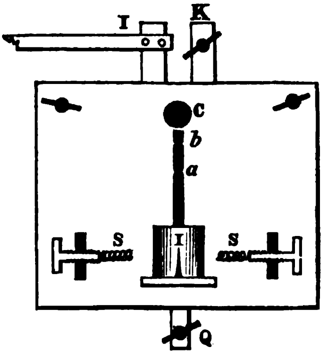
An inspection of Zöllner's apparatus shows that its form was such as to render the column CC, fig. 1, quite liable to vibrations of its own; this I guarded against by the addition of three long and the same number of short cross-braces, as shown in fig. 3; they were found to make a most marked change for the better, and gave the instrument a stability which it previously did not possess.

Again, the German astronomer provided no means for bringing his instrument to rest, and in his published observations this actually never was the case, the extremes of the oscillations being employed for ascertaining the position of the pendulum. As this mode of taking readings was, in my case, owing to the locality and nature of the work, wholly out of the question, it became necessary to supply this defect. Very good results were obtained simply by attaching to the pendulum a wire bent in a series of zig-zags, and immersed in a box filled with olive-oil. By removing with the pliers successive portions of the wire, it was found that matters could be arranged so that the instrument came to rest after a couple of oscillations. The oil-box rested on a little table provided with rack-work to adjust its height and remove it when necessary; it is shown at T in fig. 3; its dimensions were as follows: length 70 mm., breadth 30 mm., depth 32 mm. The wire was placed of course as near the center of the box as possible, and the effects of capillarity were not sensible. The above arrangement is the one actually employed in making the experiments detailed below, but I have some reason to suspect that, owing to its viscosity, there was a tendency on the part of the pendulum, *after it had once come to rest*, to obey impulses from without more tardily than was quite right. To obviate this possible difficulty I afterward provided the pendulum with a paddle instead of a wire, and caused the former to move loosely like a piston in the interior of a suitable box, which was entirely submerged in *kerosene* oil; the results were very promising, but I have not had time to use this method in an extended set of experiments.

At SS, figs. 3 and 4, are stops which confine the motion of the pendulum within narrow limits; they consist of spiral springs, which are fastened in the prolongation of adjusting screws. The springs form an essential part of this indispensable arrangement, and without their aid it was often found impossible to complete the leveling, so as to give the instrument even a moderate degree of sensitiveness.

At I, figs. 3 and 4, is shown an index or pointer, intended to facilitate the adjustment of the pendulum with regard to the scale, and readily bring it into the zero of position. It makes a very convenient addition, and is observed by a lens placed outside of the glass case enclosing the whole apparatus.

4.



I may here remark, that the above-mentioned additions ought hereafter to be made to the horizontal pendulum, even when constructed solely for astronomical purposes, the oil-box being replaced by an air-box, contrived so as to reduce the tendency to oscillate. Compare Töpler, "Ueber einige Anwendungen der Luftreibung bei Messinstrumenten," Pogg. Ann., 1873, xlix, 416.

Mode of suspension.—In the first experiments fine iron wire was employed, but it was soon found that its presence rendered accurate magnetic observations impossible; and besides, although it was as fine as was consistent with safety, its torsion was noticeable. I afterward replaced it with narrow strips of elastic copper-foil, such as is used by jewelers, the ends being soldered to small thin plates of brass to prevent tearing, and these again connected with fine, hard-drawn brass wire. The copper-foil was adopted merely as a temporary expedient, but as it was found to answer well, and to be free from sensible torsion, it was retained. Experiments were made beforehand on its strength, and it was loaded up to within about two-thirds of its breaking strain.

At M, fig. 3, is a milled head for winding up the brass wire attached to the lower copper strip and thus bringing the pendulum into a horizontal position, i.e., its real object being to adjust the mirror of the pendulum with regard to the scale and telescope

Leveling screws.—These were made of brass with eighty threads to the English inch, the two rear screws being at a distance of only two inches apart. The front screw and one of the rear screws rested on plates of brass or glass, which were properly countersunk and cemented in a level position on the shelf. They were provided with the easily removable lever-arms 70 mm. long, and the leveling was always completed by giving gentle taps to these arms, with some light rod such as a lead-pencil.

Micrometer.—The left-hand rear screw, I, fig. 4, was used as a micrometer; it rested on a small leveled plate of glass and was provided with an arm 140 mm. in length, which was capable of moving over $\frac{1}{4}$ or $\frac{1}{8}$ of a degree of arc between two fixed points. To the extremity of the arm were attached two threads, one of which reached directly to the observer, while the other first passed over a small pulley; the threads were retained in a horizontal position by proper supports, and served to turn the micrometer-screw to and fro through some known fractional part of a degree. The distance traversed by the end of the lever-arm was measured with a compound microscope placed over its extremity; the length of this arm being known, and the number of threads of the screw, the actual upward motion could be calculated.

At B, fig. 3, is a counterpoise, its object being to remove most of the pressure from the micrometer screw.

Scale and Telescope.—A glass scale divided into millimeters was used; the telescope magnified about 60 diameters: the lines of the scale appeared to the observer, when referred to a distance of ten inches, to be about eight mm. apart, so that there was no difficulty in estimating tenths of a division.

Sources of error.—The main difficulties encountered in making the observations given below may be divided into two classes, viz: those due to accidental vibrations, and those dependent on changes of temperature. The apparatus, as may be imagined, was exceedingly sensitive to vibration, and the locality used by me, in this respect, one of the most unfavorable in this country; for, apart from carriages and street-cars, locomotives and trains of cars passed at a distance of 300 feet continuously during most of the day and the greater part of the night. Hence, in spite of the oil-box, the pendulum was never completely at rest, except for three or four hours after midnight: during the day-time the vibrations were often so rapid as to render the scale-divisions invisible. Still, in spite of these disturbing causes, an extended series of experiments has shown that the inherent stability of the apparatus is such that these external influences produce, during a moderate interval of time, little or no permanent change in the instrument, and their effects can consequently be eliminated by making a sufficient number of observations. To illustrate the delicacy of the apparatus, I may remark that children playing on an iron bridge, 360 distant, caused temporary deflections of one or two divisions, and that similar deviations were caused by the lower notes of an organ in a neighboring church, the medium and higher tones producing no sensible effect. It was found. however, necessary to guard more sedulously against disturbances originating in the building itself, as they were more apt to produce permanent effects; this evil, however, practically only subjected the observer to temporary interruption.

Changes of temperature produce two different kinds of effect on the apparatus; in the first class we may place those which would be compensated by turning the leveling screw Q, fig. 4; in the second, those that would be neutralized by turning either of the other leveling screws. It is evident that changes of the first class alter the sensitiveness of the apparatus or the value of a scale-division, but on account of the comparatively large distance between IK and Q, these effects are small, and may be rendered almost insensible by attention to constancy of temperature: thus far they have not proved a source of annoyance.

The case is different with the second class; the leveling screws I and K being separated only by a distance of 50 mm., it is necessary, on their account mainly, to preserve the temperature of the apparatus as constant as possible. The means of accomplishing this I shall consider in the second part of this article, taking up in the present the method of eliminating errors that are introduced from this source. It is evident that changes of the kind now under consideration will communicate to the pendulum a continuous motion toward the right or left, and will finally drive it to the stops, and that all the readings will be affected with a constant or changing error. If the motion is so slow that the operation of reaching the stops consumes an hour or even less, it is not a source of serious inconvenience, and the errors thus introduced can easily be eliminated.

General mode of experimenting.—In all cases the micrometer screw rests directly or indirectly on the body the change in whose dimensions is the subject of study, and the first proceeding will naturally be to ascertain whether the different portions of the apparatus are at rest relatively to each other, or approximately so. Afterward the value of a scale-division can be obtained by repeatedly moving the arm attached to the micrometer screw, by the aid of the threads which reach to the observer seated at the telescope. When this has been satisfactorily accomplished, the body to be experimented on is subjected to the desired influence, and the change in its dimensions noted; for example, the change in the longitudinal dimensions of a bar of iron when magnetized produces with this instrument a large sudden deviation, and it is also possible to note the gradual increase in its dimensions, owing to the heat developed by the act of demagnetization, and I am at present engaged in studying with it the effect of magnetization on the *lateral* dimensions of iron bars; in short, the instrument as now arranged is applicable to the solution of a great number of delicate problems.

Mode of making and calculating the observations.—The best way of observing, whenever possible, is to make a continuous connected series of readings, so that the second reading is equally related to the first and third, the third equally to the second and fourth, etc. When this proceeding is used it is possible to obtain correct results, even though the pendulum have an independent motion of its own. To take a simple example, let us suppose that the object required is to determine the value of a scale-division, and that by the aid of the micrometer-screw a series of continuous readings have been obtained. We have then to consider four principal cases:

1st, *The pendulum had no independent motion of its own.*—In this case it is of course a matter of indifference what method is employed in working up the results; i.e., whether the readings are treated in independent pairs or as a continuous series.

2nd, *The pendulum has a uniform motion of its own.*—Call the actual readings A, B, A′, B′, A″, B″, etc.; let x = the true quantity, and let the motion be positive or tend to increase the readings; d = the distance passed over during the interval from one reading to the next following; let d be positive, and B, B′, B″, represent the larger readings. We then have for the readings:

$$\begin{aligned} A &= A \\ B &= A+x+d \\ A' &= A \quad +2d \\ B' &= A+x+3d \\ A'' &= A \quad +4d \end{aligned}$$

Then as the readings are continuous, subtracting the first from the second, the third from the second, the third from the fourth, the fifth from the fourth, etc., we obtain a set of differences alternately too large and too small, thus:

$$\begin{array}{ll}
\text{Differences.} & \text{M}'. \\
\left.\begin{array}{l} B-A\ =x+d \\ B-A'\ =x-d \end{array}\right\} & \ldots\ldots x \\
\left.\begin{array}{l} B'-A'\ =x+d \\ B'-A''=x-d \end{array}\right\} & \ldots\ldots x
\end{array}$$

The average of each pair of differences will give the true quantity, x, free from errors introduced by motion. In the case now under consideration the average of the column M′ will be equal to the average of the column differences, so that the final result will be identical whether we employ the average of one or the other, but by employing the column M′ we have before us the identical observations free from contaminations due to the independent motion of the pendulum, thus enabling the observer more readily to judge of the reliability of the observations, and to calculate their probable error.

2. *The distance passed over independently by the pendulum increases during each observation by a constant quantity.*—Retaining the same notation, etc., as before, we have:

$$\begin{array}{lll}
 & \text{Readings.} & \\
A & =A & =A \\
B & =A+x+d & =A+x+d \\
A' & =A+d+2d & =A\quad\ +3d \\
B' & =A+x+d+2d+3d & =A+x+6d \\
A'' & =A+d+2d+3d+4d & =A\quad\ +10d \\
B'' & =A+x+d+2d+3d+4d+5d & =A+x+15d
\end{array}$$

Taking, then, the differences according to the method above indicated, we have:

$$\begin{array}{llll}
\text{Differences.} & \text{M}'. & \text{M}''. & \text{M}. \\
\left\{\begin{array}{l} x+d \\ x-2d \end{array}\right. & & & \\
 & x-\dfrac{d}{2} & x+\dfrac{d}{2} & x \\
\left\{\begin{array}{l} x+3d \\ x-4d \end{array}\right. & & & \\
 & x-\dfrac{d}{2} & x+\dfrac{d}{2} & x \\
\quad x+5d & & &
\end{array}$$

The column marked M′ is obtained by taking the average of pairs of differences as indicated by the vinculums on their left-hand side; the column M″ by using those on the right; the final column M by combining in pairs the quantities under M′ and M″. It is evident that the final column of means, M, will in this case give the correct value, or x, and that the average of the column of differences will be incorrect by $-\dfrac{d}{2}$ if an even number of readings be used; and if an odd number be employed, by $+d$, $\frac{2}{3}d$, $\frac{3}{5}d$, $\frac{4}{7}d$, etc.

3. *The distance passed over independently by the pendulum during each observation increases by an increasing quantity.*—This case may perhaps be most quickly illustrated by a numerical example arranged algebraically, the changes introduced being far larger than ever would be met with in practice.

Let the true number, x, equal 30; the first reading equal 25; we then have:

$$\begin{array}{llllll} & & & & & \textbf{Readings.} \\ 25 & & & & = & 25 \\ 25 + 30 & & & + 1 & = & 56 \\ 25 & + & 1 + & (1 + 2) & = & 29 \\ 25 + 30 & + & 4 + & (3 + 3) & = & 65 \\ 25 & + & 10 + & (6 + 4) & = & 45 \\ 25 + 30 & + & 20 + & (10 + 5) & = & 90 \\ 25 & + & 35 + & (15 + 6) & = & 81 \\ 25 + 30 & + & 56 + & (21 + 7) & = & 139 \end{array}$$

Differences.	M′.	M″.	M.
31			
27	29·	31·5	30·25
36			
20	28·	32·5	30·25
45			
9	27·	33·5	30·25
58			

It is evident in this case, that even the final mean will be affected with a small constant error; still for practical purposes this difference from the correct quantity will be far below the unavoidable errors of observation.

As the method with three columns applies to an increasing rate, it is evident that it will apply equally well to one which diminishes, and it hence follows that if the motion decreases till it is zero, and then begins to increase, being affected with the opposite sign, its effects will still be eliminated by the proceeding above described.

I proceed now to give some examples to show the character of the results that can be expected from the horizontal pendulum under very unfavorable conditions, i. e., when exposed to vibration and changes of temperature. All the observations given below were made in the day-time, the quietest portions being selected when comparatively few locomotives and trains of cars were passing; no extraordinary precautions with regard to temperature were employed, the pendulum not being enclosed except in its glass case. All the observations are in the form of determinations of the value of a scale-division, a proceeding well calculated to illustrate the capabilities of the instrument. The first four constitute a coherent series, the same adjustment of apparatus being used in them all; the others are independent sets made with different adjustments, and intended to illustrate the effects of change of temperature, etc.

Determinations of the value of a scale division.

In No. 1, after using the micrometer-screw, five readings were taken consecutively, the interval between them being only that required for making the record; in Nos. 2 and 3 single readings only were taken, but each reading is to be regarded as a mental average of at least two or three readings; in No. 4 twenty-five consecutive readings were taken on each occasion. The best mode, on the whole, seems to be to take in each case not less than five or more than ten rapid readings, and then again to use the micrometer-screw.

No. 1.

Actual readings.					Average.	Difference.
54·2	53·2	53·			53·4	27·8
81·5	80·9	81·6	80·8		81·2	27·8
53·5	55·	53·4	53·	52·	53·4	29·2
83·	82·3	83·	83·	82·	82·6	29·0
56·3	54·2	54·	51·6	52·	53·6	28·1
82·	81·	83·	81·3	81·5	81·7	29·6
53·2	52·7	51·	53·5	50·2	52·1	29·4
80·3	81·3	82·8	82·	81·2	81·5	28·2

Although in these observations the temperature seems to have been constant, yet for the sake of uniformity I have preferred to treat them according to the method above described.

M′.	M″.	M.
27·8	28·5	28·15
29·1	28·5	28·80
28·8	29·5	29·15
	Final average,	28·7

The probable error is 0·198 of one scale division; which, with the adjustment then employed, corresponds to $\frac{1}{17,300,000}$ of an English inch.

No. 2.

Actual readings.	Diff.	M′.	M″.	M.
21·	29·	28·	28·25	28·12
50·	27·			
23·	29·5	29·5	30·25	29·87
52·5	29·5			
23·	31·	31·75	30·25	31·
54·	32·5			
21·5	28·	26·75	26·	26·37
49·5	25·5			
24·	26·5	27·	27·9	27·45
50·5	27·5			
23·	28·4	27·75	28·5	28·12
51·4	27·1			
24·3	29·9		Final average,	28·48
54·2				

The probable error is 0·461 of a scale division, which corresponds to $\frac{1}{7,486,000}$ of an English inch.

No. 3.

Actual readings.	Diff.	M′.	M″.	M.
68·	26·	29·	30·25	29·62
42·	32·			
74·	28·5	29·25	30·1	29·67
45·5	30·			
75·5	30·2	32·2	32·1	32·15
45·3	34·2			
79·5	30·	29·55	29·70	29·62
49·5	29·1			
78·6	30·3	29·15	26·9	28·02
48·3	28·			
76·3	25·8	28·8	30·9	29·85
50·5	31·8			
82·3	30·	30·1	28·85	29·47
52·3	30·2			
82·5	27·5		Final average,	29·77
55·				

The probable error is 0·31 of a scale division, corresponding to $\frac{1}{11,300,000}$ of an English inch. It is to be remarked that while these last readings were being taken three heavy trains passed.

No. 4.

Av. of 25 read.	Diff.	M′.	M″.	M.
75·04	29·54	29·45	29·27	29·36
45·50	29·37			
74·87	29·17	27·93	29·75	28·84
45·70	26·70			
72·40	32·80		Final average,	29·10
39·60				

The probable error is 0·16 of a scale division, corresponding to $\frac{1}{21,950,000}$ of an English inch.

Combination of results, 1, 2, 3 *and* 4.

It is evident that the weights of these results are quite different; accordingly after taking all things into consideration, I assigned to Nos. 2 and 3 single weights respectively, to No. 1 a double weight, and to No. 4 a three-fold weight. Under these conditions the average of the four sets of observations is 28·99, with a probable error of $\frac{1}{10}$ of a scale division, corresponding to $\frac{1}{36,254,000}$ of an English inch.

In Nos. 1, 2, and in the first four readings of No. 4, there is no decided evidence that the pendulum had any independent motion of its own; in No. 3, its independent motion seems to have been nearly uniform.

I give now a case where the pendulum was affected with a rapid uniform motion: each reading given below is the average of ten rapid readings.

No. 5.

Readings.	Diff.	M′.	M″.	M.
101·5	23·1	33·4	32·3	32·8
78·4	43·7			
122·1	21·0	31·2	31·76	31·48
101·1	41·4			
142·5	22·12		Final average,	32·14
120·38				

The probable error is 0·44 of a scale division, corresponding to $\frac{1}{8,850,000}$ of an English inch.

No. 6.

Here each reading is the average of twenty-five rapid readings: the sign of the motion changed during the observations.

Readings.	Diff.	M′.	M″.	M.
92·5	53·36	48·81	47·82	48·31
39·14	44·26			
83·40	51·38	49·72	46·13	47·92
32·02	48·07			
80·09	44·19	48·14	45·35	46·74
35·90	52·10			
88·	38·60		Final average,	47·65
49·40				

The probable error is 0·31 of a scale division, corresponding to, $\frac{1}{12,500,000}$ of an English inch.

When it is recollected, that with the best optical and mechanical means, it has hitherto been hardly possible to measure quantities smaller than $\frac{1}{200,000}$ of an English inch, the field which the use of the horizontal pendulum, even under very unfavorable circumstances, opens, will be easily appreciated.

Art. XLIX.—*Contributions from the Sheffield Laboratory of Yale College* No. XXXIII.—*On Diabantite, a chlorite occurring in the trap of the Connecticut Valley;* by George W. Hawes.

In my recent article upon the trap rocks of the Connecticut Valley,* it was shown, by means of analyses, that these rocks are of two kinds: 1st, the anhydrous dolerite; 2d, the chloritic dolerite, or diabase, which was formed from the same original melted mass as the dolerite by a process of hydration. The diabase is often amygdaloidal, and the cavities are filled with various minerals, among which the most common are carbonate of lime, silica and chlorite; the two former substances being naturally produced in the change of the bisilicate, pyroxene, to the unisilicate, chlorite. At one point in the Farmington hills these amygdaloidal cavities are of very large size, often larger than an egg, and in them, with the quartz and calcite, there are large compact masses of chlorite, possessing a foliated and radiated structure and a dark green color. This association of minerals, which has often been recognized elsewhere, seemed to indicate that this was the chlorite which was distributed through the diabase, and an analysis was therefore made to determine to which member of the chlorite group it belonged.

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* See this Journal, III, ix, 185, March, 1875.

A thin section of one of the masses under the microscope appeared to be perfectly homogeneous, and exhibited a very marked and beautiful dichroism. From a cavity in which the chlorite had thus been shown to be pure, the material was carefully selected for analysis. Its specific gravity was found to be 2·79; its hardness, 1; and its fusibility 3. The composition obtained was as follows:

	I.	II.	Mean.	Oxygen.	
Silica,	33·23	33·25	33·24	17·73	17·73
Alumina,	11·13	11·02	11·07	5·16	5·83
Ferric oxide,	2·37	2·15	2·26	·67	
Ferrous oxide,	25·09	25·12	25·11	5·58	12·66
Manganous oxide,	·40	·42	·41	·09	
Lime,	1·07	1·15	1·11	·32	
Magnesia,	16·48	16·54	16·51	6·60	
Soda,	·25	·25	·25	·07	
Water,	9·89	9·93	9·91	8·81	8·81
	99·91	99·83	99·87		

This analysis gives for the oxygen ratio of the protoxides, sesquioxides, silica and water, 4.3 : 2 : 6 : 3, showing that it approaches very closely to a unisilicate in its composition. In order to ascertain whether this composition remains constant, the analysis was repeated upon another sample selected with like care from another cavity; the following were the results:

	I.	II.	Mean.	Oxygen.	
Silica,	33·65	33·70	33·68	17·96	17·96
Alumina,	10·93	10·75	10·84	5·05	5·90
Ferric oxide,	2·95	2·77	2·86	·85	
Ferrous oxide,	24·24	24·43	24·33	5·40	12·39
Manganous oxide,	·38	·38	·38	·09	
Lime,	·72	·73	·73	·21	
Magnesia,	16·56	16·48	16·52	6·61	
Soda,	·33	·33	·33	·08	
Water,	10·04	10·01	10·02	8·89	8·89
	99·80	99·58	99·69		

This analysis does not differ essentially from the preceding, and gives an oxygen ratio still nearer to 4 : 2 : 6 : 3. This ratio may therefore be taken as the correct one, making it a unisilicate of the pyrosclerite group with the formula:

$$(\tfrac{2}{3}\dot{R}^3 + \tfrac{1}{3}\overset{\cdots}{\overline{Al}})\cdot\ddot{Si}^3 + 3\dot{\overline{H}}.$$

Although agreeing with pyrosclerite in oxygen ratio, the much greater proportion of iron appears to authorize its recognition as a distinct species.

Dr. K. L. Th. Liebe, in his article on the diabase of the Voigtland and Frankenwald,* gives analyses of a chlorite of like occurrence which he carefully selected from specimens collected at several different points, and which he named *Diabantachronnyn.* Although his analyses differ somewhat from those here given, it is evident that the material he worked upon was very similar to the above, and like it in mode of occurrence and probably the same species. But since his long name, "diabantachronnyn," which was intended, as he says, to express the idea that the mineral is the coloring ingredient of the diabase, does not convey that meaning, the Greek of the latter part of the word not signifying *color*, and since the name is not in accordance with the principles of mineralogical nomenclature, I have taken the liberty of shortening it to *diabantite*, which though not expressing all that Liebe intended to convey, still indicates its relation to diabase.

Chlorites approaching this species in composition have been often analyzed with varying results and have received various names; but the great ease with which decomposition takes place upon such material under surface action makes it easy to derive from it a great number of intermediate products. The seams and cracks of both the diabase and dolerite of this region are often filled with a soft ferruginous chlorite, but which contains varying amounts of sesquioxide of iron, and is not homogeneous. A chlorite of the kind analyzed could hardly be expected to withstand the influence of percolating waters.†

The amygdaloidal cavities afford additional evidence to that already given, proving that the hydration of these rocks was produced by vapors that gained access to the molten mass during its ascent to the surface. For these cavities often contain bitumen, which in all probability was derived from the bituminous shales of the region, and was carried up along with the other products received from the strata and the subterranean streams encountered, and deposited in the cavities. This bitumen is widely distributed in the amygdaloidal trap, as was long ago observed by Percival. The composition of the pyroxene* makes it easy to see how the change to chlorite took place under the action of these vapors. The thirteen per cent of lime was separated as carbonate, and the excess of silica was removed, while the small amount of alumina required, and which was not furnished by the pyroxene, was derived through the slight decomposition of the feldspar, which under the microscope appears to have lost its transparency.

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* Jahrbuch für Mineralogie, 1870, p. 1.

† Kenngott has endeavored to prove that Liebe's mineral was common ripidolite; but these analyses show that it has a very different oxygen ratio.

ART. L.—*The Re-discovery of the Double Star*, H I. 41; by S. W. BURNHAM.

THE double star entered as No. 41 of Sir William Herschel's Class I. was apparently not found by Struve, as it is not included in his great catalogue, *Mensuræ Micrometricæ;* nor in any of the modern lists of double stars. I have not been able to find in the various works consulted any reference to it by any observer since Herschel's discovery nearly one hundred years ago. Sir John Herschel, in the catalogue of his father's double stars (*Memoirs of the R. A. S.*, xxxv), gives the approximate place for 1880 as follows:

$$\left.\begin{array}{l}\text{R. A.} = 17^{h}\ 40{\cdot}7^{m}\\ \text{Decl.} = +\ 73^{\circ}\ 0{\cdot}6'\end{array}\right\}$$

Nothing is said by Sir William Herschel of the magnitudes of the stars, or of the distance, further than that the pair belongs to his Class I. He made two measures of the position angle:

Aug. 29th, 1782, P = 350·0°
Mch. 7th, 1783, 354·3

A few weeks since this pair was searched for and readily found with a 6-inch refractor very near the place given above. It was identified as Lalande 32725, and its place from that catalogue, reduced to 1880, is:

$$\left.\begin{array}{l}\text{R. A.} = 17^{h}\ 42^{m}\ 16{\cdot}9^{s}\\ \text{Decl.} = +\ 72^{\circ}\ 59'\ 10''\end{array}\right\}$$

I made three sets of measures of the angle, a mean of the result giving, P=340·2°. The components are of the 8th and 9th magnitudes, and separated about 1·2″, so that now it is not a difficult object even with a small aperture. A considerable diminution in the angle would seem to be beyond doubt, but further observations are necessary to determine positively whether or not this fine pair is a binary. It is about 46′ north of the well-known double star, Ψ′ Draconis, and easily found without an equatorial mounting.

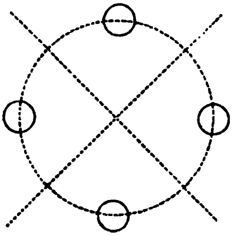
Chicago, May, 1875.

*** This volume, March, p. 187.**

ART. LI.—*Brief Contributions from the Physical Laboratory of Harvard College.* No. 17.—*On the Distribution of Electrical Discharges from Circular Disks;* by C. J. BELL.

THE peculiar figures produced by the electrification of solid dielectrics have been studied by Lichtenberg: by W. V. Bezold (*Untersuchungen über elektrische Staubfiguren*, Pogg. Ann., cxl, 145, 1870); by Prof. Kundt, Pogg. Ann., cxxxvi, 612; by Theodore Karrass, Pogg. Ann., cxl, 160, 1870; and by H. Schneebeli, Archives des Sciences Phys. et Naturelles, xlvi, 269.

If a small circular disk is connected with the inner coating of a charged Leyden jar, and is suspended vertically over a glass plate coated with lycopodium, which rests upon a metallic plate connected with the outside coating of the jar, a number of points of disturbance will be seen on the glass beneath the rim of the circular disk, as in the accompanying figure.



The following investigation was undertaken to discover the law which regulates the number of points of greatest electrical disturbance shown by the small circles in the figure. The apparatus used was arranged as follows: the telescope of a cathetometer was removed, and an insulated arm substituted for it. To this arm was clamped in a vertical position a copper wire, at the lower end of which were attached the various circular disks used in the experiments. These disks were connected with one pole of a Ruhmkorf coil. Upon an insulated tripod below the disk was placed a plate of metal, from the bottom of which ran a wire to the other pole of the Ruhmkorf coil. The dielectrics, consisting of glass plates, 17 cm. square and 1 cm. thick, rested upon the plate of metal.

Six circular disks of different diameters were used, which were placed above the glass plate, at a constant distance from it. Six experiments were made with each plate. The results are contained in the following table:

Diameter of plates.	1	2	3	4	5	6	Calculated number.
41 mm.	14	14	13	14	14	14	----
34	12	12	11	10	12	12	11·6
29	10	9	10	10	9	10	10·
20·5	7	8	7	7	8	7	7·
15·5	6	5	5	6	6	5	5·
11·25	4	5	4	5	3	4	4·

The number of discharges was closely proportional to the diameter of the circular disks, or to their circumferences. The last column of the above table was calculated on this assumption. The area of the space on the glass affected was proportional to the area of the upper plate. Although the shape of the spots on the glass was not the same in all the experiments, yet the angle between them, subtended by their arc, was constant for the same plate. Much care and patience was necessary to produce the figures. The battery which was found to give the best results consisted of one Grove cell, the platinum of which was 14 cm. by 5 cm., and the zinc 30 by 7, with an internal resistance of ·25 of an ohm. The coil would give with this battery a spark of about 4 cm.

In order to obtain perfect figures the glass plate must be very clean, and perfectly dry. The best results were obtained when the plates were heated slightly. The lycopodium dust was sifted on the plate after the discharge, produced by lifting the breaker of the coil. If it is spread over the glass before the discharge, the figures lose their sharp outline. The glass plate must be 4 or 5 cm. larger than the lower plate of metal, otherwise a spark will pass around the edge from the terminals of the coil. The distance of the upper circular disks from the glass does not appear to have any effect upon the number of figures, but if they are placed at too great distance the figures are indistinct. The circular disks must be exactly parallel to the plate of glass.

ART. LII.—*Preliminary Note on an Examination of Gases from the Meteorite of February* 12, 1875; by ARTHUR W. WRIGHT.

BY the courtesy of President Thacher, of the State University of Iowa, the writer has received some fragments of this meteorite, an examination of which has yielded some very interesting results. The meteorite is of the stony kind, containing numerous small grains of metallic iron, and not greatly differing in appearance from others of its class.

A quantity of the iron, having been separated, was found to contain several times its volume of gaseous substances, much of which it yielded on a very moderate elevation of temperature. The spectroscope plainly indicated the predominance of carbon compounds, and an analysis showed that very nearly one half the gas was made up of the two oxides of that element, the approximate percentages being; CO_2, 35; CO, 14; the two together, 49 per cent. The residue consisted largely of hydrogen, but the exact proportion was not determined. These relations show a marked difference between the iron and the stony meteorites as to their gaseous contents, as in the former the hydrogen is most abundant, while in the latter the oxides of carbon are the characteristic constituents.

The spectrum of the gases, at a few millimeters pressure, gave the carbon bands very brilliantly, the hydrogen lines being comparatively weak and inconspicuous, though at a very low pressure they became relatively stronger. The brightest carbon bands were the three in the green and blue, the red one being much feebler. Now these are precisely the ones most conspicuous in the spectra of some of the comets, and this fact is a remarkable confirmation of the received theory as to the meteoric character of these bodies. A more extended and particular account of the investigation will be given in the next number of this Journal.

Yale College, May 22, 1875.

ART. LIII. — *On Limonite with the Color and Translucency of Göthite;* by Prof. J. W. MALLET, Univ. of Virginia.

THE specimen which is the subject of this notice was received with other minerals from Capt. F. M. Imboden, of Richmond, Va., having come from the land of Dr. F. H. Griffin, near Big Lick, Roanoke Co., Va.

Passing through ordinary compact limonite there were veins of a quarter to half an inch in thickness, distinctly translucent on the edges of splinters, and having a deep blood-red color by transmitted light—by reflected light nearly black, with a luster between adamantine and resinous. Streak yellowish brown. Massive, with no traces of crystalline or fibrous structure. Hardness a little over 5. Sp. gr. = 3·76 at 15° C.

Analysis gave

Fe_2O_3	81·52	
Mn_2O_3	·11	
Al_2O_3	·26	
P_2O_5	2·38	
SiO_2	·51	
H_2O	14·95	=99·73

The water was lost at the following rate with increasing temperature:

At 100° C. (for three hours),	1·70	p. c.	
" 150° " "	·90	"	
" 200° " "	3·60	"	
" 250° " "	5·01	"	
" Red heat	3·74		= 14·95 p.c.

If 2·68 p. c. of Fe_2O_3 be deducted as combined with the 2·38 of P_2O_5 to form ferric ortho-phosphate ($FePO_4$), and the water lost at 100° (supposed hygroscopic) and the other minor constituents be neglected, we have left:

			Calc. for $Fe_4H_6O_9$ (limonite).
Fe_2O_3	78·84	85·61	85·56
H_2O	13·25	14·39	14·44
		100·	100·

The assumption of the P_2O_5 existing as a *hydrated* phosphate, with even as much water as in cacoxene or the basic salts described by Rammelsberg, will not suffice to bring the remaining ferric hydrate into accordance with the formula of göthite.

The mineral is obviously limonite in chemical composition, while the blood-red color has not, so far as I am aware, before been noticed in connection with this species, but has been looked upon as characteristic of göthite. How far the large amount of phosphorus present may have any relation to the peculiar appearance of the mineral may be questioned.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Composition of the so-called Hydrate of Chlorine.*—In many of the most recent text-books on Chemistry it is stated that the composition of the hydrate of chlorine, ordinarily given as $Cl_2+(H_2O)_{10}$, may also be written as $HOCl+HCl+(H_2O)_9$. The ground for this assumption is not entirely satisfactory; it seems to have arisen from a statement of Hiller, that the hydrate is formed when hydrochloric acid gas is passed into a solution of hypochlorous acid cooled to 0°. But since HCl and HOCl react upon each other to produce chlorine and water, the arrest of this action at 0° cannot be accepted as a fact in the absence of direct evidence. **Göpner** has undertaken to decide between these formulas. Since, were the formula $Cl_2+(H_2O)_{10}$ correct, the sole product when the solution is agitated with mercury would be mercurous chloride; and were the formula $HOCl+HCl+(H_2O)_9$ the right one, that product must be mercuryl chloride, and finally, in presence of hydrochloric acid, mercuric oxide, the matter may readily be tested. Experiment proves the latter view to be in fact the true one; though by the subsequent action of the metallic mercury, some mercurous chloride was formed. In a lecture the above experiment may be useful in order to show that the reaction with water is the same as with potassium oxide or hydrate:

$$Cl_2+HOH=HCl+HOCl.$$
$$Cl_2+KOK=KCl+KOCl.$$

To this view **Hugo Schiff**, in a subsequent paper, decidedly objects, deeming the proof entirely insufficient. He gives quite a number of facts to prove the contrary.—*Ber. Berl. Chem. Ges.*, viii, 287, 419, March, April, 1875. G. F. B.

2. *On the Constitution of Ammonium Compounds.*—The question of fixed or variable equivalence is yet a vexed one in Chemistry. V. Meyer and Lecco have made an investigation to ascertain whether nitrogen has in the ammonium compounds an equivalence of three or five. On the former hypothesis the constitution of ammonium chloride would be $\mathrm{N}\equiv\!:\!\equiv\mathrm{H}_3+\mathrm{H}-\cdot-\mathrm{Cl}$; on the latter it would be $\mathrm{H}_4\equiv\!:\!\equiv\mathrm{N}-\cdot-\mathrm{Cl}$. The method by which the authors purposed to test the question was to submit to minute comparison the two di-methyl di-ethyl derivatives of ammonium, which may be thus formulated: $\mathrm{N}\left\{\begin{matrix}\mathrm{CH}_3\\ \mathrm{CH}_3\\ \mathrm{C}_2\mathrm{H}_5\end{matrix}\right.+\mathrm{C}_2\mathrm{H}_5\mathrm{Cl}$, and $\mathrm{N}\left\{\begin{matrix}\mathrm{C}_2\mathrm{H}_5\\ \mathrm{C}_2\mathrm{H}_5\\ \mathrm{CH}_3\end{matrix}\right.+\mathrm{CH}_3\mathrm{Cl}$. If nitrogen be a pentad, these two bodies must be identical; if a triad, they are only isomeric. The two chlorides were prepared with care, the one from dimethylamine, the other from diethylamine. They were then examined: 1st, as to their physical properties and those of their derivatives; 2d, their characteristic reactions in the wet way; 3d, their distillation products; 4th, the solubility of their platinum salts; 5th, the crystallographic identity of these platinum salts; and 6th, the fusibility of their picrates. In all these particulars they were found to be identical. The hypothesis that ammonium chloride is a molecular compound would seem, therefore, to be negatived, and the quinquivalence of nitrogen in all the ammonium derivatives established.—*Ber. Berl. Chem. Ges.*, viii, 233, March, 1875.
G. F. B.

3. *On Methyl aldehyde and Methyl formate.*—Methyl aldehyde was prepared by Hofmann, the discoverer, by passing a mixture of air and the vapor of wood spirit over a red hot platinum coil. Volhard proposes a simple modification of this plan, as a lecture experiment, by which, in the course of an hour, sufficient of the aldehyde can be obtained to show its reducing power. A Davy's aphlogistic lamp—which consists of an ordinary spirit lamp upon and above the wick of which is placed a small platinum wire coil—is filled with methyl alcohol, lighted, and after a minute the flame blown out; the slow combustion which then takes place produces methyl aldehyde. It is only necessary to place over the wick an adapter connected at its smaller end with a Liebig's condenser, a receiver, one or two wash bottles, and an aspirator. The current of air is regulated so that the coil does not appear red in daylight, while yet it is sufficiently hot to prevent condensation in the adapter. If the aspiration be continuous, it is only necessary to keep the lamp filled in order to have the process continuous. The author collected in his experiments 90 to 100 grams of liquid in the receiver in twenty-four hours. By the warmth of the hand it reduced in a few minutes a solution of ammonio-silver nitrate and

gave a brilliant mirror. Saturated with hydrogen sulphide, it became a magma of crystals of methyl sulph-aldehyde. Estimated by its reducing power, however, the liquid contained but a little more than ·5 of one per cent. of aldehyde. The author subsequently prepared methyl formate, by distilling methyl alcohol recently saturated with hydrochloric gas, from calcium formate, in order to ascertain whether it would yield methyl aldehyde by the action of heat; a reaction analogous to that by which its chlorine derivative, methyl perchloroformate, yields carbonyl chloride. But the results were negative, the ether yielding methyl alcohol, carbonic oxide, carbonic acid, and marsh gas, the two latter in small proportion.—*Liebig's Annalen*, clxxvi, 128, Feb., 1875. G. F. B.

4. *Composition of Gum Tragacanth.*—GIRAUD has made a minute examination of the chemical characters of gum tragacanth. He finds (1) that this gum is but very slightly soluble in water, and that the product in the filtrate is not a definite principle, like arabin, but is a mixture of several substances; (2) that, digested on the water bath for twenty-four hours, with fifty times its weight of water, much of it is transformed into a soluble gum, which no longer swells after drying; this new substance is pectin; (3) that under the action of water containing one per cent of acid, the production of pectin takes place in two or three hours. It becomes entirely soluble, and alcohol precipitates pectin, not arabin, from the solution. Alkalies change it into pectates and metapectates. Hence gum tragacanth consists for the most part of a pectic principle insoluble in water, apparently identical with Fremy's pectose. From it, by precipitating the pectin solution by barium hydrate, and decomposing by an acid, pure pectin acid was obtained. Upon analysis, gum tragacanth yields as follows: water, twenty per cent; pectic compounds, sixty per cent; soluble gum, eight to ten per cent; cellulose, three per cent; starch, two to three per cent; mineral matter, three per cent; nitrogenous matters, traces.—*Moniteur Scientifique*, III, v, 361, April, 1875. G. F. B.

5. *On the Occurrence of Cane Sugar in Malted Grain.*—The question of the occurrence of dextrin and sugar in the cereals, whether malted or unmalted, is yet an open one. KUHNEMANN has sought to throw some light upon it by carefully examining malted barley for both these substances. Five kilograms of finely bruised malted barley was extracted cold with eight kilograms ninety-five per cent alcohol, the expressed liquid mixed with twice its weight of ether, and agitated with a little more than one-fourth its weight of water. The lower aqueous layer, treated with a little barium hydrate, was evaporated, filtered, and polarized. It rotated to the right, but contained two sugars, one reducing the copper test, the other not. Upon evaporation to dryness and extraction with alcohol, the two sugars were separated by fractional crystallization. In this way 0·6 of one per cent of crystallized sugar was obtained from the malt. When pure, this sugar is white, is sweet to the taste, crystallizes in well-

defined forms, and does not reduce the copper test. In its optical behavior, therefore, as well as in its physical and chemical properties, it appears to be identical with cane sugar. The malt-residue, extracted with water, yielded a solution from which alcohol precipitated a substance in white flocks, which is similar to, but is chemically not identical with, dextrin.—*Ber. Berl. Chem. Ges.*, viii, 202, March, 1875. G. F. B.

6. *Carbonyles, a new Class of Organic Bodies.*—M. Berthelot has recently instituted a new class of organic bodies, to which he has given the name of *carbonyles*, and to which he assigns three bodies hitherto rather ambiguous in chemical behavior. These are: allylene oxide (dimethylene carbonyle), $CO\left\{\begin{matrix}CH_2\\CH_2\end{matrix}\right)$ diphenylene-acetone (diphenylene carbonyle, $CO\left\{\begin{matrix}C_6H_4\\C_6H_4\end{matrix}\right)$ and ordinary camphor (terebutylene carbonyle, $CO\left\{\begin{matrix}C_5H_8\\C_4H_8\end{matrix}\right)$. Suberone, $C_7H_{12}O$, perhaps, belongs here also. The distinguishing feature of carbonyles is their double function. In the first place, they act like aldehydes, being able to fix hydrogen directly and to produce alcohols; while, like aldehydes, they are re-produced by the dehydrogenization of these alcohols. Again, like aldehydes, they may be formed by the direct or indirect oxidation of hydrocarbons; just as ethylene hydride and oxygen, $C_2H_4(H_2)+O$, produces ethyl aldehyde, $C_2H_4(O)$, so camphene hydride and oxygen, $C_{10}H_{16}(H_2)+O$, produces camphor, $C_{10}H_{16}(O)$, by indirect oxidation; and as ethylene by direct oxidation yields ethyl aldehyde, $C_2H_4+O=C_2H_4(O)$, so camphene, $C_{10}H_{16}+O$, gives camphor, $C_{10}H_{16}(O)$. But, secondly, it is to be observed that while aldehydes are produced by the indirect oxidation of saturated hydrocarbons, carbonyles result from the indirect oxidation of unsaturated hydrocarbons. This is a very material difference, since, beside its aldehydic function, the carbonyle molecule is unsaturated, and can unite directly with other unsaturated molecules. Hence, like carbonyl itself, from which these bodies take their name, they can fix directly the elements of water and form monobasic acids; as $CO+HOH=CHOH$, formic acid, so $CO\left\{\begin{matrix}CH_2\\CH_2\end{matrix}\right.+HOH=C_3H_6O_2$, propionic acid. So also, by reason of this unsaturation, they can unite directly with three atoms of oxygen to form dibasic acids; camphor, $C_{10}H_{16}O$, for example, forming with $O_3, C_{10}H_{16}O_4$, camphoric acid. So, conversely, the removal of water and carbon dioxide from a single molecule of a dibasic acid, yields a carbonyle, differing from the analogous production of acetones by the fact that the removal in the latter case is from two molecules of a monobasic acid. Regarding all hydrocarbons as derivatives of marsh gas, or formene, the formation of the three classes of aldehydes from them by indirect oxidation may be represented as follows:

$$\begin{array}{r}\text{Hydrocarbons.}\\ CH_4\\ CH_2.CH_4\\ C_2H_4CH_4\\ C_6H_4CH_4\end{array}\left.\vphantom{\begin{array}{c}a\\a\\a\\a\end{array}}\right\}\text{yield}\left\{\begin{array}{r}\text{Aldehydes.}\\ CH_2O\\ CH_2.CH_2O\\ C_2H_4.CH_2O\\ C_6H_4.CH_2O\end{array}\right.\qquad\begin{array}{c}\text{Hydrocarbons.}\\ \left.\begin{array}{l}CH_2.CH_2.CH_4\\ C_6H_4.CH_2.C_6H_4\\ C_6H_4.CH_2.C_6H_6\end{array}\right\}\text{yield}\end{array}$$

$$\begin{array}{c}\text{Secondary}\\ \text{aldehydes or acetones.}\\ \left\{\begin{array}{l}CH_2.CO.CH_4\\ C_6H_4.CO.C_6H_4\\ C_6H_4.CO.C_6H_6\end{array}\right.\end{array}\qquad\begin{array}{r}\text{Hydrocarbons.}\\ CH_2[-]\\ C_2H_4\ .\ CH_2[-]\\ C_6H_6\ .\ CH_2[-]\\ C_9H_{16}.CH_2[-]\\ C_{12}H_8.CH_2[-]\end{array}\left.\vphantom{\begin{array}{c}a\\a\\a\\a\\a\end{array}}\right\}\text{yield}\left\{\begin{array}{r}\text{Carbonyles.}\\ CO[-]\\ C_2H_4\ .\ CO[-]\\ C_6H_6\ .\ CO[-]\\ C_9H_{16}.CO[-]\\ C_{12}H_8.CO[-]\end{array}\right.$$

The author gives in conclusion the evidence that camphor itself belongs to this class of bodies, and says that had he not hesitated to found a new class of bodies on a single compound, he would long ago have proposed to consider camphor as a carbonyle.—*Bull. Soc. Ch.*, II, xxiii, 146, Feb., 1875. G. F. B.

7. *Formation of Hydroxylamine by the reduction of Dinitro-compounds.*—Whenever a nitro-organic derivative, in which but a single nitro-group is united to any one carbon atom, is reduced with tin and hydrochloric acid, the nitro-group, as is well known, becomes an amido group. V. MEYER and LOCHER have experimented to ascertain what would be the result on reduction if two such groups were so united. For this purpose they selected the dinitropropane $CH_3-\cdot-C(NO_2)_2-\cdot-CH_3$ recently discovered by them, and introduced it into dilute hydrochloric acid containing metallic tin. In a short time the nitro-body had disappeared, and the solution, after removal of the tin by hydrogen sulphide, afforded on evaporation pure hydroxylamine hydrochloride. The organic reduction-product was acetone, so that the reaction is as follows: $CH_3-\cdot-C(NO_2)_2-\cdot-CH_3+H_8=CH_3-\cdot-CO-\cdot-CH_3+(NH_3O)_2+H_2O$. Ethyl-nitrolic acid reacts similarly, yielding nearly the theoretic quantity. Nitroform, however, combines both reactions, yielding beside hydroxylamine, hydrogen cyanide, and ammonia.—*Ber. Berl. Chem. Ges.*, viii, 215, March, 1875.

G. F. B.

8. *Viscosity of Gases.*—Capt. A. VON OBERMAYER has compared the previous determinations of this quantity, and states that of the two hypotheses from which the dynamical theory of gases start, the older gives the co-efficient of friction of gases proportional to the square root, the newer (Maxwell's) gives it proportional to the first power of the absolute temperature. From the retardation of vibrating disks by the friction of the air, Maxwell found experimentally the power 1, O. E. Mayer the power $\frac{3}{4}$; by experiments on capillary tubes O. E. Mayer found the power $\frac{3}{4}$, J. Puluj the power $\frac{2}{3}$ of the absolute temperature.

For the more certain determination of this ratio, experiments on currents through four capillaries of glass and one of brass were undertaken; and together with the temperature of the room those of boiling water, congealing paraffin, and a mixture of salt and snow were employed. A first series of experiments, less accurate, were carried out, with the difference of pressure variable; a second, considerably more exact, with the difference constant. The results of the two series agree very well, and confirm those of Mayer's experiments in a perfectly satisfactory manner. For the co-efficient of friction μ at the temperature t there were found:

According to the first series, $\mu = \cdot 0001706\ (1 + \cdot 002735\ t)$.
According to the second series, $\mu = \cdot 00016477\ (1 + \cdot 002723\ t)$.

—*Roy. Acad. of Vienna*, Feb. 4th, 1875; *Phil. Mag.*, xlix, 332.

E. C. P.

9. *Specific Heat of Carbon, Boron and Silicon.*—Dr. H. F. **Weber** presented a paper on this subject at the fifty-sixth anniversary of the Royal Würtemberg Land and Forest Management Academy at Hohenheim. The law of Dulong and Petit, that the product of the specific heat by the atomic weight is constant, holds true for most of the solid elements. Carbon, boron and silicon, however, seem to deviate considerably from this law, and give different results in their various allotropic conditions. A comparison of the results of different observers shows that while they differ very greatly from one another, the larger the interval of temperature employed the greater the result.

Dr. Weber has accordingly made a series of measurements at temperatures from −80° to 1,000°. The amount of heat was determined by Bunsen's ice calorimeter, and the temperature when below 300° by a thermometer; for higher temperatures the heat given out by a platinum bar in cooling served to determine the temperature by calculation.

The first experiments were on the specific heat of the diamond. The collections of the Universities of Berlin and Vienna were placed at his disposal. It was found that the specific heat increased with the temperature from ·07 at −50° C, to ·23 at 100°, and ·46 at a red heat. A similar increase is found in the case of graphite, and leads to the following conclusions:—That from a red heat upward, the specific heat of carbon does not vary more than the specific heats of those elements which fulfil the law of Dulong and Petit, and at these high temperatures there seems to be no real difference in the specific heat of the graphitic and that of the diamond modification of carbon. All anomalies, therefore, disappear above 600° C., and carbon then obeys the above law. A comparison of the specific heats of graphite, of dense amorphous carbon and of porous carbon showed that they were identical within the temperature interval 0° to 225°. A unit weight of porous carbon, as far as possible free from water, evolves 4·16 heat-units when wetted with water.

In the case of boron two hundred small crystals were used, having a total weight of ·67 grams. The observations were extended from −40° to +230° and showed a similar increase to that of carbon. It is, therefore, very probable that at higher temperatures the specific heat attains a constant value of about ·50, and consequently the number 11, which is generally accepted as the atomic weight of boron, is really the atomic weight; further that the atomic heat of boron is about 5·5, and that therefore at a red heat boron obeys the law of Dulong and Petit.

Small brilliant steelgray crystals of silicon, weighing 1·123 grams, were next employed. From these it was shown that silicon does not form an extraordinary exception to the law of Dulong and Petit; so soon as the temperature passes 200° it comes within the sphere of this law. The smallest relative weight of silicon (28) hitherto found in the molecule of any of the gaseous compounds of this element is in reality the atomic weight of silicon.—*Phil. Mag.*, xlix, 161, 276. E. C. P.

10. *Action of Electric Currents on Alloys.*—M. E. OBACH, at the suggestion of M. Wiedemann, has tested the conclusions of Gerardin on the electrolytic decomposition of alloys. A series of experiments were made from which he concludes that the passage of the galvanic current produces no electrolytic effect either in amalgams or in alloys. After being traversed by a current, the sodium amalgam decomposes water at both poles exactly as before. The action of the current modifies neither the hardness nor the malleability of the lead and tin alloys, nor the fluidity of the potassium sodium alloys. It introduces in the neighborhood of the electrodes no change exceeding the limits of possible error in the analyses.—*Bibl. Univ.*, ccvii, 229. E. C. P.

11. *New Property of Aluminum.*—M. E. DUCRETET observes that on inserting in a galvanic circuit a voltameter with two electrodes, one of aluminum, the other of platinum, different effects are obtained according to the direction of the current. When the aluminum receives the negative electricity the water is decomposed and the current traverses the circuit freely. But on reversing the current the decomposition ceases and scarcely any electricity is transmitted. An electric bell inserted in the circuit in the first case rang violently, and in the second case did not move. Replacing the bell by an iron wire, in the first case it was melted, and not even heated in the second. A galvanometer gave in one case a deflection of 22° and in the other of only 2°.

The effect is produced instantly; it is constant and durable whatever the number of inversions of the current. If other metals are used instead of platinum, they are deposited on the aluminum and interfere with the experiment This stoppage of the current is not produced by a plate of gold, silver, platinum, copper, zinc, magnesium, tin, lead, etc., replacing the aluminum. A partial effect is produced with iron, but the surface is soon altered, with the disengagement of a bad odor. As to the aluminum, its surface appears to be preserved by a slight layer of alumina which is formed immediately, and remains, in spite of the inversion of the current.

Many practical applications of this property suggest themselves. Two messages may be sent over a telegraph line at the same time in opposite directions by using two voltameters with the aluminum on opposite sides. All trouble from variable resistance is thus avoided.—*Journ. de Phys.*, iv, 84. E. C. P.

II. GEOLOGY AND NATURAL HISTORY.

1. *The Surface Geology of Ohio;* by J. S. NEWBERRY. 80 pp. 8vo. (From the Report of the Geological Survey of Ohio, vol. ii. Columbus, 1874.)—Dr. Newberry here presents in detail the many facts which he has observed connected with the Quaternary of Ohio and the adjoining States, and the conclusions to which his various observations have led him. The successive events of the Quaternary, which he gives in the opening part of the chapter, are briefly as follows:

(1.) The period of cold and the great glacier over New England and the region west, north of the Ohio, during which the land stood above its present level.

(2.) A period of lower level, milder climate, retreating glaciers, great freshwater lakes; and also of the deposit of the bowlder clay, which is usually thinly laminated in Ohio and contains no bowlders; also of extended forests over much of the region left bare by the retreat of the glacier from Ohio, Indiana and Illinois, and of the formation of peat beds, in which occur the remains of the Mastodon, Elephant, Castoroides, etc.

(3.) A further subsidence, making the Mississippi an arm of the sea, and putting that part of the continent including Southern Ohio under salt water; the time when deposits of sand and gravel were spread over the peat beds of Southern Ohio, lacustrine clays laid down in Northern Ohio, and the lœss along the Mississippi Valley; when icebergs floated southward across the great lake region from the Canadian highlands, carrying bowlders, gravel and earth; when portions of the highlands of Ohio were low islands and shallows, and sand and gravel were accumulated and assorted about them by the waves; when certain drainage lines were determined, as those along the valleys of the Wabash, Scioto, Muskingum and the Beaver.

(4.) A very gradual "retirement of the sea," with "intervals of rest and recession;" and, as a consequence, the formation of terraces along river valleys, "by the arrest of their flow and the deposition of the materials they transported in the dead water which partially filled these valleys;" the gradual draining of the lake-region, leaving old shore lines around the lakes with terraces or lake ridges in some parts of their circuit.

The eras of elevation, subsidence, and subsequent elevation, indicated in § 1, 2 and 3, and 4, are those which the writer has defined as the Glacial, Champlain or Fluvial, and Recent or Terrace periods.

No marine relics of the era of submergence (§ 3) are stated to occur over the region that is supposed to have been then under the sea, or in the deposits then made; that is, in the lœss along the Mississippi River, or the beds of Southern Ohio. In fact, the fossils found are those of the land or of fresh water, and have led others to regard the deposits as simply of river origin. Indeed, this seems to be the opinion which Dr. Newberry presents in his chapter on the lœss, pages 36–38.

Dr. Newberry follows the announcement of his conclusions by an account of his observations on the Drift deposits of Ohio, the old deserted and buried river channels of the State and elsewhere, the heights and positions of the lake ridges, and on other topics of interest. The more prominent lake ridges south of Lake Erie, as shown on a map, are: the "North ridge," nearest the lake, 99 to 105 feet in height above the lake; the "Center ridge," 148 to 162 feet; and the "South ridge," 200 to 205 feet. Others also are named: the "Coe ridge," which may be identical with the last, 180 to 200 feet; the "Sugar ridge," 165 to 167 feet; the "Chestnut ridge," 189 to 191 feet. How far the different levels of these lake-ridges are due to differences, existing before the elevation, in the level of the high-water flats along the lake-borders, and to the existence of other mud or sand flats under water at different levels, it is hardly possible to determine. In bays and lakes having shallow shore regions, such flats, various in level, are common; and after an elevation, they would exist as terrace plains and be of different heights because retaining their former differences of level.

In the remarks on the origin of the cold of the Glacial period, Dr. Newberry gives his objections to the theory of Lyell, and states, in the course of his remarks, that this theory is that advocated by Prof. Dana. He has not appreciated the paragraphs in the last edition of Prof. Dana's Geology (pp. 541 and 755), in which the cold climate is attributed, not simply to an elevation of high-latitude regions, but to the effect which such an elevation would have on the oceanic currents, and thereby on the distribution by these currents of heat and cold, as noticed in this volume on page 314.

Dr. Newberry has labored long and zealously among the Quaternary deposits of Ohio and the adjoining States, and his chapter on the subject is one that may be studied with great profit. J. D. D.

2. *Prototaxites of Dr. Dawson, a genus related to the Taxineæ.*—Dr. Dawson's view that the *Prototaxites Logani* of the Devonian of Gaspé, cannot be regarded as a sea-weed, a surged by Carruthers, appears to be sufficiently sustained—independently of the argument from the structure—by the following facts which Dawson has published respecting the condition and general character of the fossil.* He says (in a paper in the "Monthly Microscopical Journal" for August, 1873) that "the mode of occurrence is sufficient" to convince any practical palæontologist that the plant cannot be a sea-weed. Its large dimensions, one specimen found at Gaspé Bay being three feet in diameter; its sending forth strong lateral branches, and gnarled roots; its occurrence with land plants in beds where there are no marine organisms, and which must have been deposited in water too shallow to render

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* For a figure, see Dana's Manual of Geology, 1874, page 258.

possible the existence of the large oceanic Algæ to which Mr. Carruthers likens the plant—these are all conditions requiring us to suppose that the plant grew on the land. Further, the trunks are preserved in sandstone, retaining their rotundity of form even when prostrate; and are thoroughly penetrated with silica except the thin coaly bark. Not only are Algæ incapable of occurring in this way, but even the less dense and durable plants, as Sigillariæ and Lepidodendra are never found thus preserved. Only the extremely durable trunks of coniferous trees are capable of preservation under such circumstances. In the very beds in which these occur, *Lepidodendra*, tree ferns and *Psilophyton*, are flattened into mere coaly films. This absolutely proves to any one having experience in the mode of occurrence of fossil plants, that here we have to deal with a strong and durable woody plant."

The paper discusses also the microscopic structure of the fossil, and its probable affinities.

3. *Fossil Vertebrates of Cretaceous and Tertiary affinities in the same beds on the Saskatchewan.*—Prof. Cope has examined fossils collected by G. M. Dawson in beds of the "Fort Union Group," from a horizon 100 to 200 feet above the beds of Cretaceous No. 5. They include among Dinosaurs, *Cionidon stenopsis* Cope (about the size of *C. arctatus* Cope) and a species related to *Hadrosaurus*. But with these species of Cretaceous affinity there are two tortoises of the genus *Plastomenus* Cope (*P. coalescens* and *P. costatus* Cope), a genus of the Eocene, and scales of gars, closely resembling those of the genus *Clastes* of the Lower Eocene of the Rocky Mountains. "The list of species," says Cope, "short as it is, indicates the future discovery of a complete transition from Cretaceous to Eocene life more clearly than any yet obtained in the West" (or Rocky Mountain region).—*Proc. Acad. Nat. Sci. Philad.*, 1875, 9.

4. *Systematic Catalogue of Vertebrata of the Eocene of New Mexico, collected in* 1874. E. D. Cope, A.M., Paleontologist. 38 pp. 8vo. Washington, 1875. Geographical explorations and surveys under Lieut. Wheeler, of the Engineer Department, U. S. A. —Contains descriptions of some new genera and many species of Mammals, with some species of Reptiles, and embraces many details of importance.

5. *Daubrée on the Platiniferous rocks of the Urals.*—At a session of the French Academy of Sciences in March, Daubrée described the rocks of the Ural affording platinum. They have a base of chrysolite. The masses come from the conglomerates near Nischne Tagilsk, where platinum is obtained. Besides chrysolite, serpentine and chromic iron are intimately associated with the platinum. The facts seem to prove that the original platinum-bearing rock was a chrysolite rock more or less transformed into serpentine, and was accompanied with diallage, which is common in the specimens. The presence of chromic iron is also to be noted; for it appears to bear evidence as to the changes through which the gangue rock of the platinum had passed.

6. *The Geological Story briefly told, an Introduction to Geology for the General Reader and for beginners in the Science;* by JAMES D. DANA. 264 pages, 12mo, with numerous illustrations. New York & Chicago. 1875. (Ivison, Blakeman, Taylor & Co.) Price $1.50.—This work, although small, and different in arrangement from the author's Geological Manual and Text-book, gives the reader full illustrations of the principles of the science, of the methods of geological work, and of the course of geological history. It is simple in its language and full in its illustrations. After a few pages on the constituents, kinds, and structure of rocks, it takes up, in Part II, the subject of Causes in Geology and their Effects. Under this head it explains, *first*, the *making of rocks* (including the preparing of the material) through the agency of plants and animals, through the quiet work of air and moisture, through the work of the winds, fresh waters, ocean, and ice, and through heat,—the last topic including the effects of contraction and expansion, the formation of volcanic and other igneous rocks, and the subjects of solidification, metamorphism, and mineral veins. Then, *secondly*, it treats of the making of valleys and all depressions on the earth's surface; and, *thirdly*, of the making and shaping of hills and mountains, (1) by igneous ejections, (2) by the erosion of elevated lands, and (3) by the upturning and flexures of rocks, and bending of the earth's crust.

Part III, occupying 150 pages, takes up Historical Geology, describing the rocks that were forming, the most prominent kinds of plants and animals that were living, the mountains that were making, and other events, during each of the successive ages in the course of geological time; and ends with a brief review of the progress of life.

This volume is handsomely printed, and contains several cuts not in the Manual of Geology.

7. *Catalogue of Lower Silurian Fossils of the Cincinnati Group, found within* 40 *or* 50 *miles of Cincinnati, with descriptions of some new species of Corals and Polyzoa;* by U. P. JAMES, Cincinnati. New edition, much enlarged, 8 pp. large 8vo. Cincinnati, April, 1875.—The new species described by Mr. James are *Chætetes calycula*, *C. clavacoideus*, *C. Cincinnatiensis*, *C.* (?) *O'Nealli*, *Alveolites* (?) *granulosa*, *Ceramopora Nicholsoni*, *Ptilodictya acuminata* and *Alecto nexilis*.

8. *Origin of the Lower Silurian Limonites of York and Adams Counties, Pennsylvania;* by PERSIFOR FRAZER, Jr.—(Proc. Amer. Phil. Soc., March, 1875.)—Mr. Frazer, after describing the limonite deposits, attributes the origin of the ore to the presence of pyrites in the slates. The beds usually lie between the slate and Lower Silurian limestone, but the latter is generally little, if at all, acted upon or eroded, and often is not even stained.

9. BENTHAM, *Revision of the Sub-order Mimoseæ.*—This paper forms the third part of the 30th volume of the *Transactions of the Linnean Society*, fills over 300 quarto pages of letterpress, and is illustrated by five plates, exhibiting the principal varieties

in the legumes of *Mimoseæ.* The group, which now numbers 1200 species, under 29 genera, was first put into good order by Mr. Bentham about 30 years ago; and he has now taken occasion to revise it completely, while studying the South American species for the *Flora Brasiliensis*, after having re-examined the Australian ones for his *Flora Australica.* The generic characters, founded mainly on the stamens, of which he made happy use in the former monograph, have stood the test, as being the best as well as the readiest; and his facile arrangement has approved itself to other botanists (with one exception) as well as to his own enlarged experience. He has now brought in a new subsidiary character, that of the presence or absence of albumen in the seeds. This, for instance, is present in *Mimoseæ* but not in the *Acacieæ* and *Ingeæ*, i. e., the tribes with indefinite stamens. As to genera, the adage "by their fruits ye shall know them," however true it may be elsewhere, has little application here. This is illustrated by the figures of the pods of various *Mimoseæ* and *Acacieæ* subjoined to the memoir, and is shown in the letterpress by a general discussion of the facts. As to geographical distribution, it is interesting to note that Mr. Bentham reduces the number of species really common to the New and to the Old World, without any evidence or reasonable suspicion of recent transportation, to four species. *Entada scandens*, with its huge pods and hard seeds, not rarely wafted across the Atlantic by the Gulf Stream, is one; if of African origin, the trade-wind current may have given it to the West Indies. *Neptunia oleracea* is a tropical aquatic, conjecturally of South American origin, the seeds of which may have traversed the Atlantic; *Mimosa asperata*, probably also American, is not quite so readily accounted for in Africa; and the fourth, *Acacia Farnesiana*, is still more puzzling; but it would seem somehow to have found its way from Western South America to Australia and the Indian Archipelago before the days of Columbus; yet in all four cases, the want of differentiation is thought to indicate a comparatively modern dispersion. Of distinct but closely representative species in the two hemispheres, Mr. Bentham enumerates nine pairs. He might have added the case of two insular species, *Acacia heterophylla* of Mauritius, and *A. Koa* of the Sandwich Islands, only that he doubts if the two are sufficiently distinct. Seventeen of the 29 genera are restricted to one of the two hemispheres; but nine of these have only from one to three species, and but two (*Albizzia* in the Old World and *Inga* in the New) are at all numerous in species. Upon the more speculative discussion of the geographical data in reference to probable derivation, we cannot here enter. The subject is treated after the manner adopted in the memoir on *Cassia* and that on *Compositæ.* Remarking the great American predominance of the order in all its tribes, the author acutely remarks that "this high degree of recent luxuriance and prosperity of American races, however, can by no means be relied upon as evidence as to local origin, or even as to comparative remoteness of antiquity; for that may rather be sought for where there are the greatest number of highly differentiated monotypes or oligotypes of limited areas and fixed characters, exhibiting the last remains, as it were, of expiring races; and these are undoubtedly to be met with chiefly in the Old World—in the first place in East Africa and the Mascarene Islands, and secondly in the Malayan Archipelago." But the search for original birth-places, as he intimates, leads only through hazardous conjectures into thick obscurity. Our notice is confined to the few prefatory pages, on account of their general interest. As to the monograph itself, it is a model, in form and substance. The nominative specific characters make a most favorable presentation of that mode. Still, where condensation is important, the single sentence and the prevalent ablative case have some advantage in terseness. A. G.

10. *Flora of British India*, by J. D. Hooker, C.B., etc., *assisted by various botanists.* Vol. I, pp. 740, 8vo. Reeve & Co., London. 1872–1875.—The third part, issued at the close of the past winter, brings this important work up to the close of the first volume, and nearly up to the *Leguminosæ.* This present part, which begins with *Geraniaceæ* and *Rutaceæ*, from the editor's own hands, is otherwise mainly the work of three or four English collaborators who have been enlisted in it, Messrs. Bennett, Hiern, and Lawson, and also Dr. Masters, the latter a botanist of no little experience. The younger botanists at all capable of this kind of work are few; accessions to the number are heartily to be welcomed. By their aid, under Dr. Hooker's efficient supervision, we may hope that this great undertaking will be duly carried through. A. G.

11. Miers, *On the Lecythidaceæ.*—A splendid memoir, of 64 pages and 33 plates, large quarto, forming the second part of the 30th volume of the *Transactions of the Linnean Society*, 1874, a wonderful piece of work for a man of Mr. Miers' great age. He proposes to restore the *Lecythideæ* or *Lecythidaceæ* as an independent order. The plates illustrate the floral structure of the twelve genera which the author recognizes, and the fruits of most of them. It is a remarkably interesting group, consisting of huge trees, all tropical American, with singular flowers and large woody fruits, a sort of pyxis, containing numerous nut-like seeds, of which Brazil-nuts (from *Bertholletia excelsa*) and Sapucaia-nuts (from a species of *Lecythis*) are well-known examples. Few botanists have had the opportunity of properly studying these noble plants, and no one has devoted to them so much attention as the veteran Miers. A. G.

12. *Synopsis Filicum.*—The second edition of the *Synopsis of all known Ferns*, by Hooker and Baker (London: Robert Hardwicke), issued near the end of the year 1874, has just reached us. It bears Sir Wm. Hooker's honored name, for the work was planned by him, as an abridgement of the *Species Filicum*, and the first sheets had passed through the press before his death; but most of the volume was prepared by Mr. Baker, from

mere notes and indications by his predecessor and from his own studies of the rich materials which were ready to his hand. Accordingly, in any particular reference to the contents, after p. 48, Mr. Baker's name would be used. The original edition was issued, in successive parts, between 1865 and 1868. A new one has long been wanted, and still is so, in a certain sense. For, although the editor has "endeavored in this edition to briefly characterize and fit into their places the new discoveries and the plants found upon fuller information to have been inadequately dealt with in the first," he has not been able to deal with them quite freely. The pages are in stereotype, so that the body of the old work has been reprinted—no doubt with corrections as far as might be—and the additions are collected in an appendix of 75 pages, followed by a good new index to the whole. This must serve for the present, and fern-students will be very thankful for the new help. But we hope that Mr. Baker, in due time, will re-elaborate the order completely. A. G.

13. GRISEBACH, *Plantæ Lorentzianæ.*—An elaborate paper, of 213 pages and two plates, 4to, separately issued from the 19th volume of Transactions of the Royal Society of Sciences of Göttingen, 1874. It describes a good collection of plants of the Argentine Republic made by Prof. Lorentz of Cordoba. It is prefaced by a brief account of the region, followed by some remarks upon the more interesting plants of the collection, under their natural orders. Many new species are proposed and a few new genera. Under *Hydroleaceæ*, a name adopted instead of *Hydrophyllaceæ* (we know not why), a new *Nama* and a new *Phacelia* are described; also a new "genus, valde anomalum," *Sterrhymenia*, which is appended to the order, along with *Cardiopteris!* The suggested relationship is not apparent. A. G.

14. *Botanical Contributions* contained in the *Proceedings of the American Academy of Arts and Sciences*, vol. x, 1875.—This volume, just about to be issued, contains the papers communicated to the Academy and accepted for publication during the year ending in May. The botanical papers are,—

Conspectus of the North American Hydrophyllaceæ; by ASA GRAY. pp. 312–332. As this paper consists wholly of descriptive matter, some general considerations respecting the order, its distribution, affinities, &c., may hereafter be given in this Journal.

Revision of the Genus Ceanothus, and Descriptions of New Plants, with a Synopsis of the Western Species of Silene; by SERENO WATSON. pp. 333–350. The species of *Ceanothus*, mainly Californian, had become difficult of determination. They are here cleared up and arranged, under 28 species, two or three of which are still obscure or uncertain, and disposed under two groups (*Euceanothus* and *Cerastes*), which are well marked, but hardly worthy of subgeneric rank and substantive names.—The miscellaneous new species described are 32 in number, all western, but the first is a low *Clematis*, which belongs as far east as Kansas. Among them are eleven species of *Silene*, of Oregon and California, the determination of which led to a study of all our western Silenes. The results are briefly recorded in a foot-note, giving the diagnoses of 21 species. At this rate we may rival Europe.

List of the Marine Algæ of the United States, with notes of New and Imperfectly Known Species; by W. G. FARLOW. pp. 351–280. The preceding papers bear the date of publication (April, 1875) as well as that of presentation, which it is well to know. It is omitted in Professor Farlow's neat and useful paper; but copies had reached us before the 1st of May. The species of the list being continuously numbered, we perceive that they amount to 430, of which 54 are additions to Dr. Harvey's Nereis (1857), and three or four are new. The notes are numerous and critical. We understand that Professor Farlow intends to follow up this paper with other publications upon our *Algæ*. A. G.

15. DANIEL HANBURY.—Not long ago we called attention to a most valuable book, the *Pharmacographia or History of Drugs*, by Prof. Flückiger of Strasburg and Daniel Hanbury of London, the first-fruits of much investigation, the precursor, as was hoped, of more extended similar works by the English author. We have now sadly to record the decease of Mr. Hanbury, of enteric typhoid, on the 24th of March, at his residence on Clapham Common, in the 50th year of his age. The obituary and biographical notices which have appeared in the London scientific journals, and in the Proceedings of the learned Societies, as well as loving individual tributes to an endeared memory, have given expression to the loss which has been sustained, and delineated the outlines of a most worthy and winning character. The loss is deplored, personally and scientifically, over wider circles and on this side of the Atlantic. The pupil and friend of Pareira and his successor in his line of work, an adept in pharmaceutical knowledge, a keen botanist, and a most assiduous and conscientious investigator, a man of simple and pure tastes, and happily of sufficient means, he had just withdrawn wholly from business in the noted house in which he had an inherited share, so that he might devote his powers and acquisitions without distraction to the natural history of drugs and useful vegetable products. He had already done much: more than sixty articles were contributed by him to a single journal, the editor of which declares that "the quality of what he did was almost faultless," that "he never wrote without having original information to impart, and his papers uniformly bear evidence of careful investigation and thorough knowledge." The Transactions and Journal of the Linnean Society (of which he was repeatedly a Councillor and the Treasurer at the time of his death) contain several of his papers. His first published paper, "On Turnsole," appeared in 1850; his latest, on the "Countess Chinchon and the Cinchona genus," appeared, since his death, in "The Academy" of the third of April. An ardent botanist and lover of plants, he travelled much in the south of Europe, accompanied Dr. Hooker in his explorations of Lebanon, and took an active interest especially

in the introduction of officinal plants and in ornamental cultivation. With one villa-garden on the shores of the Mediterranean—that of his brother at Mortola—his memory to us is indelibly associated. Although remarkably self-reliant, Mr. Hanbury was the opposite of self-asserting or ambitious; but his sterling worth was soon recognized by the principal learned societies and associations. He was early chosen into the Royal Society and served upon its Council in 1873. Born and educated in the Society of Friends, he remained a devout and attached member of it, while the graces and goodness of his character endeared him to all who knew him. With the sense of personal loss his scientific comrades mingle deep regrets that a career of unusual usefulness and promise is cut short, and that in a line in which, it is to be feared, he leaves no successor. *Hanburia Mexicana*, a striking Cucurbitaceous genus, commemorates his services to Botany.
A. G.

16. *The Crustaceans of the Caves of Kentucky and Indiana;* by S. I. SMITH.—Through the courtesy of Dr. Packard of Salem, Mass., I have recently been enabled to examine the types of his *Crangonyx vitreus* from near Orleans, Ind., and also several specimens of an amphipod collected in Mammoth Cave by himself. All the specimens from Mammoth Cave are of a single species, which, there can be little doubt, is really the *Stygobromus vitreus* unintelligibly described from the same locality by Professor Cope. The species is really a *Crangonyx* and it should stand as *C. vitreus*, although very different from the one from Indiana which is referred to Cope's species by Dr. Packard and by him called *C. vitreus*. It is a small species, the largest specimen being less than a fourth of an inch (5·2 mm.) long, apparently wholly eyeless, and remarkable for the rudimentary character of the unibranched posterior caudal stylets, which are shorter than the telson. It seems to be near the typical species described by Bate, and it is closely allied in some respects to *C. tenuis*, also an apparently eyeless species, which I have described from wells at Middletown, Conn. Since this note was first written, I have examined several specimens of this last species, collected by Mr. J. K. Thacher, under stones in a small brook, near New Haven. From this it seems not at all improbable that the allied species from Kentucky and Indiana—and very likely also the eyeless, cave species of other groups—may still be found in the surface streams of the same region.

The specimens of Dr. Packard's species from Indiana are badly preserved but are sufficient to show that the species is very closely allied to *Crangonyx gracilis*, from Michigan, Lake Superior, etc., differing principally in the structure of the eyes, which are well developed and abundantly supplied with black pigment in *C. gracilis*, while in Dr. Packard's specimens they are observable with difficulty, are wholly without black pigment, are undoubtedly colorless in life, and are probably only imperfect organs of vision, although the structure of the facets can be distinctly made out.

The other differences are all very slight, scarcely sufficient to distinguish the subterranean form as a species, and certainly so slight that they would almost surely be overlooked if the two forms were found together.

As the crustaceans have recently been several times referred to as indicating the partially marine origin of the cave fauna of the Western States, a word in regard to their affinities may not be out of place. The species already described from Indiana and Kentucky are the following: *Cambarus Bartonii* Erichson, Mammoth Cave; *C. pellucidus* Erichson, caves in Ky. and Ind.; *Crangonyx vitreus* Smith, Mammoth Cave; *C. Packardii* Smith, wells, Ind.; *Cæcidotea stygia* Packard, caves and wells, Ind., and Mammoth Cave; *Euphiloscia Elrodii* Packard, caves, Ind.; *Cauloxenus stygius* Cope, caves, Ind. The genus *Cambarus* is strictly confined to American fresh waters, and *C. Bartonii* is one of the commonest species in the streams of the Western States. *Crangonyx*, as far as known, is wholly confined to fresh water. *Cæcidotea*, as far as we can judge from description and figures, is scarcely distinguishable, except in wanting eyes, from *Asellus*, a characteristically fresh water genus. The *Euphiloscia* was found also outside the caves and is allied to other terrestrial genera. The *Cauloxenus*, a Lernæan parasite of the blind fish, is so poorly described and figured, and the genera of the whole group to which it belongs are so difficult and imperfectly known, that it is useless to speculate on its exact affinities. In our Western and Southern States, species of perch, brook trout, the siscowet, lake white-fish, species of *Catostomus* and *Pomotis*, and other fresh water fishes, are infested with different species of Lernæans, and there is no more reason for regarding *Cauloxenus* as a "marine form" than any of these parasites. As well might we call a *Cambarus* or a *Crangonyx* a marine form because the great majority of the species of the orders to which they belong are marine. Considering the crustaceans alone, I can see no reason for supposing that the fauna of the caves of Kentucky and Indiana has been derived from any other source than the recent fauna of the surface of the neighboring region.

17. *United States Commission of Fish and Fisheries. Part II. Report of the Commissioner for* 1872 *and* 1873, *with Supplementary Papers.* 8vo, cii, 808 pp., with 37 plates and four maps. Washington, D. C., 1874. Received April, 1875.—In this very valuable report Professor S. F. Baird, the Commissioner, has presented us with a vast amount of information concerning the fresh-water fishes and fisheries of the United States and other countries, together with extended accounts of what has been accomplished in the way of artificial propagation of the fishes, and their introduction into rivers and lakes in all parts of the United States, whether by the commissioners of the various States, or under his own authority. The scope of the work may be seen from the following summary of contents:—Inquiry into the decrease of the food fishes; Action in regard to propagation of food-fishes (shad,

Maine salmon, Rhine salmon, California salmon, white-fish); Multiplication of fish in general, including a history of fish-culture and the various modes of propagating fish of different kinds. The preceding subjects are treated in the special report of the Commissioner. In thirty-four supplementary papers the following subjects are treated: The fisheries of the Great Lakes, and the species of Coregonus, or white-fish, by J. W. Milner; Descriptions of the North American species of salmon and trout, by George Suckley; The Salmon of the Danube, or the hucho, and its introduction into American waters, by B. Hessel; Report of operations, in 1873, at the U. S. hatching establishment on the McCloud River, and on the California Salmonidæ generally, by Livingston Stone; several interesting papers on the salmon and trout of other parts of the United States and Canada, by Mr. Stone, Chas. Lanman, C. G. Atkins, A. C. Hamlin, A. S. Adams, Dr. H. C. Yarrow, and others; several valuable papers giving accounts of the shad and the shad fisheries in all the principal rivers, both southern and northern, together with accounts of the various experiments made in the artificial increase of shad; and a detailed account by Mr. Milner of the mode of propagation and extensive introduction of young shad into the various rivers and lakes, by the U. S. Commissioner; also a detailed history of fish culture, both in Europe and this country, and descriptions of the various methods and apparatus. Appendix E. is devoted to papers on obstructions preventing fishes from ascending various rivers, and on fish-ways, &c, by C. G. Atkins, E. M. Stilwell, M. C. Edmunds, M. McKennie, J. F. Ingalls, and J. W. Milner. Appendix F is devoted to invertebrate Zoology, and includes a descriptive paper on the higher fresh-water Crustacea of the northern United States, and one on crustacean parasites of fishes, with descriptions and figures of a number of new species, by S. I. Smith; a synopsis of the fresh-water leeches, by A. E. Verrill; Sketch of the invertebrate fauna of Lake Superior, by S. I. Smith; Food of fresh-water fishes, by S. I. Smith; Natural History of the gourami, by Theodore Gill; Notes on the grayling (*Thymalus*) of North America, by J. W. Milner. Also several miscellaneous papers relating to temperatures of the Gulf of Mexico; Fish-culture; Bibliography of reports relating to the fisheries, etc. The plates illustrate a great variety of traps, pounds, weirs and other devices for capturing fishes; fish-ways; hatching apparatus; crustacea; aquatic insect larvæ; the gourami, etc. V.

18. *On some Parasitic Worms*, by Dr. LEIDY.—Dr. Leidy has identified the *Ascaris mystax* as an intestinal worm of a Bengal tiger. The species has been found "in many other feline species, including the domestic cat and lion."

A long thread-worm from an apple, submitted to Dr. Leidy, was found by him to be the *Mermis acuminata*, a species that is parasitic in the larvæ of many insects, including the codling-moth, or fruit-moth, of the apple. He states that "twenty-five years ago he described a worm (Proc. 1850, 117) belonging to the collections of the Academy, and labelled it as having been obtained from a child's mouth, which was evidently the same species. It having been in a child's mouth, is probably to be explained by supposing that the child had eaten an infected apple."

Two specimens of a tape worm, Tænia, were obtained by Dr. Leidy from the stomach of an Australian Wombat. He names it *T. bipapillosa*. An Australian Whallabee afforded, from its peritoneal cavity, a *Filaria*, which he has named *F. spelæa*.—*Proc. Acad. Nat. Sci.*, 1875, 14, 17.

19. *Jahresbericht der Commission zur wissenschaftlichen Untersuchung der deutschen Meere in Kiel, für die Jahre* 1872, 1873, *II und III Jahrgang*. Large 4to, with 12 plates and a chart. Berlin, 1875.—This report is a valuable contribution to the physical and biological history of the European seas. It consists of five parts. The first, upon the Physics of the Seas, is by Dr. H. A. Meyer. It discusses very fully the physical features of the waters of the North Sea, observed during the cruise of the "Pommerania," and especially the temperature and specific gravity. The instruments used are described and figured, and the chart of the North Sea gives the track of the expedition and the location of the various stations. The waters near the coasts of England, Scotland, and Norway were explored, as well as those of Holland. The second part, by Prof. Oscar Jacobsen, relates to the chemical composition of the sea-waters. The third part consists of a report on the marine botany, by Dr. P. Magnus. He gives a catalogue of the algæ, with their distribution, etc., and two plates. The fourth part is a report on the Diatomaceæ, by Adolf Schmidt; it is illustrated by three beautiful plates. The fifth part is devoted to zoology. It includes a report on the Rhizopods, by F. E. Schulze, with figures; on the Sponges, by Oscar Schmidt, with a plate; on Cœlenterata, with a plate, by F. E. Schulze; on Echinoderms, by Karl Mobins and Butchli; on Worms, with a plate, by Karl Mobins. Quite a number of new forms are described in several of the groups. Among the new Rhizopods there is a new genus and species called *Astrodiscus arenaceus*, which appears to be very closely related to a singular species,* evidently of the same genus, which occurs very abundantly on the sandy bottoms off the New England coast, in fifteen to forty fathoms. We have dredged it during the four seasons past in the Bay of Fundy, off Casco Bay, in Vineyard Sound, and off Fisher's Island. It consists of a flat, sand-covered, disk-like body, with a variable number (usually five to nine) of irregular projections radiating from the periphery, and emitting, during life, irregularly branching pseudopodia. Our species grows to nearly a fourth of an inch in diameter, and scarcely differs from the figures of *A. arenaceus*, except in being usually covered by coarser grains of sand, and in having the projections a little more irregular, and often branched.

V.

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* See also the writer's Report on Invertebrata, in First Report of U. S. Commissioner of Fish and Fisheries, p. 503, 1873.

20. *Illustrated Catalogue of the Museum of Comparative Zoology.* No. VIII. *Zoological Results of the Hassler Expedition.* II, *Ophiuridæ and Astrophytidæ, including those dredged by the late Dr. Wm. Stimpson;* by THEODORE LYMAN. 34 pp., with cuts and five plates. Jan., 1875.—This work contains a list of 76 species collected by the Hassler expedition, and by Dr. Stimpson, in the Gulf of Mexico. Of these, 19 species were undescribed. Our knowledge of the distribution of the known species, both geographically and in depth, is much extended by this catalogue, while several genera are added to the West Indian fauna, among which is the northern genus *Ophioscolex*, of which a new species (*O. Stimpsonii*) is described, from 240 fathoms off Sombrero Key. The other new species belong to *Ophiomusium*, *Ophiozona*, *Ophioceramis*, *Ophiacantha* (three species), *Ophiomitra*, *Amphiura* (four new species, with notes on known species), *Ophiocnida*, *Ophioplax* (gen. nov.), *Astrotoma* (gen. nov.), *Astroschema*, *Astrophyton* (two species). An analytical table of the six recognized genera of simple armed Astrophytons is given on page 26, and also of the five species of *Astroschema*. V.

21. *The Elements of Embryology;* by M. FOSTER and FRANCIS M. BALFOUR. 8vo, 272 pp., with 71 cuts. Macmillan & Co., 1874.—In this work the authors have given a very complete and detailed account of the development of the chick, from the egg up to the period of hatching. The work contains a large amount of original matter, and the different views on many points are freely discussed. At the end a chapter is devoted to practical instructions for studying the embryology of birds. The book forms an excellent introduction to the general study of embryology, as well as a very useful manual for the embryology of birds. V.

22. *Nature and Life; Facts and Doctrines relating to the Constitution of Matter, the new Dynamics, and the Philosophy of Nature;* by FERNALD PAPILLON; translated from the second French edition by A. R. Macdonough. New York: D. Appleton & Co., 1875.—This book consists of a series of essays, which, in addition to the general subjects indicated in the title, discuss the constitution of Living Beings; the relations of Life to Light, Heat, Electricity, Odors, Medicaments, Ferments, etc. There are also essays on Animal Grafts and Regenerations; Great Epidemics; the Physiology of Death; Heredity in Physiology, in Medicine, and in Psychology. These essays are written in a popular style and cannot fail to interest and instruct a large class of intelligent readers.
V.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *On Ancient Cave-dwellings in Kentucky;* by F. W. PUTNAM.—That some of the caves were used as places of, at least, temporary residence, was conclusively shown by my exploration of Salt Cave, which proves important in revealing a new phase in American archæology. This cave approaches the Mammoth Cave in the size of its avenues and chambers. Throughout one of the

principal avenues, for several miles, were to be traced the ancient fire-places both for hearths and lights. For the latter purpose, small piles of stones were made with a hole in the center of the pile to receive the bundle of dried fagots perhaps smeared with grease. Bundles of these fagots, tied up with twisted bark, were found in several places in the cave; and canereeds, probably the remains of ancient torches of the same character with those found in the Mammoth, Short, and Grand Avenue Caves, were also very abundant.

The most important discovery in this cave, however, was made in a small chamber, about three miles from the entrance, first noticed by my guides, Messrs. Cutlip and Lee. On the dry soil of the floor were to be seen the imprints of the sandaled feet of the former race who had inhabited the cave, while a large number of cast-off sandals were found, neatly made of finely-braided and twisted leaves of rushes.—*Eighth Annual Rep. Peabody Museum of Archæology and Ethnology*, Cambridge, 1875.

2. *The catastrophe of the Zenith.*—On the morning of the 16th of April last, under the auspices of the Academy of Sciences, the *Zenith*, containing the three experienced aeronauts, Captain Sivel, Crocé-Spinelli and Gaston Tissandier, and well equipped for scientific work, started on its ascent from the gas works of La Villette, Paris. By 1 o'clock, at noon, they had reached an altitude exceeding 5,000 meters, the barometer marking a pressure of 400 millimeters, and the thermometer 5° C. They had oxygen in bags for breathing in the upper rarefied air, and found it very beneficial. At 1^h $20'$ the barometer marked 320 mm., showing an altitude of 7,000 meters; the temperature was $-10°$ C., and soon afterward 7,400, with the temperature $-11°$ C. Sivel and Crocé were already pale and very feeble. By mutual consent Sivel with his knife cut the cords which kept closed three sacs of ballast of 25 kilograms each that were hanging outside. The three sacs emptying themselves, the balloon ascended rapidly, and near 1^h $30'$ all three of the aeronauts had fainted. Tissandier, as his consciousness was leaving him, read from the barometer 280 mm., but was already too much paralyzed to speak out his thought—8,000 meters. Tissandier and his friends partially revived, as the balloon was making a very rapid descent; but again all became asphyxiated. The survivor supposes that more ballast was probably dropped by one of them to prevent a fatal descent, and up the balloon went. At 3 o'clock the balloon was again descending rapidly, and Tissandier became conscious; and at 4 o'clock it struck the earth at Ciron near Le Blanche with a severe shock. Sivel and Crocé were dead, their faces black and their mouths full of blood. The greatest height reached, as indicated by the self-registering barometer, was 8,540 to 8,600 meters. The lessons taught to science are: that man cannot safely make a rapid balloon ascent to an altitude of 8,000 meters; that the only chance for reaching alive that altitude in a balloon is by making the rate of ascent above 7,000 meters very slow, giving 12 hours at least

to the next 1,000 meters, and a rate half as fast for the meters beyond; that better arrangements for carrying up air or oxygen to supply the breathers may be of service; that man reaches soon the upward limit of atmospheric investigation.

3. *Protracting Sextant.*—At a recent meeting of the California Academy of Sciences, Mr. T. J. Lowry, of the U. S. Coast Survey, described a new instrument invented by him which may prove of the highest value in surveying, especially in hydrographic surveying. It is based upon the principles of the common sextant, but by it *two* adjacent angles can be at once measured by one observer. Immediately after the observation, moreover, the instrument without change can be laid as a protractor upon the chart, and the place of the observer pricked down, the principle involved being that of the "three point problem."

4. *Mean Height of Europe.*—Dr. Gustav Leipoldt, in a recently published work on the "Mean Height of Europe," after an elaborate calculation founded on a broad basis of measurement, concludes that it is 296·838 meters, 92 meters higher than the calculation of A. von Humboldt, who indeed made out the average altitude of all the land on the earth to be about 308 meters. The mean height of Switzerland Leipoldt makes to be 1299·91 meters, while that of the Netherlands is only 9·61 meters. That of Great Britain is 217·70. Further interesting details will be found in the April number of Petermann's Mittheilungen.—*Nature*, April 15.

5. *Smithsonian Report for* 1873.—The papers in the Appendix to this Report, which give it special scientific value, are the following: Biographies of Babbage, Agassiz, Torrey, G. Gibbs; J. C. Dalton on the origin and propagation of disease; Helmholtz on mathematical theories; Clerk Maxwell on action at a distance; B. A. Gould on the Cordoba Observatory; E. Mailly, estimate of the population of the world; A. Morin on warming and ventilating occupied buildings; E. L. de Forest, additions to a memoir on methods of interpolation applicable to the graduation of irregular series; Schumaker on the Kjökken-Möddings of N. W. America; H. Berendt on a grammar and dictionary of the Cavil language; H. Gilman on the mound-builders and platyonemism in Michigan; T. M. Perrine on the antiquities of Union Co., Illinois; W. H. Dall on explorations on the western coast of N. America; W. M. Pierson on the discovery of a large meteorite in Mexico; F. R. Brunot on the habits of the Beaver; W. S. Jevons on a national library; and a list of Prize Questions issued by Scientific Societies.

6. *Expedition for the Geological and Geographical Survey of the Territories, for* 1875.—The amount appropriated by the last Congress for the U. S. Geological and Geographical Survey of the Territories under the direction of F. V. Hayden was $75,000 for field work, and $20,000 for engraving illustrations, maps, &c. Six parties will take the field in various portions of Colorado about the first of June. Mr J. T. Gardner will carry the primary triangulation of the area between lat. 36° 45′ and 39° 15′; long.

104° 30′ and 109° 30′. Mr. A. D. Wilson, chief of field party No. 1, will survey an area in the southern part of Colorado, with a portion of northern New Mexico lying between lat. 36° 45′ and 37° 45′, long. 105° and 108°. Mr. Henry Gannet, in charge of field party No. 2, will occupy the area between lat. 38° and 39° 15′ and long. 108° and 109° 30′.

Field party No. 3 will survey the area in the southwestern Colorado between lat. 36° 45′ and 38° and long. 108° and 109° 30′. Field party No. 4 will complete unfinished areas in central and eastern Colorado. The photographic party under Mr. Jackson will make a study of the ancient rivers in southern and southwestern Colorado and also some special studies of remarkable mountain scenery in various portions of the Territory.

It is expected that at least four of the six sheets of the Colorado atlas will be completed this season, with portions of the other two. The maps will be issued as geological as well as topographical maps, in the final atlas.

7. *National Academy of Sciences.*—The following is a list of the papers read at the session of the National Academy of Sciences, held on the 20th to the 22nd of April, 1875:

Results derived from an examination of the U. S. Weather Maps for 1872–3–4; E. Loomis.

Notes on observations of the transit of Venus; G. Davidson.

On an improvement of the present civil or Gregorian calendar; J. P. Bradley.

Results of experiments on the set of rectangular bars of wood, iron and steel, resulting from a transverse stress; W. A. Norton.

Note on Goldschmid's Aneroid Barometer; J. E. Hilgard.

Progress of Second Geological Survey of Pennsylvania; J. P. Lesley.

Orography of the Catskill group of mountains; A. Guyot.

Report of the Committee on Weights, Measures and Coinage; F. A. P. Barnard.

On the observations of contacts in transits of Venus and Mercury; S. Newcomb.

New formula for the deflections of rectangular bars or beams, resting on supports and subjected to a transverse stress: W. A. Norton.

Size of the brain in extinct mammals; O. C. Marsh.

Account of researches in Solar Physics made at the Allegheny Observatory during the past four years; S. P. Langley.

Use of the Stereoscope in the study of Solid Geometry; James D. Warner.

On the interpolation of a change of sign by passage through infinity of a mathematical function expressive of a physical phenomenon; a curious particular case in the theory of tides; J. G. Barnard.

Discussion of the Laws of Atmospheric Circulation, by Prof. James H. Coffin (now deceased); Selden J. Coffin.

Influence of Arsenical compounds upon Vegetation; Wm. McMurtrie.

Preliminary account of results of a Magnetic Survey, made at the charge of the Bache Fund; J. E. Hilgard.

Relations of some of our ancient Fossil Fishes to living Forms; J. S. Newberry.

On areas of Cold; by A. Woeikof.

8. *Mastodon of Otisville, Orange County, N. Y.*—Prof. Marsh has secured, for the Peabody Museum of Yale College, the skeleton of a large mastodon, exhumed by Mr. A. Mitchell on his grounds at Otisville, seventy-five miles from New York and within a mile and a half of the Erie Railroad. The bones were found on and in clay beneath a deep bed of muck, and are in an excellent state of preservation. This Otisville mastodon is the sixth that has been found in the swamps of Orange County.

9. *Statistical Atlas of the United States, based on the results of the Ninth Census*, 1870, with contributions from many eminent men of science and from several departments of the Government. Compiled, under authority of Congress, by FRANCIS A. WALKER, M.A., Superintendent of the Ninth Census, Professor of Political Economy and History in the Sheffield Scientific School of Yale College. In large folio. (Julius Bien, Lith.)—The publication of Parts 2 and 3 of this Atlas has been already announced on page 74 of this volume. Part 1 completes the great work, and in a most satisfactory manner. Among its maps there is one of great interest showing the distribution of wood-land in the United States, by Prof. Wm. H. Brewer; another giving a hypsometric view of the surface, prepared from data furnished by Prof. Guyot and C. A. Schott of the U. S. Coast Survey; others illustrating the meteorological characteristics of the country; one giving the dis-distribution of the Coal fields, compiled by Prof. C. H. Hitchcock; and one which is an excellent geological chart of the United States, 19 by 28 inches in size, by Profs. C. H. Hitchcock and W. P. Blake. This geological chart has been prepared with care and is beautiful in its lithography and coloring. In fact, the whole work is highly creditable to the photolithographic establishment of Mr. Bien. The maps are accompanied by many pages of text giving explanatory, descriptive and statistical information. We defer to another number a more extended notice of the Atlas.

10. *Popular Science Library.*—Under this title D. Appleton & Co. are publishing a valuable series of small works in Science. The following have been issued: *Health*, by EDWARD SMITH, F.R.S.; *The Natural History of Man*, by Prof. A. DE QUATREFAGES; *The Science of Music*, by SEDLEY TAYLOR; *Outline of the Evolution Philosophy*, by Dr. E. CAZELLE, with an Appendix by E. L. YOUMANS, M.D., on *Herbert Spencer and the Doctrine of Evolution*, giving a brief exposition of the views and claims of Spencer.

11. *New York Academy of Sciences.*—The Lyceum of Natural History of New York has recently changed its name to New York Academy of Sciences.

12. *Geological Society of London.*—Dr. F. V. HAYDEN has been recently elected Foreign Correspondent of the Geological Society of London.

Life of Murchison, by Archibald Geikie. 2 vols. London, 1875. (Murray.)

Statement and Exposition of certain Harmonies of the Solar System, by Stephen Alexander, Prof. Astron. Coll. of New Jersey. 96 pp. 4to. 1875. Smithsonian Contributions to Knowledge, No. 280.

Half-hour Recreations in Natural History of Estes and Laureat. Boston. 72 to 80 pages each.—Half-hours with Insects, Parts 6 and 7, by A. S. Packard, Jr.—The Glacial Epoch of our Globe, by Alexander Braun.—The Sun and the Earth, by Prof. Balfour Stewart, F.R.S.; Force electrically explained, by J. W. Phelps.—The first volume of these Half-hour Recreations, numbering 478 pages, has been issued by the publishers in a handsome form.

Catalogue of the Fishes of the East Coast of North America, by Theodore Gill. Washington, 1875. 52 pp. 8vo.—Smithsonian Miscellaneous Collections.

Astronomy by J. N. Lockyer, being No. VII of Science Primers, edited by Professors Huxley, Roscoe and Balfour Stewart. 120 pp. 16mo, with many fine illustrations. 1875. (D. Appleton & Co.)

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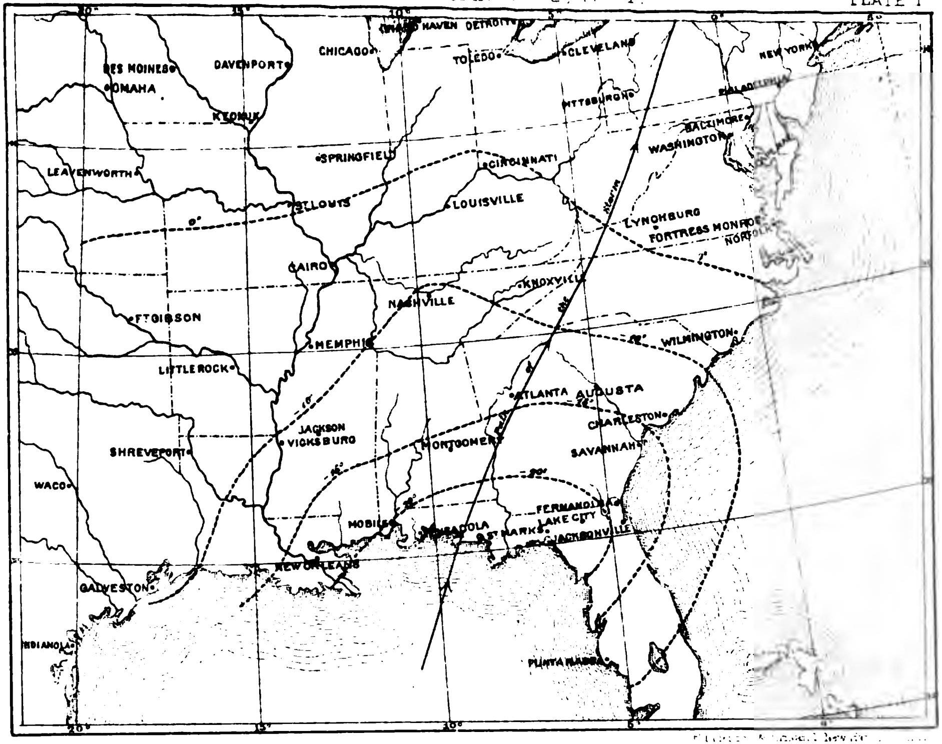

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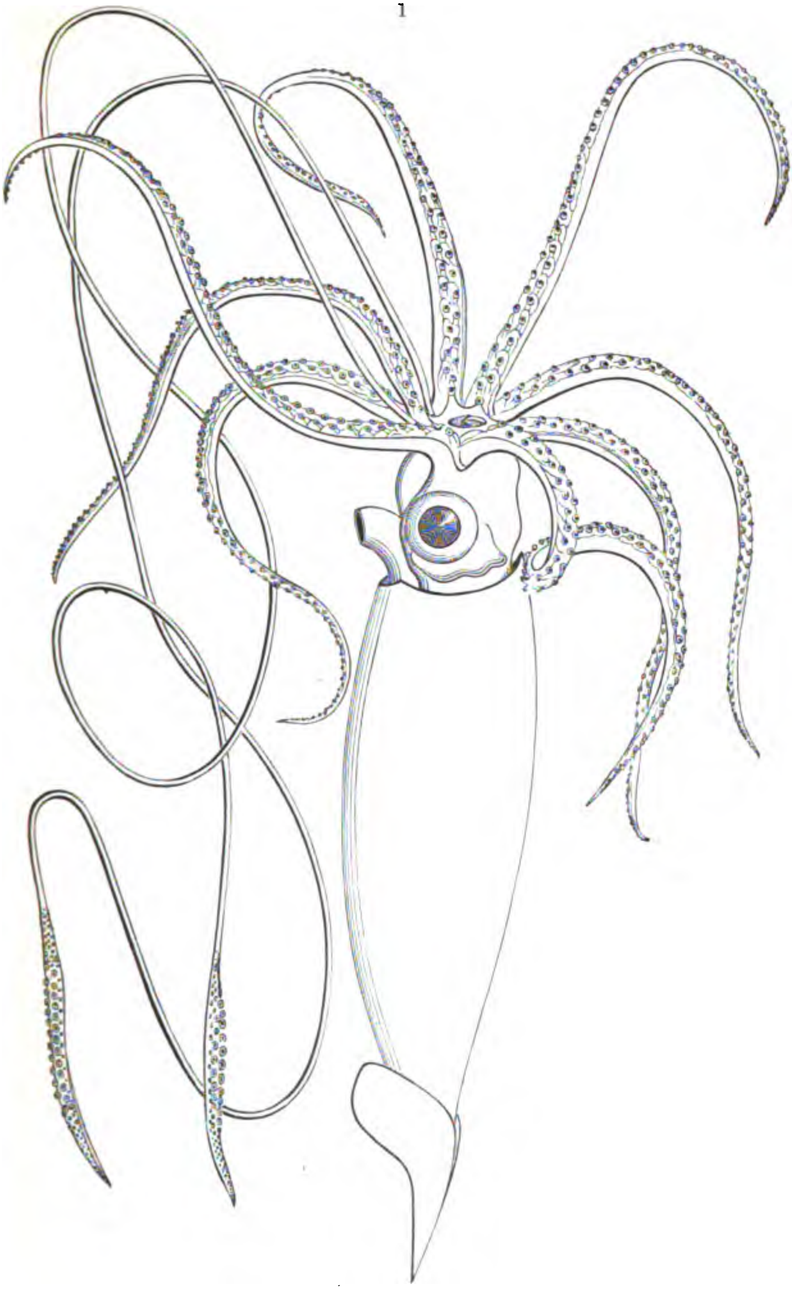

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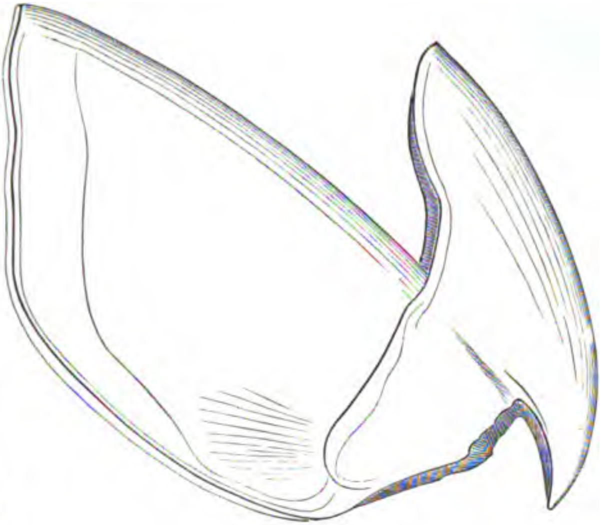

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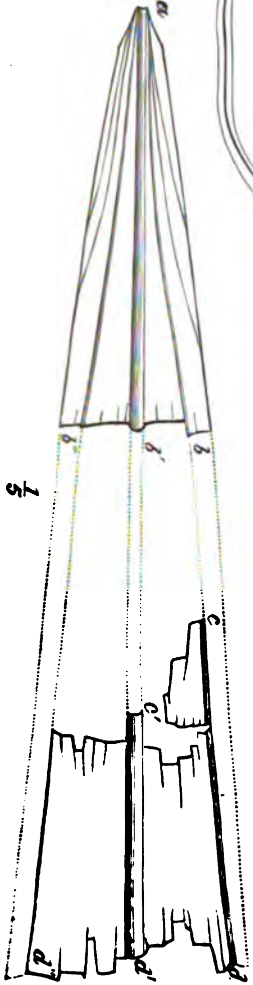

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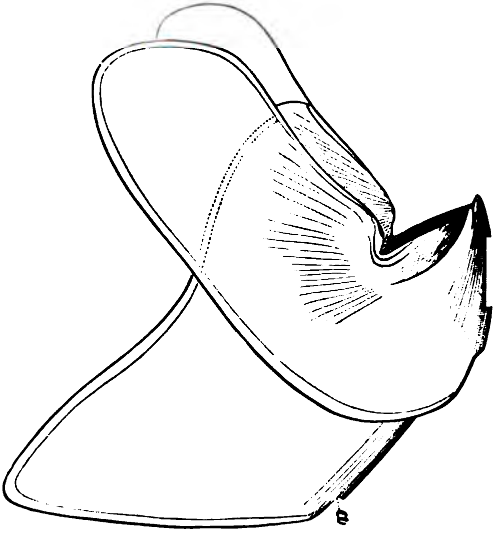

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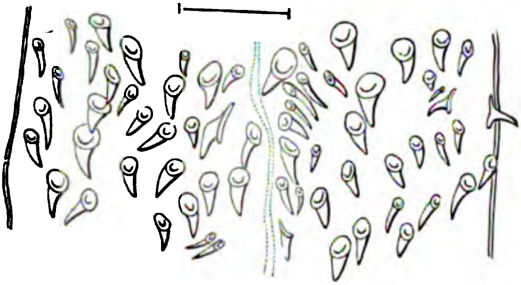

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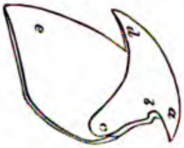

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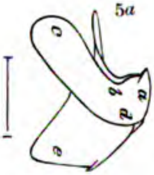

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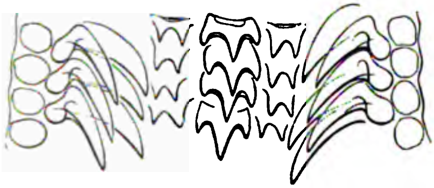

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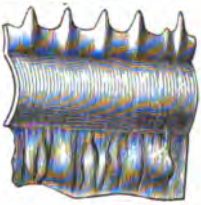

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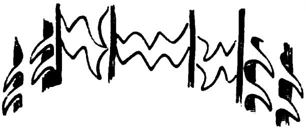

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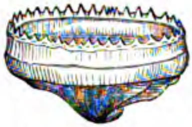

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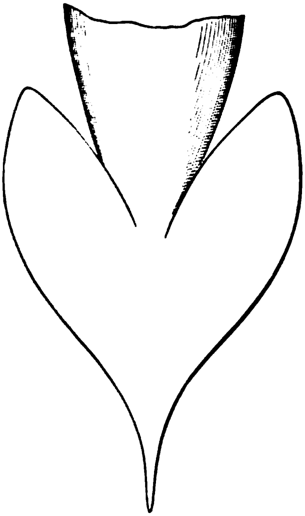

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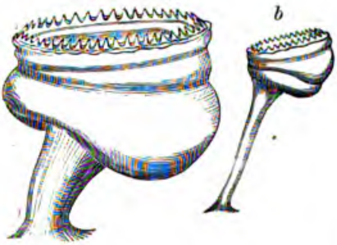

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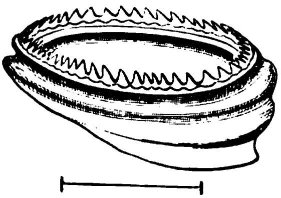

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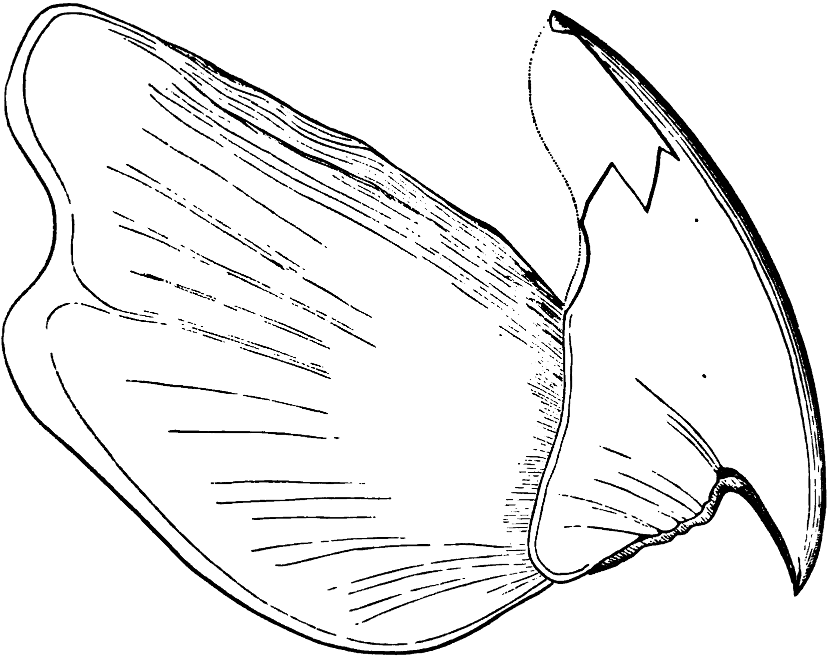

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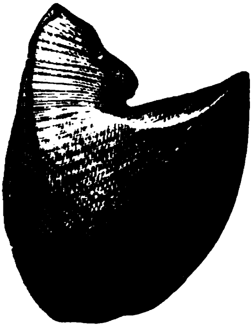

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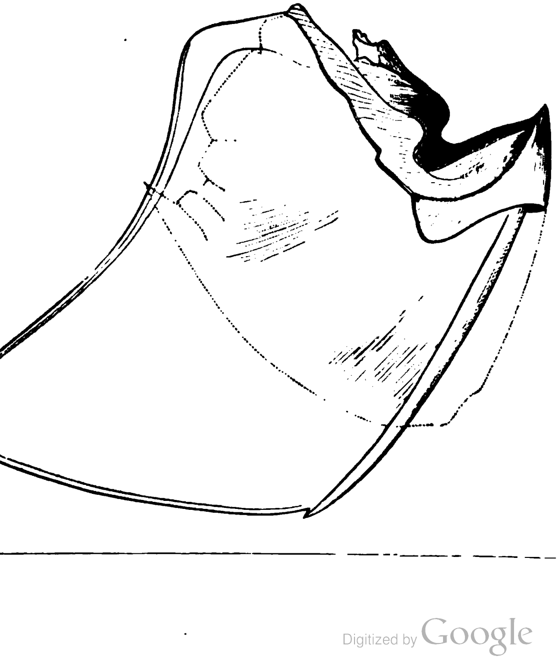

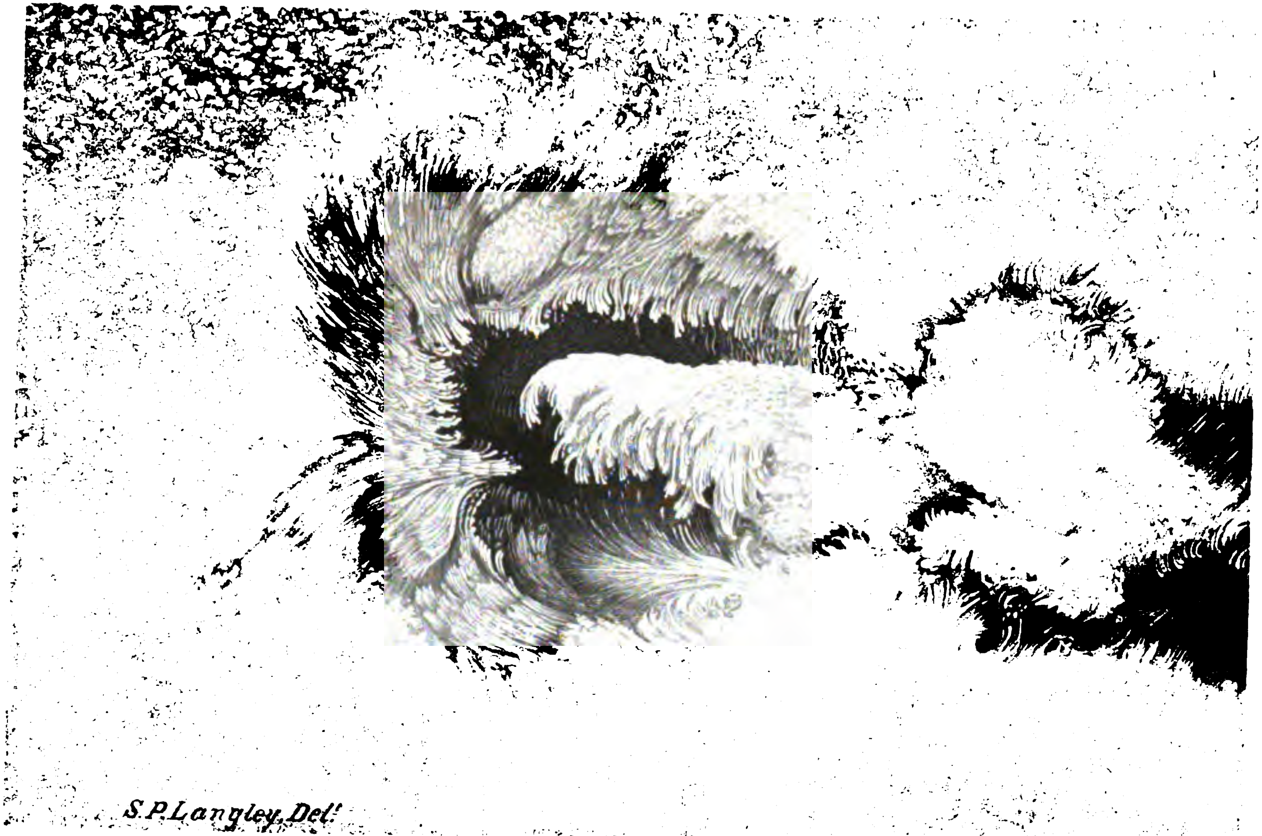
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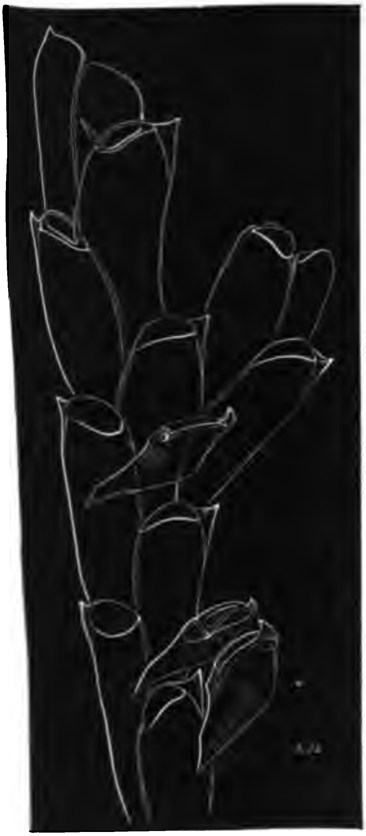

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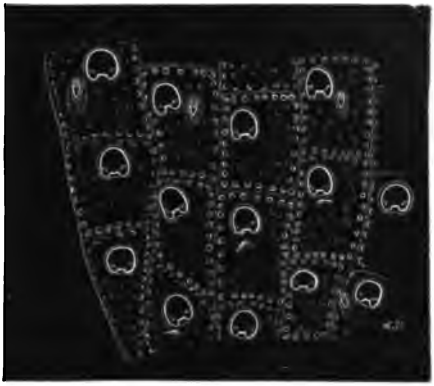

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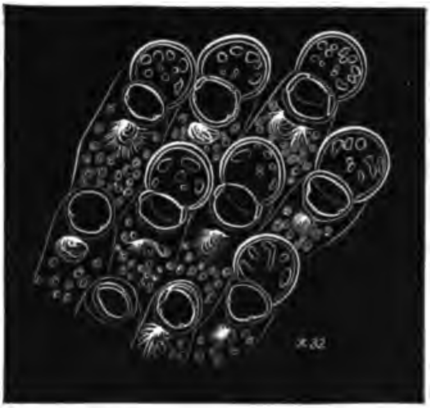

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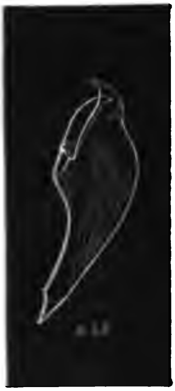

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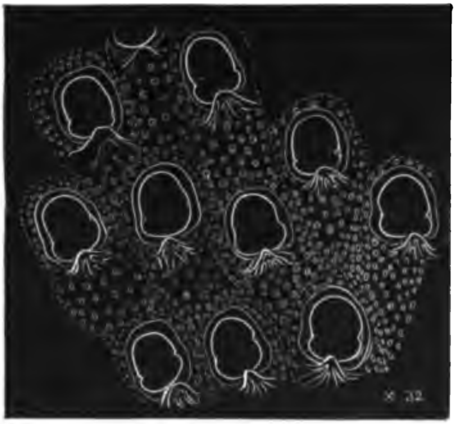

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THE

AMERICAN

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[THIRD SERIES.]

Art. I.—*Results derived from an examination of the United States Weather Maps for* 1872, '73, *and* '74; by Elias Loomis, Professor of Natural Philosophy in Yale College. Third paper. With Plate I.

[Read before the National Academy of Sciences, Washington, April 20, 1875.]

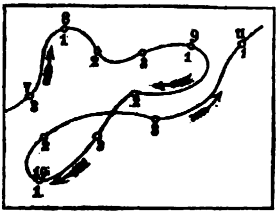
In this Journal for July, 1874, I gave the average direction of storm paths and the velocity of progress for each month of the year as deduced from 314 cases in 1872 and '73. I have made a similar examination of the weather maps for 1874, and now present the results derived from 171 cases for that year, together with the average results derived from combining all the observations of the three years, making in the aggregate 485 cases.

	Results for 1872 & '73.		Results for 1874.		Results for 3 years.	
	Course of storm.	Velocity in miles per hour.	Course.	Velocity.	Course.	Velocity.
January	N. 85° E.	28·5	N. 74° E.	23·0	N. 81° E.	26·7
February	76	31·0	70	33·9	74	32·0
March	79	30·8	86	29·8	81	30·5
April	74	25·6	68	31·4	72	27·5
May	78	24·2	76	22·2	77	23·5
June	93	21·2	81	22·4	89	21·6
July	102	24·4	88	25·9	97	24·9
August	85	17·7	85	19·9	85	18·4
September	78	22·8	70	23·1	75	22·9
October	69	24·4	84	28·5	74	25·8
November	80	28·3	74	30·3	78	29·0
December	84	27·6	81	32·7	83	29·3
Year,	N. 82° E.	25·6	N. 78° E.	26·9	N. 81° E.	26·0

The average course of the storms in 1874 was four degrees more northerly than for the two preceding years, and the average velocity was 1·3 miles per hour greater; but the change both in the direction and velocity depending upon the season of the year was similar to that before noticed. According to the combined results derived from the three years observations, the course of storms is most southerly in July, and most northerly in April and October, the mean difference between the extreme months amounting to 25°. The velocity of progress is greatest in February, and least in August, the former exceeding the latter by 74 per cent. In only one instance was the course of a storm for an entire day directed toward a point west of north. This occurred on the 18th of April, in the case of a storm which originated in the Gulf of Mexico, and whose course for one day was 13° west of north. The most southerly direction for the year occurred in the case of the storm of Aug. 21st, whose course for one day was N. 163° E., or S. 17° E., a directional most exactly opposite to that of the storm of April 18th.

The most rapid progress of a storm center observed in one day occurred Feb. 22nd, being 1280 miles, or 53·3 miles per hour; and the least velocity occurred Aug. 21st, being 228 miles in 24 hours, or 9·5 miles per hour.

These are the results derived from observations made at intervals of 24 hours. If we make the comparison at intervals of eight hours, we shall find much greater variations in respect to both direction and velocity. According to the monthly maps, published under the direction of the Chief Signal Officer, from the 7th to the 11th of May, 1874, the path of a storm center situated about 300 miles west of Lake Superior was such as is represented by the annexed diagram; where the figures above the curve line show the days of the month, and those below 1, 2, 3, indicate respectively the 7.35 A. M., 4.35 P. M. and 11 P. M. observations. It will be perceived that the direction of the storm's progress changed more than 180° in 24 hours, and on the morning of the 11th the center of the storm was distant less than 100 miles from its position on the morning of the 9th. If, then, we regard the actual motion of the storm's center from hour to hour, as remarked in my former article, we find that the storm path may have any direction whatever, and the velocity of progress may vary from 15 miles per hour toward the west, to 60 miles per hour toward the east.



Diurnal inequality in the progress of storms.

In order to ascertain whether the progress of storms is uniform throughout the entire day, I availed myself of the monthly maps issued by the Signal Service Bureau. These maps show the position of the storm centers for three hours of the day, viz: at 7.35 A. M., which is designated by (1); at 4.35 P. M., which is designated by (2); and at 11 P. M., which is designated by (3). On each storm track, the distance from 1 to 2, from 2 to 3, and from 3 to 1 (of the next day), was carefully measured on a scale of inches, and these distances were afterward reduced to miles by computation. The total distance traveled by a storm center during each interval was divided by the number of hours in the interval, and thus the hourly velocity of progress obtained for three different portions of the day. The following table shows the average results of this comparison for the storm paths for 1873 and 1874. The numbers given in column second represent the average hourly velocity in miles for the interval from 7.35 A. M. to 4.35 P. M.; the numbers in column third for the interval from 4.35 P. M. to 11 P. M.; and the numbers in column fourth represent the velocity from 11 P. M. to 7.35 A. M. of the following day.

	1–2	2–3	3–1		1–2	2–3	3–1
Jan.	26·6	37·3	28·2	July	25·2	32·6	28·2
Feb.	28·6	37·0	28·9	Aug.	24·2	28·6	20·8
March	27·2	33·6	27·2	Sept.	23·9	30·2	24·5
April	22·5	25·9	22·9	Oct.	22·9	29·6	23·5
May	19·2	25·6	19·5	Nov.	33·6	40·6	29·3
June	22·5	25·9	22·2	Dec.	33·3	37·0	29·9
				Year,	25·9	31·9	25·4

From this table it appears that the average velocity of storms from 4.35 P. M. to 11 P. M. is about 25 per cent greater than it is for the remainder of the day. This excess varies for the different months, ranging from 14 to 32 per cent, but in each month the most rapid progress occurs during the same portion of the day.

This diurnal inequality is doubtless connected with a diurnal inequality in some one and perhaps several of the other meteorological elements. Its maximum value apparently occurs about 7 P. M. Now this is not the hour of the maximum force of the wind, nor of the maximum or minimum temperature, and hence it may be presumed that the inequality does not depend directly upon the velocity of the wind nor upon the absolute temperature. This hour (7 P. M.) is, however, the time when the temperature of the day is declining most rapidly, and this condition must be favorable to a rapid extension of the rain-area in front of a storm. When, at a given place in front of a storm center, a system of causes is in operation which is soon to result in a fall of rain, the ordinary change of temperature at evening must accelerate the commencement of the rainfall; that is, must extend the rain-area more rapidly, and this, we have seen in a former article (this Jour., vol. viii, p. 3), must increase the velocity of the storm's progress.

This extension of the rain-area in front of a storm does not perhaps necessarily imply an increase in the average rainfall for that part of the day; nevertheless, I have endeavored to determine whether there is a diurnal inequality in the fall of rain. For this purpose I first examined the observations made at Girard College, Philadelphia, from 1840 to 1845, but found only seven months (viz: Jan., March, June, July, Aug., Sept., Oct., 1843) in which the self-registering rain-gauge is reported to have been in good condition. In the following table, column second shows the aggregate amount of rain at Philadelphia for each hour of the day during these seven months.

Rainfall at Philadelphia.

Hour.	Rain, Inches.	Average.	Hour.	Rain, Inches.	Average.
Midnight	1·633	0·869	Noon	0·366	0·914
1 A.M.	0·748	·917	1 P.M.	·783	·861
2	·398	·826	2	1·438	1·036
3	·666	·636	3	0·869	1·152
4	·686	·677	4	1·727	1·265
5	·684	·731	5	0·945	1·234
6	·949	·716	6	1·346	1·580
7	·671	·733	7	1·285	1·310
8	·588	·822	8	2·598	1·206
9	·771	·802	9	0·375	1·165
10	1·133	·741	10	·427	1·235
11	0·848	·780	11	1·140	0·865

As these numbers show considerable irregularities, I have taken the average of each successive five numbers in column second and set down the results in column third. These resulting numbers show a pretty steady progress from a minimum at 3 A. M. to a maximum about 6 P. M., and from thence a steady decline to the following minimum. The maximum is nearly 2½ times the minimum, although the former is made too small and the latter too great by the mode of reduction employed. It seems probable, therefore, that the diurnal inequality in the rainfall, as indicated by these observations, is a law of the Philadelphia climate, and we may presume, therefore, that it is a law for the immediate vicinity of Philadelphia.

I next examined the observations made at the signal service stations for Sept., Oct., and Nov., 1872, these being the only months for which the observations have been fully published, and I confined the comparison to those stations which are north of lat. 35°. The total rainfall for a month at all the stations north of the parallel of 35° was determined for each of the three intervals into which the day is divided; these results were divided by the number of days in the month, giving the average daily fall for each of these intervals; and these last results were divided by the number of hours in the corresponding interval, giving thus the average hourly rainfall for all the stations for these three portions of the day. The following is the final result:

Hourly rainfall at the S. S. Stations.

	1—2	2—3	3—1
September	0·244 inch	0.308 inch	0·288 inch
October	·174	·200	·192
November	·155	·143	·180
Mean	0·191	0·217	0·220

These numbers indicate that the average rainfall for the stations employed is nearly uniform throughout the day. If, then, there is a decided diurnal inequality in the rainfall at Philadelphia, it seems probable that for other parts of the country the maximum takes place at a different hour, so that in taking the aggregate for a series of stations stretching across the continent, the total rainfall is nearly uniform for all hours of the day.

The British Government has published the hourly observations of rainfall for eleven months of the year 1874 at seven stations, viz: two in Ireland, two in Scotland, and three in England. From these observations it may be inferred that not only the absolute amount of rain, but also the time of maximum, depends very much upon the locality. In the aggregate for the seven stations there are indications of two daily maxima, one at 5 in the morning, and the other at 7 in the evening, with minima near noon and midnight; but since the fall of rain is dependent upon such a variety of causes, it is hazardous to draw general conclusions, except from a long series of observations.

Influence of rainfall upon the course of storms.

In a former paper (this Jour., vol. viii, p. 4) I endeavored to show the connexion between the velocity of a storm's progress and the extent of the rain-area on the eastern side of the storm. I have made a similar comparison of the observations of 1874, and now present the results. The number of cases suited to this comparison in 1874 was 80; and I have divided them into four equal classes, the first division embracing those cases in which the progress of the storm in one day was at least 855 miles; the second, those cases in which the progress was from 855 to 665 miles; the third, from 665 to 490; and the fourth embracing the cases in which the progress was less than 490 miles in one day. In the following table, columns one and two show the results heretofore published, derived from the observations of 1872 and '73; columns three and four show the results derived from the observations of 1874, while columns five and six show the results derived from combining the observations of the three years.

Obs. of 1872 and 73.		Obs. of 1874.		3 years obs.	
Velocity in miles per hour.	Extent of rain-area in miles.	Velocity.	Rain-area.	Velocity.	Rain-area.
38·8	590	42·7	740	40·1	640
28·5	548	30·7	609	29·2	568
21·6	503	23·7	611	22·3	539
14·5	365	17·0	535	15·3	422

Although the velocity of progress does not appear to be strictly proportioned to the eastward extent of the rain-area (for the observations are seldom sufficiently numerous to indicate precisely the extent of this area, and it is plain that there are other causes which contribute to influence the result), yet I think it is well established that an unusual extension of the rain-area is generally accompanied by a velocity of progress greater than the mean. The result of the three years' observations shows the average extent of the rain-area eastward from the center of the storm to be 542 miles. When the eastern extent of the rain-area is 100 miles greater than the mean, the hourly velocity of the storm's progress is increased 13·7 miles; but when the eastern extent of the rain-area is 100 miles less than the mean, the hourly velocity of the storm's progress is diminished 9·5 miles.

I have also determined the influence of the rain-area upon the direction of the storm's path, as shown by the observations of 1874, in the same manner as described in my former paper (this Jour., vol. viii, p. 5). The following table shows: first, the results heretofore published; second, the results derived from the observations of 1874; and third, the results derived from combining the observations of the three years.

Obs. of 1872 and 73.		Obs. of 1874.		Obs. of three years.	
Course of Storm.	Axis of Rain-area.	Course of Storm.	Axis of Rain-area.	Course of Storm.	Axis of Rain-area.
N. 40° E.	N. 53° E.	N. 53° E.	N. 54° E.	N. 44° E.	N. 53° E.
N. 116 E.	N. 118 E.	N. 100 E.	N. 109 E.	N. 111 E.	N. 115 E.

From the result of three years' observations, it appears that when the course of a storm is most northerly, the axis of the rain-area is inclined to the storm's path 9° toward the south; but when the course of a storm is most southerly, the axis of the rain-area is inclined to the storm's path only 4°. If, then, in any case we can learn the precise limits of the rain-area about a storm-center, we ought to be able to predict with considerable confidence the direction and velocity of the storm's progress.

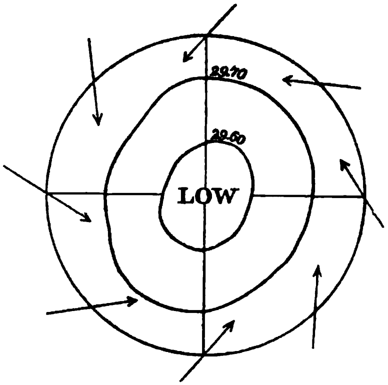
Influence of a neighboring area of high barometer upon the progress of a storm.

In order to determine the influence of a neighboring area of high barometer upon the progress of a storm, I selected all those cases in which a weather map showed both a storm center and an area of high barometer. I then divided the cases into eight classes, according to the direction of the center of high barometer from the center of low barometer. From the center of low barometer I drew eight radii, making with each other angles of 45°, and so situated that two of the octants should be bisected by a meridian line. These octants are designated by the terms, north, northeast, east, etc. A large sheet of paper was then ruled with appropriate divisions for each of the octants, and when the area of high barometer was on the north side of the storm center, the velocity and direction of the storm's path were entered in the column headed north. I proceeded in like manner with each of the cases in succession. An average was then taken of the directions and velocities in the several columns.

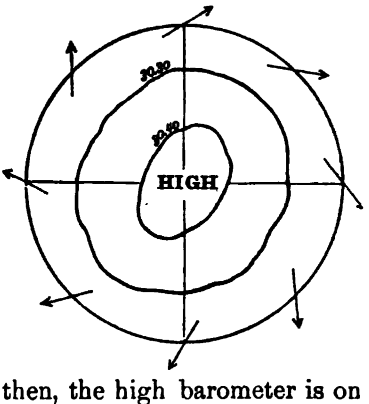
The following table shows the result of this comparison for 1872, '73 and '74. Column 1st shows the direction of the area of high barometer from the storm center; column 2d shows the number of cases employed; column 3d shows the average velocity of the storm's progress; and column 4th shows the direction in which the storm advanced.

Direction of high barometer.	No. of Cases.	Velocity of Storm.	Direction of Storm-path.
North	23	26·0	N. 58° E.
N.E.	39	26·4	54
East	90	25·4	83
S.E.	75	29·5	90
South	25	30·3	93
S.W.	20	26·1	81
West	37	28·8	70
N.W.	19	28·7	62

The influence of a neighboring area of high barometer upon the velocity of a storm's progress is not very decided, nevertheless the observations indicate that when the high barometer is on the east side of the storm, the velocity of the storm's progress is diminished eight per cent; and that the velocity is increased by about the same amount when a high barometer is situated on the south side of the storm.



The effect of an area of high barometer upon the direction of a storm's progress seems to be more decided, the course of the storm being most northerly when the high barometer is on the northeast side, and most southerly when the high barometer is on the south or southeast side; in each case the storm-path is deflected toward the center of high barometer.



By referring to my former article (this Journal, vol. ix, p. 2) it will be seen that on whichever side of a storm center an area of high barometer is situated, both systems tend to impress nearly the same direction upon the intermediate air. It seems, therefore, that the immediate effect of an area of high barometer must be to extend the area of the wind which belongs to that side of the storm, and probably also to increase the velocity of the wind upon that side of the storm which is toward the high barometer. If, then, the high barometer is on the southeast side of the storm's center, the south wind which belongs to that side of the storm should extend to a greater distance, and probably blow with increased force, which would cause increased rainfall upon that side, and the storm's path would incline in that direction. In like manner, if the high barometer were on the north side of the storm's center, the increased precipitation on that side should cause the storm's path to incline more to the northward.

It might be supposed that the same course of reasoning would lead us to conclude that a storm's progress should be most rapid when there is an area of high barometer on its eastern side. To this it may be answered, that although the fall of the barometer in such a case should be more rapid than usual, still, as the barometer starts from a point unusually high, the fall must continue for a proportionally longer time before the minimum is reached. The same consideration may in part explain why the progress of a storm is not as much accelerated by a high barometer on the northeast side as it is by a high barometer on the southeast side. In the former case, the high barometer is nearer to the track which the storm is to pursue, and there must be a greater fall of the barometer before the minimum is attained.

Form of the isobaric curves.

In my first article (this Jour., vol. viii, p. 11) I gave some comparisons illustrating the form of the isobaric curves about a storm center, as shown by the weather maps of 1872 and '73. I have made a similar comparison for the storms of 1874, and with the following results: The number of cases found suited to this kind of comparison was 75. In 35 of these cases (that is, 47 per cent of the whole) the major axis of the isobar measured was at least double the minor axis. In 15 cases (that is, 20 per cent of the whole) the major axis was at least three times the minor axis; and in 5 cases (that is, 7 per cent of the whole) the major axis was at least four times the minor axis. The average form of the isobars about a storm center may be said to be an irregular oval, whose length is nearly double its breadth. In order to give a more distinct idea of the form of these curves, I have selected the storm of March 29, 1873, and have represented several of the isobars on the accompanying chart, Plate I. It will be seen that the longer diameter of these curves is nearly three times the shorter diameter. The arrows show the direction of the wind at the signal service stations, and the force of the wind is partially indicated by the length of the arrows; a length of 0·4 inch indicating a velocity of at least 20 miles per hour; a length of 0·3 inch a velocity of from 10 to 20 miles; and a length of 0·2 inch a velocity less than 10 miles per hour.

An inspection of this map will show that the centrifugal force arising from the circulation of the wind around the storm center cannot be the principal cause for the fall of the barometer, for otherwise the form of the isobars would be more nearly circular.

With regard to the direction of the major axis of the isobars, there is but little tendency to uniformity; nevertheless, the most prevalent direction is about N. 35° E., which is almost identical with the result derived from the observations of 1872 and '73.

Great and sudden changes of temperature.

In a former paper (this Jour., vol. ix, p. 8) I called attention to the great and sudden changes of temperature frequently experienced in the United States and in other parts of the world, and suggested an explanation of this phenomenon. I will now present some additional facts bearing upon the same question. In the Report of the Chief Signal Officer for 1873 is given the maximum and minimum temperature of each day in 1873 at 40 stations in the United States and Canada. I have examined these tables to find all the cases in which the difference between the maximum and minimum of the same day amounted to at least 40°. The following table shows all the stations at which so great a difference was observed, and also the number of cases which occurred each month.

Diurnal Change of Temperature of 40° *and upwards in* 1873.

	Lat.		Lon.		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
	°	′	°	′													
Denver	39	44	104	58	3	5	5	2	2	6	4	3	4	*	1	2	37
Fort Sully	44	39	100	40	3	--	2	4	--	1	--	1	5	9	--	1	26
Cheyenne	41	12	104	42	1	1	--	1	--	4	3	--	2	3	3	2	20
Virginia City	45	0	112	3	2	--	*	*	*	2	1	*	*	1	2	3	11
Breckenridge	46	16	96	38	1	--	--	--	--	1	1	--	--	5	--	--	8
Fort Garry	49	52	96	58	--	--	--	--	4	--	3	--	1	*	*	--	8
San Antonio	29	25	98	25	--	--	--	6	2	--	--	--	--	--	--	--	8
La Crosse	43	48	91	27	3	2	1	--	--	--	--	--	--	1	--	--	7
Halifax	44	40	63	35	--	1	--	--	1	--	--	1	1	*	*	*	4
Geneva	42	53	77	5	--	--	--	--	--	2	*	*	1	*	--	--	3
Omaha	41	16	96	0	--	--	1	--	1	--	--	--	--	*	*	*	2
Chicago	41	52	87	38	--	1	--	--	1	--	--	--	--	--	--	--	2
Leavenworth	39	19	94	58	--	--	--	1	--	--	--	--	--	--	--	--	1
St. Paul	44	53	93	5	--	--	1	--	--	--	--	--	--	--	--	--	1
Morgantown	39	40	79	55	*	--	1	--	--	--	--	--	--	*	*	*	1
Oswego	42	28	76	35	1	--	--	--	--	--	--	--	--	*	*	--	1

An asterisk shows that observations for the month indicated are wanting.

The number of stations at which a difference of 40° between the maximum and minimum of the same day was observed in

1873, is 16, which is 40 per cent of the whole number of stations; and among the stations at which so great a difference was *not* observed, are Kingston, Toronto and Montreal, in Canada, and the summit of Mount Washington in New Hampshire.

In the Report of the Chief Signal Officer for 1874 is given the maximum and minimum temperature of each day in 1874 at 107 stations in the United States and Canada. The following table shows all the cases in which the difference between the maximum and minimum of the same day amounted to at least 40°.

Diurnal Change of Temperature of 40° *and upwards in* 1874.

	Lat.		Lon.		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
	°	′	°	′													
Colorado Springs	38	55	104	58	8	3	4	3	11	9	8	--	--	*	2	8	56
Denver	39	44	105	4	2	1	1	4	6	6	4	1	8	7	4	1	45
Cheyenne	41	12	104	42	--	--	--	--	6	5	7	4	5	2	3	1	33
Fort Sully	44	39	100	40	5	1	2	4	1	--	1	--	5	3	2	--	24
Breckenridge	46	11	96	17	1	4	1	--	3	1	--	--	4	1	1	3	19
Yankton	42	45	97	30	4	1	--	2	--	--	--	--	4	3	1	--	15
Geneva	42	53	77	5	2	--	--	1	4	4	*	*	*	--	--	--	11
San Antonio	29	25	98	25	3	1	--	5	--	--	--	--	--	--	--	1	10
Pembina	49	0	97	5	--	1	1	--	3	--	--	--	2	1	1	--	9
Fort Garry	49	52	96	58	--	2	--	2	1	--	--	--	--	1	1	--	7
Stayner	44	25	80	15	1	--	--	1	3	1	--	--	--	*	--	--	6
Fort Gibson	35	43	95	16	1	--	--	--	--	--	--	--	--	1	2	1	5
Chatham	47	1	65	30	--	2	2	--	--	--	--	--	--	--	--	--	4
Halifax	44	40	63	35	--	1	1	--	--	--	--	--	--	--	--	1	3
La Crosse	43	48	91	23	--	2	--	--	1	--	--	--	--	--	--	--	3
Morgantown	39	36	79	52	--	--	--	1	--	--	--	--	--	2	--	--	3
Leavenworth	39	19	94	58	--	--	--	--	--	--	--	--	--	--	--	2	2
Mt. Washington	44	16	71	16	2	--	--	--	--	--	--	--	--	--	--	--	2
Ottawa	45	22	75	45	--	--	--	--	1	1	--	--	--	--	--	--	2
Sydney	46	18	60	9	--	--	1	--	1	--	--	--	--	*	--	--	2
Wytheville	36	56	81	0	*	*	*	*	--	--	--	1	1	*	*	*	2
Albany	42	40	73	45	--	--	--	--	1	--	--	--	--	*	*	--	1
Buffalo	42	53	78	55	--	1	--	--	--	--	--	--	--	--	--	--	1
Burlington	44	29	73	15	--	--	--	--	--	--	--	--	--	--	--	1	1
Charleston	32	45	79	55	--	--	--	--	--	--	--	1	--	*	*	--	1
Davenport	41	32	90	38	--	--	--	--	--	--	--	--	--	--	1	--	1
Duluth	46	48	92	8	*	*	*	*	1	--	--	--	--	*	*	*	1
Eastport	44	55	66	54	1	--	--	--	--	--	--	--	--	*	*	--	1
Erie	42	7	80	10	1	--	--	--	--	*	*	--	--	--	--	--	1
Kingston	44	12	75	41	--	--	--	--	--	--	--	--	--	--	--	1	1
New London	41	22	72	9	*	*	--	--	1	--	--	--	--	*	*	*	1
Norfolk	36	51	76	19	--	--	--	--	1	--	--	--	--	*	*	--	1
Omaha	41	16	96	0	1	--	--	--	--	--	--	--	--	--	--	--	1
Portland, Oreg.	46	30	122	27	*	*	*	*	*	1	--	--	--	*	*	*	1
Port Stanley	42	40	81	13	*	*	*	*	--	1	--	--	--	*	*	*	1
Saugeen	42	40	81	16	--	--	--	--	1	--	--	--	--	*	--	--	1
Squan Beach	40	8	74	1	*	*	--	--	--	--	--	--	--	*	*	1	1
Saint Louis	38	37	90	15	1	--	--	--	--	--	--	--	--	--	--	--	1

The number of stations at which a difference of 40° between the maximum and minimum of the same day was observed in 1874, is 38, which is 35 per cent of the whole number of stations; and among the stations at which so great a difference did *not* occur, are Kingston, Toronto and Montreal, in Canada, and Pike's Peak, at an elevation of 14,092 feet above the sea.

From the preceding tables it appears that throughout the greater part of the United States there is occasionally observed a difference of 40° between the maximum and minimum temperature of the same day, and there are a few places where such changes are remarkably frequent. This phenomenon occurs most frequently at stations situated between the Mississippi River and the Rocky Mountains, and at the head of the list stand Colorado Springs and Denver. Colorado Springs is situated on the eastern side of Pike's Peak, at an elevation of 5,935 feet above the sea, and Denver is distant from it about 60 miles on the eastern slope of the Rocky Mountains, at an elevation of 5,135 feet.

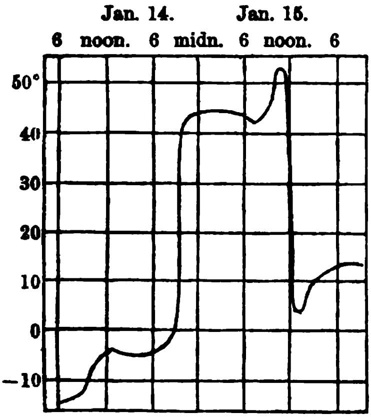
These cases of great diurnal change of temperature result from the ordinary diurnal change combined with the effect due to the passage of a great storm; and the sudden fall of temperature, which frequently succeeds a great storm, I think cannot be fully explained without admitting the sudden descent from a great height of air whose temperature is unusually low. The proximity to mountains appears to favor this sudden descent of air from a great height. Colorado Springs and Denver are situated where the upward and downward motion of atmospheric currents must be uncommonly frequent, and they surpass all the other stations of the signal service in the magnitude and suddenness of the changes of temperature. An instance of this kind has recently been reported which is the most remarkable I have ever known. I have received a statement of the facts from three different sources, all of which agree in the main, but they show differences of several degrees of temperature, which may be ascribed to a difference in the exposure of the instruments. The most moderate statement is that furnished by the observer at the U. S. signal service station, and I will therefore adopt his numbers in preference to either of the others.

Storm of Jan. 15, 1875, *at Denver, Colorado.*

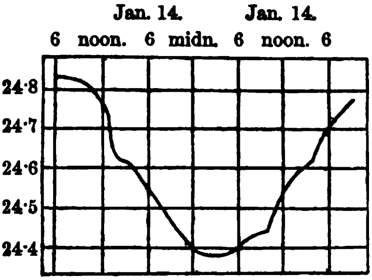
On the 14th of January, 1875, the thermometer at Denver had been below zero all day, with a variable northeast wind. At 9 P. M. of that day the thermometer was one degree above zero. The wind then veered suddenly to southwest; and at 9.15 P. M. the thermometer stood at 20°; at 9.20 P. M. it stood at 27°; at 9.30 P. M., 36°; and at 9.35 P. M. at 40°; after which there was but little change till near noon of the next day. The preceding observations show a rise of the thermometer amounting to 39 degrees in 35 minutes.

On the 15th of January, the thermometer had been above 40° all the morning, with a fresh southwest wind. About 11.30 A. M. the thermometer stood at 52°. The wind then suddenly backed to northeast, and at 12.30 P. M. the thermometer stood at 4°; being a change of 48° in one hour. Another observer, who is pronounced perfectly reliable, says that between 11 A. M. and noon a thermometer fell from 58° to 22° (that is, thirty-six degrees) in five minutes. The annexed figure on the left will perhaps give a better idea of the extent and suddenness of these changes of temperature than would be derived from a simple description. The entire curve shows the state of the thermometer for nearly two days, from 5.43 A. M., Jan. 14th, to 9 P. M., Jan. 15th, according to the observations made at the signal service station. The figure on the right shows the changes in the pressure of the atmosphere during the same period. On the 14th the barometer fell from 24·83 to 24·40 inches, and on the 15th it rose again to 24·76 inches.

Thermometer.



Barometer.



These changes of temperature and pressure which were noticed at Denver, were the effects of a considerable storm which came from the northwest, and whose center passed on the east side of Denver, within about 250 miles of that place. This storm was accompanied by high winds and gales in Colorado and Kansas. It probably did not differ materially from the winter storms frequently experienced in other portions of the United States, except in the extreme suddenness of the changes of wind and temperature. I do not think that these sudden changes can be fully explained by the supposition of a polar current sweeping along the earth's surface from a higher to a lower latitude, but it seems necessary to admit a sudden transfer of very cold air from a higher to a lower level. The heat of Jan. 14th probably resulted from the sudden precipitation of vapor caused by the elevation of air from the earth's surface; and this warm air near the earth's surface suddenly ascended on the 15th, being displaced by the colder air of a greater elevation.

In preparing the materials for this article, I have been assisted by Mr. Edward S. Cowles, a graduate of Yale College of the class of 1873.

ART. II.—*Preliminary Note on a Magnetic Proof Plane*; by HENRY A. ROWLAND.

ABOUT four years ago I made a large number of experiments on the distribution of magnetism on iron and steel bars by means of a coil of wire sliding along the bar; the induced current in the coil as measured by a galvanometer was a measure of the number of lines of force cut by the coil and can be found in absolute measure by my method of using the earth inductor. These researches have never yet been published owing to circumstances beyond my control, but are known to quite a number of persons in this country, and will soon be published. The method there used is the only correct one that I know of for experimenting on magnetic distribution, and my purpose in this note is to extend it to bodies of all shapes, so that experiments on magnetic distribution may become as simple and easy to perform as those on electrical distribution. And so well has my magnetic proof plane accomplished this that I can illustrate the subject to my classes with the greatest ease.

The apparatus required is merely a small coil of wire $\frac{1}{4}$ to $\frac{1}{2}$ inch in diameter, containing from 10 to 50 turns, and a Thomson galvanometer. When we require to reduce to absolute measure, another coil about a foot in diameter and containing 20 or 30 turns is required. Having attached the small coil (or, as I call it, the magnetic proof plane) to the galvanometer, we have merely to lay it on the required spot, and when everything is ready, to pull it away suddenly and carry it to a distance, and the momentary deflection of the galvanometer needle will be proportional to that component of the lines of force at that point which is perpendicular to the plane of the coil. And if we apply it to the surface of a permanent magnet the so-called surface density of the magnetism at that point will be nearly proportional to the deflection. In the case of an electro-magnet the surface density will be nearly proportional to the deflection minus the deflection which would be produced by the helix alone, though the last is generally small and may be neglected. I use the words *nearly* in the above statement because they are only exactly true in the cases where the lines of force proceed from the surface in a perpendicular direction; otherwise the deflections must be multiplied by the secant of the angle made by the lines of force with the surface of the magnet. In the case of an electro-magnet made of very soft iron, theory shows that the lines pass out nearly perpendicular to the surface and so no correction is needed.

We can also, by a coil of this kind, determine the intensity of the magnetic field at any point and thus be able to make a complete map of it. Having done this, we have all the data necessary to substitute in the formula which I have given in this Journal,* and by a simple experiment can thus determine the coefficient of magnetization of any diamagnetic or weak paramagnetic body probably in a more accurate manner than any Weber used. Only the largest-sized magnets could of course be used for this purpose with any accuracy, and indeed they are always to be preferred in obtaining the distribution by this method.

Having obtained the distribution for any given magnet, the distribution for any similar magnet of the same material but of different size becomes known by a well known law of Sir William Thomson.

As, in the present state of our knowledge, magnetic measurements are of small value unless made on the absolute scale, we require to reduce our results to this system. There are several methods of doing this, but the simplest is that which I have used in my experiments on magnetic permeability, and consists in including an earth inductor in the circuit. A coil laid on a perfectly level surface is sufficient for this: when this is turned over, the induced current will be equal to $C=\frac{2nVA}{R}$, where n is the number of turns in the coil, A its mean area, V the vertical component of the earth's magnetism, and R the resistance of the circuit. When the small coil is pulled suddenly away the current will be $C'=\frac{n'Qa}{R}$, and so we have $Q=2V\frac{AnC'}{an'C}$, in which when a Thomson galvanometer is used C′ and C can be replaced by the corresponding deflections; hence $Q=2V\frac{AnD'}{an'D}$, in which a and n' are the area and number of turns in the small coil and Q is that component of the magnetic field we are measuring in the direction of the axis of the small coil.

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* On a new diamagnetic attachment to the lantern, &c., this Journal, May, 1875.

As an illustration of this method I will give a few experiments made with the magnets of a Ruhmkorff diamagnetic apparatus, which was altogether about 2 ft. long and had its magnets 2 in. in diameter, with a hole ½ in. in diameter through them for experiments on the rotation of the plane of polarization of light, but which in these experiments were closed by the solid poles which were screwed on. The first experiments were with two discs of iron, 4·6 in. in diameter and 1⅜ in. thick, screwed on to the poles. In the first place the poles were turned away from one another, the current being sent through only one magnet, and the values of the magnetic field obtained at different points close to the surface of the disc. These may be numbered as follows: No. 1, at center of face of disc; No. 2, on face of disc half an inch from the edge; No. 3, on center of edge of disc. The measures are on the meter, gram, second system.

1st. Strength of current, 4·4 farads per second.

1. 2220. 2. 3550. 3. 4440.

2nd. Strength of current 8·3 farads per second.

1. 3600. 2. 5300. 3. 7500.

Next the poles were turned toward each other and the current sent through both magnets, so as to make the poles of the same name. Current 4·6 farads per second.

1st. Distance of poles, 3 in.

1. 1300. 3 3800.

2nd. Distance of poles, 1½ in.

1. 600. 3. 4000.

Here we see an approach to one of Faraday's places of no magnetic action.

After this the current in one of the magnets was reversed so as to make the poles opposite. Current the same.

1st. Distance of poles, 3 in.

1. 5800. 2. 8200. 3. 6700.

2nd. Distance of poles, 1½ in.

1. 9800. 2. 7500. 3. 5800.

It is curious to note how the distribution changes with the distance of the discs; thus, on one disc free from the other, the edge of the disc has the greatest magnetic surface density, but when the two discs form opposite poles and are 3 in. apart, position 2 gives the greatest effect, while, when they are 1½ in. apart, the field is greatest at the center. This entirely agrees with theory.

The conical poles for diamagnetic experiments were then screwed on. These were portions of cones with an angle at vertex of about 60°, with the vertex considerably rounded off. They were one inch apart and the poles were opposite. Current 4·4 farads per second.

At center of field between the poles,	12500
On the axis near one pole,	32100
On cone one inch from vertex,	11000
On cylindrical portion of magnet 2⅛ inches from the vertex of the cone,	5800

These poles were now replaced by frustums of cones with flat ends, the original diameter of the iron, 2 inches, being reduced at the end to 1⅛ inches, and they were placed ½ inch apart. The field in this case between them was 61000, or nearly up to the maximum of magnetization of nickel at common temperatures, and above that at high temperatures.

Troy, April 1, 1875.

ART. III.—*On Pseudomorphs of Chlorite after Garnet at the Spurr Mountain Iron Mine, Lake Superior;* by RAPHAEL PUMPELLY. With Plate II.

PSEUDOMORPHS of garnet occur in abundance in a bed of chloritic schist, just overlying the great magnetite bed of the Spurr-Michigamme iron range.*

This schist is of Archæan age and belongs in the upper beds of the Huronian iron series. It is a very fine-grained, dark green chlorite, which gives a light green streak and powder, dissolves in acids leaving a deposit of silica, and fuses B.B. on the edge to a black magnetic enamel (fus. = 4.) It is impregnated with octahedrons of magnetite, which rarely reach a diameter of one-eighth inch. Throughout the rock are scattered the pseudomorphs in very sharply defined rhombic-dodecahedrons of all sizes below 1⅛ inches in diameter. Often perfect crystals can be easily detached from the matrix.

On breaking the crystals and polishing the surface of fracture, they are found to be changed more or less to chlorite, in some instances specimens an inch in size containing not more than five per cent of garnet, while in others 30–50 per cent of unaltered mineral is present.

The octahedral crystals of magnetite are scattered through the pseudomorphs. They are visible to the naked eye, half imbedded on the surface planes, and in the interior of the crystals, both in the chlorite and in the unaltered garnet.

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* I am indebted to Dr. Cobb, the agent of the mine, and to Col. F. Norvell, general manager, for several hundred fine specimens of these pseudomorphs.

I have made several thin sections passing through the middle of crystals, about one inch in diameter, and have studied them under the microscope, making such examination of the optical characteristics as the nature of the minerals and the limitations of the method would permit.

Under a one-tenth inch objective (500 diameters) the garnet appears not to be strictly homogeneous in texture; it has a curdled structure, particles of a transparent bluish-white filling the irregular meshes of a less clear, white net-work. Both of these portions of the garnet are isotrope, remaining dark through a full revolution between crossed nicols. Throughout both of these members are scattered exceedingly minute particles of a transparent red substance (hematite?) and larger opaque grains and discoidal plates.

A glance at a section under a low power shows that the change has taken place by an attack on the garnet along the countless fissures that traverse it in every direction (fig. 1), progressing most rapidly in the larger cracks, and ramifying through the more minute ones.

Two substances, one greenish-yellow, the other clear green, seem at first sight to be among the products now forming the pseudomorphs, though, as we shall see, they both probably belong to the same mineral.

I. The slightly greenish-yellow mineral (fig. 1) surrounds the remaining garnet fragments in bands which are in places clear and transparent, and in others are marked with longitudinal wavy lines, which probably indicate the cleavage of the mineral. From these broader bands, narrow ones branch off to form an intricate net-work in the garnet fragments. The same mineral occurs in isolated and grouped, long and slender crystals, which often branch out from or intersect the bands; while in other places the bands are often made up of these crystals, arranged more or less parallel to each other.

These bands are generally $\frac{1}{1000}$ to $\frac{1}{100}$ of an inch wide, and under a low power (figs. 1 and 2) their edges are sharply defined. Where a garnet fragment has been entirely destroyed, its place is occupied by an interwoven mass of them, often associated with irregular patches of the green substance described below. Under a high power, both the transparent red particles and the opaque grains and plates that were noticed in the garnet, are observed in this alteration-product.

These bands, when observed with only one nicol—the polarizer—show a high degree of absorption for intensity, and an appreciable amount for color, changing from very dark (with bluish-green tint), when the longer direction is parallel to the undulation plane of the nicol, to very light (with greenish-yellow tint) when perpendicular to that plane. Assuming that the parallel sides of the bands are crystallographic outlines and that they lie either in the basal plane, or else parallel to the principal crystallographic axis, I have attempted to determine optically the system to which these crystals belong. The method followed is that recommended by Tschermak in distinguishing pyroxene, hypersthene and biotite. Having carefully adjusted the microscope,* so that the cross hairs in the ocular coincided exactly with the undulation planes of the crossed nicols, I first selected an individual, generally a long one with straight sides, and brought it by means of one of the cross-hairs into parallelism with the undulation plane of one of the nicols, and then revolved the stage till the nearest point of maximum darkness was reached, when the principal sections of the crystal coincided each with a principal section of a nicol's prism. The number of degrees of this revolution indicate the inclination of the principal sections of the crystal to the crystallographic feature chosen for reference.

Of course, if the mineral were either uniaxial or orthorhombic, the maximum of darkness would occur when two of the axes of the crystal were parallel to the undulation planes of the nicols, and there would be no revolution required. But this could occur in a monoclinic crystal only when one of the principal sections happened to coincide with the plane of symmetry; in every other position the axes of the crystal would make with its principal sections an angle which would vary between 0° and the number of degrees representing the inclination of the bisectrices to the vertical and inclined lateral axes; the full amount of this inclination could only be observed when the principal sections of the crystal were perpendicular to the plane of symmetry, but any inclination suffices to determine that the crystal belongs to a clinobasic system. Observations on a great number of the bands and isolated crystals failed to show any inclination; the mineral, therefore, does not belong to a clinobasic system.

II. The clear green portions occur isolated in the garnet fragments, and in places in the fissures with the mineral last described, and more or less diffused through the larger bands, but more generally in irregularly-shaped spots, and with outlines which are not necessarily determined by those of the garnet fragments. These larger areas exhibit lamellar aggregate polarization both between crossed nicols and with the polarizer alone. Between crossed nicols portions remain wholly dark during a revolution, some show only a faint change, and others are not distinguishable from the substance forming the bands, except that the cleavage lines are not so distinct. So also with one nicol, the portions that remain dark between crossed nicols show no absorption, while other portions change from clear green to almost colorless faint green-yellow, and still others show about the same changes as the bands.

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* One of Beck's first class binoculars, in which the polarizer is attached to a sub-stage, and the main stage is graduated.

Again, we find in places, on the same individual, all these conditions, shading gradually one into the other in a manner that seems to indicate a bent crystal. I am inclined to look upon the bands and the clear green as identical, and as belonging to a hexagonal chlorite. The green portions would then be those which were cut more or less parallel to the basal plane, and the dichroitic bands those cut perpendicular to this.

While the plane of contact between the chlorite bands and garnet appears sharp under a low power, higher objectives (one-tenth or one-sixteenth inch) show it to have a rough surface caused by the projection of countless chlorite points into the garnet substance, in a manner that leaves on the observer the impression that the attack is facilitated in some way by the curdled structure of the garnet.

The chloritic schist which encloses the pseudomorphs consists apparently of exactly the same chlorite, the only perceptible difference being that in the schist the individuals are very minute, averaging ·00015 inch thick by ·0005 long, with scattering aggregations of crystals ·0008 by ·004 inch.

The optical characteristics of these larger, and so far as determinable, of the smaller, are identical with those of the chlorite in the pseudomorph.

The only other substances observed in the schist are minute octahedrons of magnetite and the discoidal plates which occur indifferently throughout the schist, the garnet substance, and the pseudomorphous chlorite. These plates average about ·00025 inch thick by ·00075 in diameter, and in reflected light have metallic luster. They are not attracted by the magnet and show no change after continued boiling in sulphuric and in muriatic acids. I had at one time the impression that they were graphite, but on comparing them with microscopic plates of that mineral in the Port Henry limestone, the difference in luster and fracture appeared very great.

These minute plates appear to me to be older than either the garnet or the chlorite—to have been enclosed in the original rock (argillaceous limestone ?), and to have resisted the changes which successively produced the garnets and destroyed these and substituted chlorite for them and the original rock.

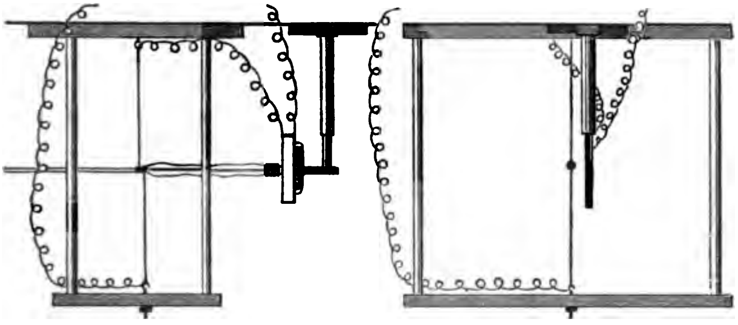
If this view is correct, the paragenesis should be as follows:

I. ORIGINAL ROCK (*Marl?*)

II. **Metamorphic Change** with crystallization of
   (*a.*) Octahedrons of *magnetite* and the *discoidal crystals.*
   (*b.*) Garnets.
III. **Pseudomorphic Change.** *Chlorite* after the original rock and after garnet, but preserving the magnetite and discoidal crystals intact.

Art. IV.—*Brief Contributions from the Physical Laboratory or Harvard College.* No. 18.—*An Application of the Horizontal Pendulum;* by Harcourt Amory.

The paper on the horizontal pendulum published by Zöllner, in Poggendorf's Annalen, suggested the following application of the instrument to proving Ampère's laws of the attraction and repulsion of currents. To prove these laws, the circuit which the current traverses must consist of two parts, one fixed and the other movable. The apparatus devised by Ampère is difficult to make. By the use of the horizontal pendulum, however, the mutual action of the currents can be readily shown. The apparatus is arranged as in the accompanying figure. The wire which forms the upper support of the pendulum is connected with one pole of a battery, and is then led along the horizontal bar of the pendulum, best made of glass, and is bent in the form of a parallelogram at the extremity of the bar. The wire is then led back to form the lower support of the pendulum, and is then connected with the other pole of the battery through a fixed coil of wire placed in the neighborhood of the end of the pendulum.



The current first passes to the upper suspending wire, around the parallelogram at the extremity of the pendulum, back through the lower supporting wire, through the outside coil, and returns to the battery. By turning the outside coil upon a horizontal axis, the laws of attraction or repulsion of rectilinear currents can be shown. The apparatus is peculiarly well adapted to show the action of solenoids upon each other.

Art. V.—*Explosive Properties of Methyl Nitrate;* by M. Carey Lea, Philadelphia.

Several dangerous accidents have recently occurred in the manufacture of methyl nitrate, one of them unfortunately resulting in the death of Mr. Chapman. This, and the remarks recently published on the subject by M. Girard, the well-known French chemist (an abstract of which lately appeared in the pages of this Journal), leads me to make a few observations on this subject.

When I first attempted to prepare this substance by the only method published up to that time, I felt convinced that the chances were greatly in favor of an accident, though no warning was given in the text books. I therefore wore a mask, and operated cautiously with moderate quantities in a very large flask. A tremendous explosion followed, in which the flask entirely disappeared; no fragment of the body could be found. I then tried the use of urea in the same modified manner which I had proposed in the case of ethyl nitrate. The operation was entirely successful, and was many times repeated without any trouble or difficulty. And it is, I presume, in this way that it is now commercially manufactured on a large scale.

Within the last few days I have made the following experiments on its explosive properties.

Contrary to what has been stated, I do not find it liable to explode by percussion. Some extra thick filtering paper was saturated with it, was placed on a piece of iron, and forcibly struck with a hammer. This was repeated a dozen times, until the paper was broken to pieces, without explosion.

Five or six drops were placed in a test tube; this was placed in a deep cup, and a little alcohol poured into the cup and inflamed. In this way the flames played chiefly on the surface of the test tube above the liquid, thus preventing its escape by evaporation at low temperatures. A slight explosion followed, which did not break the test tube.

When ethyl nitrate was similarly treated, it quickly evaporated without explosion.

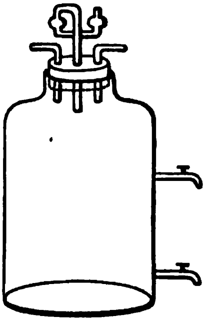
Twenty measured minims of methyl nitrate were then placed in the tube, and the experiment repeated. A moderate explosion followed, breaking the tube.

The same quantity as in the first experiment, five or six drops, was placed in the test tube, and dry sand added more than enough to absorb the liquid. The heat was applied in the same way as before, but the explosion was not greater than without the sand, except that the tube was broken.

When poured on filtering paper and inflamed, it burns quietly with a peculiar livid flame.

These trials do not seem to indicate a very violent explosive power. Nevertheless, the unfortunate experience which has been already gained sufficiently indicates that it is not a substance to trifle with. Indeed, a liquid whose vapor explodes at 150° C., as determined by Dumas and Peligot, cannot be otherwise than dangerous, especially when handled in large quantity. Having been the first person to prepare this substance in any quantity, and having had occasion to study carefully its conversion into the methyl ammonias, it occurs to me that as these substituted methyl compounds are now used on a large scale, a few suggestions toward a safe method of managing the operation for large quantities may be acceptable.

I would propose to construct a receptacle in which to receive the methyl nitrate vapors as they are distilled over, of the shape shown in the margin, a cylindrical vessel with a large mouth and two faucets, one near the bottom, the other a little above the middle.



To commence the operation, the vessel should be filled one-third full with lumps of ice. The cover should then be closely fitted on with a safety tube, and a third tube connecting with another similar vessel.

As the partly condensed mixed alcoholic and ethereal vapors come over, the products of distillation are cooled by the ice, and the water resulting from its melting precipitates the ether. This last can be from time to time drawn off by the lower faucet, into stoneware bottles already containing a proper quantity of strong ammonia and alcohol. When one-third full of the ingredients in the proper proportions, these bottles should be securely closed, and placed for several days at a temperature of about 90° F., until decomposition is complete.

The upper faucet shown in the cut is to draw off the mixed water and alcoholic distillate which accumulates in large quantities over the ether, and which otherwise would impede the operation.

In the process as above described, the methyl nitrate would be destroyed almost as soon as formed. As soon as any considerable quantity has passed over, it could be transferred into the vessels in which, by contact with ammonia, decomposition begins, and this without interrupting the distillation. If desired, sodium carbonate can be placed in the condensing vessel to remove any nitric acid that may adhere to the methyl nitrate. But the presence of a little acid is not important in the decomposing jar; it would be converted into ammonium nitrate, and as this salt is always formed to some extent by the action of methyl nitrate on ammonia, the presence of a fraction more or less would be unimportant.

I am inclined to believe that in the manner described, the danger in the preparation of this substance will be reduced to a minimum, a matter of some importance, as the quantity consumed in the manufacture of the methyl violet seems likely to be increasingly large. A good cooling apparatus between the retort or still and the receiver here figured would be necessary.

W. Weith has just published* an interesting communication on the formation of methyl-ammonias by the action of excess of methyl alcohol on sal ammoniac, which at 285° C. was completely converted into methyl ammonias. This method of methylizing ammonia may perhaps take the place of mine: it is, however, liable to two objections; first, that a considerable proportion of the methylic alcohol is, according to Herr Weith, lost by conversion into methyl ether; secondly, the high temperature and pressure necessary. I found that methyl nitrate, unlike ethyl nitrate, does not require pressure vessels, but reacts at or near the ordinary temperature and pressure. My own operations were performed in large stoppered vials set on the cooler part of a sand bath, where the temperature did not rise above 90°. The loss by formation of methyl ether may perhaps be compensated by the fact that in the formation of methyl nitrate there is always loss of both methylic alcohol and nitric acid.

Philadelphia, May 17, 1875.

ART. VI.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXXIV.—*On Zonochlorite and Chlorastrolite;* by GEORGE W. HAWES.

AT the meeting of the American Association for the Advancement of Science which was held at Dubuque, Iowa, in 1872, Prof. A. E. Foote described "a new hydrous silicate" found at Neepigon Bay, on the north shore of Lake Superior.† It is a hard, green mineral occurring in the amygdaloidal trap, associated with calcite, quartz, and various zeolites; and it is also found in the form of smooth water-worn pebbles upon the shore. This mineral, on account of its structure, he called *Zonochlorite*, since many of the specimens are beautifully banded; and his analysis convinced him that it was a zeolite. The stones which he regarded as pure were of a uniform dark green color; while those which were very plainly banded with white he considered impure. But thin sections of some of the dark green stones, received from Prof. Foote, and considered by him as the purest zonochlorite, when examined under the microscope, show that these, like the other specimens, are more or less banded and consist of green earthy particles disseminated through a white mineral. A dark green specimen gave me, on analysis, the following composition:

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* Bericht Deut. Chem. Ges., 26th Ap., 1875.

† Proceedings of the American Association for the Advancement of Science, 21st meeting, August, 1872, p. 65.

Silica	35·94
Alumina	19·41
Ferric oxide	6·80
Ferrous oxide	4·54
Lime	22·77
Magnesia	2·48
Soda	*trace*
Water	8·40
	100·34

The analysis indicates that the mineral is a very impure variety of prehnite, a mineral which is common in the trap of that region; and its hardness and behavior before the blowpipe point to the same conclusion. The analytical results obtained by Prof. Foote show that the material he examined was not homogeneous, as he states that the percentage of water, the average of which was 8·7, varied from 7·03 to 12·9. The presence of magnesia shows that a portion of the impurity is chlorite.

Zonochlorite in its mode of occurrence resembles *Chlorastrolite*,* which is found on the shores of Isle Royale in rounded pebbles derived from the amygdaloidal trap, and has also been found *in place* in the trap. At the suggestion of Prof. Brush, who placed at my disposal the specimens which were brought by him from Lake Superior, thin sections were made from some of the best stones in his possession; and the microscopic examination of these made it very evident that chlorastrolite is not a homogeneous substance—the impurities in this case being distributed through a white mineral of a radiated structure; and to the irregular arrangement of the pure and impure material the stone owes its beauty. When a flat surface is cut upon one of these stones, the polished face presents various shades of green; but when the other side is cut away, thus making a thin section of the stone, those spots which have the deepest green are found to be perfectly clear and white, receiving their deep green shade from the colored surfaces beneath. The green impurities are arranged along lines radiating from these clear centers, till at some distance the mixture becomes so intimate as to appear nearly homogeneous until more highly magnified. An analysis of a very fine stone gave the following result:

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* Boston Journal of Natural History, vol. v, p. 488. Report on the Geology of the Lake Superior Land District, Part II, p. 97. Dana's Mineralogy, 5th edition, p. 412.

	I.	II.
Silica	37·41	
Alumina	24·62	
Ferric oxide	2·21	
Ferrous oxide	1·81	
Lime	22·20	
Magnesia	3·46	
Soda	·34	·30
Water	7·72	
	99·77	

The essential difference between this analysis and those by Prof. J. D. Whitney is, that the percentages of magnesia and soda have exchanged places, since he obtained no magnesia and 4 per cent. of soda. For this reason, I repeated my alkali determination upon another sample prepared from portions of several stones, but with no variation in the result, as will be seen in No. II above. This great difference can be accounted for in no way save by the evidence, which the microscopic examination affords, that the stones are mixtures of minerals which have been carried into the amygdaloidal cavities of the trap. The stones that I have seen appear to consist, like the zonochlorite, largely of impure prehnite. The higher specific gravity may be due to an enclosure of epidote, which is everywhere associated with the chlorastrolite in the rocks, and moreover is often present in the same cavity.

ART. VII.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXXV.—*On Glycogen and Glycocoll in the Muscular Tissue of Pecten irradians*; by R. H. CHITTENDEN, Assistant in Physiological Chemistry.

THE genus Pecten is world-wide in its distribution. The species *irradians* is entirely American, being found most abundantly on the eastern shores of the United States. It is closely allied to the European and English species *opercularis* and *maximus*. The large central muscle which closes the valves of this mollusk is highly valued as an article of food, although its peculiar sweet taste is objectionable to some.

With this central muscle the following experiments were made:

Glycogen.—By extracting the edible portion of the scollop with cold water, a milky opaque fluid with slight acid reaction is obtained, and an insoluble residue consisting principally of syntonin or fibrin mixed with inorganic matters. The strong opacity of the aqueous solution is not due to an emulsion of fatty matters. On boiling the solution with or without the addition of acetic acid, a large amount of albumin is precipitated, leaving the fluid still opalescent. On treating the fluid, after the removal of albumin, with a small amount of ninety-five per cent alcohol, a light flocculent precipitate is obtained which dissolves by agitation, leaving the fluid unaltered in appearance; but if three or four volumes of the alcohol are added, a copious, permanent precipitate settles, leaving the supernatant fluid perfectly clear. This precipitate is of snowy whiteness, except when previous to precipitation the fluid has been boiled considerably, in which case both filtrate and precipitate assume a yellow or brownish color, from which the latter can be freed by solution in cold water and reprecipitation by alcohol. The precipitate, if allowed to dry in contact with air, after having been washed with alcohol merely, soon becomes translucent on the edges and finally is transformed completely into a gummy mass, which is sticky when moistened; but if after precipitation it is washed with ether thoroughly, it loses this property of becoming gummy, which seems to be due to the presence of water and of albuminous matters in small quantity. This gum-like mass when hard is brittle and yields on trituration a white hygroscopic powder showing under the microscope no distinct structure. A portion of the precipitate so prepared, dried in the air, yielded by analysis:

	1.	2.	Calculated. $C_6H_{10}O_5 + H^2O$ or $C_6H_{12}O_6$
C	39·52	39·56	40·00
H	6·62	6·55	6·60
O	53·86	53·89	53·34

In this state it is not quite pure, giving with Millon's reagent a strong reaction for albumin and containing some inorganic matter, one specimen 1·57 per cent, another 1·88 per cent, consisting in all cases, so far as were examined, of calcium phosphate. From this analysis it is seen that the substance has the formula of the sugars, or that of the starch group plus a molecule of water. The substance is tasteless, gummy when moistened and gives an opaque fluid with water, seemingly a true solution, which passes unchanged through filter paper and animal charcoal, and shows no particles under the microscope with a half inch objective. When this aqueous solution is boiled, thin films separate, forming a scum on the top of the fluid, which goes into solution again as the liquid becomes cool.

The substance is insoluble in alcohol and ether, has no reducing action with cupric sulphate and sodium hydroxide, but when boiled with a few drops of dilute hydrochloric acid gives a clear fluid which has strong reducing action. This same reaction takes place also with nitric and sulphuric acids, but not so readily as with the former. A portion of the substance was treated with a small quantity of saliva at the ordinary temperature, and at 40° C., and in both cases the ptyalin acted immediately upon it and sugar was formed. Treated with a solution of iodine in potassium iodide, a brownish red or maroon color was obtained. These and other reactions pointed to glycogen. It was yet to be ascertained whether the sugar formed by the action of acids and ferments was glucose, also to examine the action of boiling dilute nitric acid upon it, and to determine whether the different formulæ of glycogen could be obtained by drying it at different temperatures. A portion of the substance was then boiled with hydrochloric acid until alcohol produced no precipitate in a sample tested, the excess of acid removed by oxide of silver and the sugar obtained by evaporation. The product had all the properties of glucose, was intensely sweet, reduced alkaline solutions of copper and silver and yielded Pettenkofer's reaction. Analyzed, it gave the following result:

	1.	2.	Calculated. $C_6H_{12}O_6+H_2O$
C	36·15	36·17	36·36
H	6·82	6·88	7·07
O	57·03	56·59	56·57

By the action of boiling dilute nitric acid, oxalic acid was formed and separated. A different sample of the original substance, dried over sulphuric acid until a constant weight was obtained, yielded:

	1.	2.
C	43·81	43·90
H	6·43	6·46
O	49·76	49·64

A sample dried at 100° C. gave by analysis:

	1.	2.	Analysis of starch dried at 100° C. by Mulder
C	43·86	43·89	43·86
H	6·41	6·38	6·28
O	49·73	49·73	

A sample dried at 140° C.:

	1.	2.	Analysis of starch dried at 140° by Mulder
C	44·32	44·40	44·47
H	6·38	6·41	6·28
O	49·30	49·19	

On treating an aqueous solution of the substance at the ordinary temperature with an excess of a saturated solution of barium hydroxide, a heavy white precipitate was obtained, soluble in water, insoluble in baryta water and alcohol. This precipitate was dissolved in water, the baryta removed by a little dilute sulphuric acid in the cold and then reprecipitated by an excess of alcohol.

Prepared thus, it seemed to have lost the property of becoming gummy so readily as before, and on examination was found to be completely free from albuminous matters, giving no reaction even with Millon's reagent and also contained only 0·61 per cent of ash. The substance dried at 100° C. gave by analysis the following result, agreeing closely with that of the preceding preparation dried at the same temperature:

	1.	2.
C	43·93	43·91
H	6·45	6·40
O	49·62	49·69

Another sample, prepared in the same way and dried between 110°–120° C., gave:

	1.	2.
C	43·56	43·63
H	6·71	6·71
O	49·73	49·66

Casting a backward glance, we see that the analysis of the air-dried substance corresponds with the formula $C_6H_{12}O_6$, that of the substance dried at 140° C. with $C_6H_{10}O_5$, which requires 44·44 C. 6·11 H. These results agree with glycogen, which in different states of hydration has been found to have the formulæ $C_6H_{10}O_5$, $C_6H_{12}O_6$ and $C_6H_{14}O_7$. But the results obtained by the analyses of the substance dried at 100° C. and 110°–120° C. do not agree closely with any of the above formulæ. The same is true of members of the starch group to which glycogen is closely related, and lately Dr. Nägeli * has published a paper in which he points out that the elementary composition of starch, dextrin and "amylo-dextrin" dried at temperatures not exceeding 116°, agrees better with the formula $C_{36}H_{62}O_{31}$, which requires 43·63 C. 6·3 H, than with $C_6H_{10}O_5$, and that after exposure to a temperature of 140 C., when the composition corresponds to $C_6H_{10}O_5$, we probably do not deal with undecomposed starch. The analysis of amylo-dextrin by Nägeli (loc. cit. p. 35), dried in a stream of hydrogen, at a temperature of 112° to 116° C, gave the following result:

	1.	2
C	43·58	43·85
H	6·86	6·58

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* Beiträge zur näheren Kenntniss der Stärke Gruppe. Dr. Walter Nägeli. 1874.

A sample of dextrin dried at the same temperature gave Nägeli as a mean of two analyses, C. 43·52. H. 6·78, in both cases agreeing closely with my analyses of glycogen dried at 110°–120° C. Thus this substance, which is without doubt glycogen, coincides in this respect with its neighbors, dextrin, amylo-dextrin, etc.

A sample of glycogen, prepared by the preceding methods and dissolved in water, on treatment with basic lead acetate, with the application of a gentle heat, yielded a heavy gelatinous precipitate, which, when filtered off by the aid of a pump and washed with water, was found to contain lead. Dried at 100° C, it gave by analysis the following result:

	1.	2.
C	21·62	21·78
H	2·91	2·96
Pb	48·39	48·34
O	27·08	26·92

Since this result was obtained, I find that M. Bizio * has already discovered glycogen in some invertebrates. Among the Mollusks, he found it in considerable quantity in the oyster. With glycogen from these sources he prepared a lead compound by means of tribasic acetate of lead, and says its "analysis has given me the formula $Є_{12}H_{18}PbΘ_{10}$," which requires:

C	27·22
H	3·40
Pb	39·13
O	30·24

It will be seen at once that my result does not agree with this formula. I therefore made some further lead precipitates from the same and other preparations of glycogen, and in these simply determined the lead as follows:

Pb.	1st Prep.	2d Prep.	3d Prep.	4th Prep.
No. 1.	48·39	53·63	51·45	50·27
No. 2.	48·34	53·58	51·45	50·28

Some glycogen was also prepared from the liver of an ox by the usual method and dried at 100°. It yielded by analysis:

	1.	2.	Calculated. $C_{12}H_{22}O_{11}$
C	41·87	41·90	42·11
H	6·35	6·38	6·43
O	51·78	51·72	51·46

A lead preparation made from this gave:

	1.	2.
Pb	61·99	61·94

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* Comptes Rendus, lxx, 175. Zeitschrift für Chemie, 1867, 745.

These results indicate that the composition of the precipitate is not constant.

The amount of glycogen occurring in this muscular portion of the scollop is quite large; at one time, from three quarts 160 grams were obtained; at another, two quarts yielded 70 grams.

Glycocoll.—On evaporating the alcoholic filtrate from the precipitated glycogen until quite concentrated and adding neutral lead acetate, a heavy white precipitate is produced, which is a combination of inorganic matters with the lead. The excess of lead is then removed from the filtrate by hydrogen sulphide, and after concentration the liquid is decolorized by animal charcoal. On further evaporation the fluid deposits white prismatic crystals. The crystals have a sweet taste, but upon ignition with soda lime, ammonia is evolved, evincing the presence of nitrogen, which at once separates it from the saccharine group. The crystals first obtained were not quite pure, but after treatment with animal charcoal and recrystallization gave by analysis a result corresponding to the composition of glycocoll. Two more distinct preparations were made and gave by analysis:

	1st Prep.		2d Prep.	3d Prep.	Calculated.
	1.	2.			$C_2H_3O_2NH_2$.
C	31·98	31·99	32·09	31·97	32·00
H	6·88	6·84	6·79	6·81	6·66
N	18·57	18·58	18·49	18·45	18·66
O	42·57	42·59	42·63	42·67	42·66

The impurities which seemed to be the most difficult to remove were coloring and inorganic matters. The crystals were soluble in water and weak alcohol, insoluble in ether and absolute alcohol. An aqueous solution, when treated with sulphate of copper and sodium hydroxide, assumed an azure blue color without separation of cuprous oxide on heating. The substance melted at about 180° C, then decomposed. With nitric acid fine crystals corresponding to nitrate of glycocoll were obtained. These analyses and reactions identify the substance as glycocoll, which I believe has never before been found in nature.*

A preparation was now made in which the alcoholic filtrate from the precipitated glycogen was evaporated without the addition of any reagents, and here the same crystals were obtained mixed with a considerable quantity of inorganic matters and some dextrose.

The amount of glycocoll occurring in the tissue is small, although where two or three quarts of material are used a fine crop of crystals may be obtained.

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* Lehrbuch der Physiologischen Chemie, Gorup-Besanez, page 236. 1875.

The quantitative analysis of the edible or muscular portion of the scollop, as obtained at the market, is as follows:

1ST ANALYSIS.

	1.	2.
Water,	79·60	79·66
Solids,	20·40	20·34
Ash,	1·26	1·26
Nitrogenous matters (=N×6·4),	15·68	15·68
Ether extract,	·38	·28
Non-nitrog. by difference,	3·13	3·12

2D ANALYSIS.

	1.	2.
Water,	80·25	80·25
Solids,	19·75	19·75
Ash,	1·24	1·22
Nitrogenous matters,	15·04	15·04
Fatty "	·32	·24
Non-nitrog. "	3·15	3·25

Total amount of nitrogen in the substance dried at 100° C.:

	1.	2.
N	11·35	11·37

The percentage of glycogen was determined in two separate quantities.

	1.		2.	
	1.	2.	1.	2.
Glycogen,	2·43	2·40	1·98	2·19

The percentage of glycocoll was determined, but owing to the inaccuracy of the method, can be considered only as an approximation to the truth.

	1.	2.	3.	4.
Glycocoll,	·46	·68	·71	·39

The ash of the muscle consisted of the bases, soda, potash, magnesia and lime; acids, chlorine, sulphuric and phosphoric.

In conclusion, I wish to express my obligations to Prof. S. W. Johnson for advice freely given.

ART. VIII.—*Dr. Koch and the Missouri Mastodon;* by EDMUND ANDREWS, M.D., Professor of Surgery in the Chicago Medical College.

THE recent article of Professor Dana on the credibility of Dr. Koch's statement respecting the occurrence of human remains with those of the Mastodon in Missouri is a timely contribution, for many scientific men still quote Koch's testimony, as though it had never been impeached. The following facts were laid before the Chicago Academy of Sciences by Professor P. Hoy of Racine, Wisconsin, a well known scientist, and one whose honor and truth are above every shade of suspicion.

Many years ago, Professor Hoy visited the spot from which Dr. Koch had exhumed the skeleton now in the British Museum. He found the men who assisted him at the work and took their account of it. He himself also excavated and recovered some fragments of the skeleton missed by Dr. Koch.

It will be remembered (see Professor Dana's article on Dr. Koch's pamphlets) that the discoverer of this skeleton claims to have found it overlaid by one stratum of alluvium, one of marl, three of clay, and three of conglomerate, amounting to some fourteen feet of deposits above the bones. Professor Hoy states that this whole list of strata is a pure fiction. The skeleton was found close to the surface, with nothing but the surface muck over it. The men who assisted at the exhumation also informed Dr. Hoy that Dr. Koch did not drain the spot, as might have been done without great difficulty, but that he and they simply dug out the muck and earth, often working up to their waists in the water, and groping with their hands at the bottom to find the bones. In these circumstances it is obvious, aside from the question of veracity, that no accurate determination could have been made between flint weapons of later date and those which might be contemporaneous with the animal.

There seems no *à priori* improbability in the idea that the mastodon may have survived until after the advent of man upon this continent, but it is evident that Dr. Koch's testimony contributes nothing to the solution of that question.

The following fact may throw light on the burnt skeleton in Gasconade County, without impeaching Dr. Koch's testimony as to the scorched condition of those bones. The Western States are much more subject to long droughts than the Atlantic States or European countries. Here it is a common thing, in very dry seasons, for a peat-bed to be accidentally set on fire and completely burned up. In these cases all the bones in it are scorched, trees growing on it are undermined, and fall over, and any stone implements, or other incombustible objects lying on the surface sink down among the more ancient relics below. Afterward, the peat grows again, covering them all up; or sometimes the freshets of neighboring streams fill the hollow with clay and other river detritus. The skeleton in Gasconade County may have been scorched in this way, and stones and arrowheads of a later age mingled with the bones. In examining the relics found in western swamps, it is always necessary to guard against the possibility of error introduced by the action of fires of this character.

No. 6, 16th street, Chicago.

ART. IX.—*Rate of Growth of Corals.* From a letter to Professor J. D. DANA, by Professor JOSEPH LECONTE, dated University of California, May 1, 1875.

I OBSERVE in your work on Corals and Coral Reefs, while discussing the rate of coral growth, you mention an interesting observation of Weinland on the corals about Hayti, bearing on this subject. This recalls to my mind a very similar observation on a much larger scale, made by myself during the winter of 1851, while assisting Professor Agassiz in his examination of the Florida reefs. Knowing your interest in the subject, I send you an account of it.

Professor Agassiz and his party were at Fort Jefferson, Tortugas. Dr. Wm. L. Jones and myself had gone to examine a little island about 8 or 10 miles to the northwest. On returning to Fort Jefferson in a small boat, when about half way between the two islands and in the still shoal water on the inside of the line of reefs, to our great surprise the boat suddenly grounded on the close-set prongs of an extensive grove of madrepores (*Madrepora cervicornis?*). On examining closely the trees of this grove, we found: 1. That the prongs were far more thickly set than is usual in this species; 2. that all the prongs not only of the same tree, but of all the trees of the whole grove, grow up to nearly the same level, which at the time examined was very near the surface; 3. that all the prongs at that level were dead for a distance of one to three inches from the point. The lower limit of death seemed to be a *perfectly horizontal plane.* The dead points rose above it to various distances not exceeding three inches. We rowed around the margin of this grove for a considerable distance and found everywhere the same phenomena. I satisfied myself that the whole grove, for hundreds of acres in extent, had been clipped in a similar manner.

On subsequent inquiry at Key West, I learned that the mean level of the ocean, owing probably to the prevalence of certain winds, was higher during one portion of the year than during the other. It became evident, therefore, that during the high water the living points of the madrepores grow upward until the descending water level exposes and kills them down to a certain level. With the rise of the mean level again, new points start upward, to be again clipped at the same level by the descending water. The levelness, the thick setting, and the deadness of the points are all thus completely accounted for. It is precisely the phenomena of a clipped hedge.

I have long been accustomed to estimate from these facts the rate of madrepore growth. I have recently looked up the data necessary to do so more accurately.

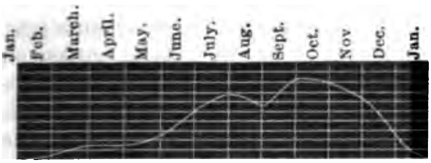
The following table, taken from the Coast Survey Report for 1853, p. 76, gives the mean sea level for the different months of the year.

	Feet.	Dif.		Feet.	Dif.
Jan.	5·10	0	**July**	·78	·68
Feb.	·15	5	**Aug.**	·63	·53
March	·26	·16	**Sept.**	·93	·83
April	·26	·16	**Oct.**	·90	·80
May	·32	·22	**Nov.**	·73	·63
June	·60	·50	**Dec.**	·31	·21

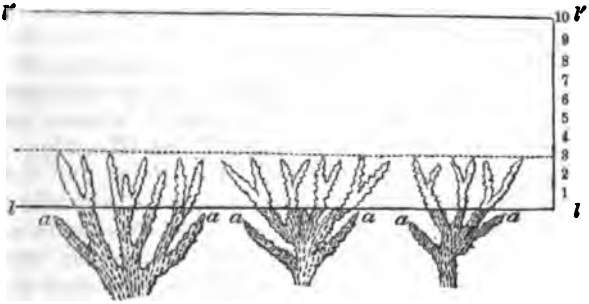
From the differences I have roughly plotted a curve representing the annual variation of mean level.

It is seen that the mean level at Key West is lowest in January and highest in September, the difference being 0·83 feet or about ten inches. Now starting with the lowest mean level in January, *l l*, it is evident that living points *a a a* near that surface would grow upward and continue to grow all the time the water was rising from *l l* to *l' l'*, i. e., from middle of January to the middle of September, and also while it was falling to three inches above *l l*, i. e., until about the beginning of December. At this level the growing points would be nipped. It is evident, therefore, that the three inches were grown in 10 to 10½ months, which would make about 3½ inches per annum.

1.



2.



This table is taken from Key West; my observations were at the Tortugas. There may be differences in the amount and the curve of variation in different places. But this would make but little difference in the result. I believe we may say with confidence that the annual growth of madrepore points in the Gulf is not more than 3½–4 inches per annum.

ART. X.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXXIII.—*Results of Dredging Expeditions off the New England Coast in* 1874; by A. E. VERRILL.

IN the last number of this Journal a general statement was made of the operations in connection with the U. S. Fish Commission, located during the summer of 1874 at Noank, Connecticut, on Fisher's Island Sound, and close to the eastern end of Long Island Sound.

In the following article only a brief summary of the results can be given; the full details will eventually appear, however, in the report of the Commissioner.

The total number of recorded stations, where dredgings were made during the summer, is 180, but many others, not registered, were made by members of the party. A large number of additional localities along the shores and in the shallow waters of the harbors were explored by hand nets and otherwise with excellent results. Temperatures were not taken at all the dredging localities, and therefore, in the accompanying table, such localities have been, for the most part, omitted.

The localities dredged may be conveniently grouped as follows:

a. Fisher's Island Sound, mostly hard gravelly and stony bottoms, often rocky, and occasionally with some sand or mud, the depth varying from 4 to 15 fathoms. The tidal curren were rather strong and the bottom temperatures were lo (usually 61° to 65° F.).

b. Block Island Sound, including a wide region from o Point Judith, R. I., to Race Point, at the western end o Fisher's Island; the depth varying from 5 fathoms or less t upwards of 40 fathoms, near Race Point (No. 45). The cu rents are strong, especially toward Race Point, and the tempe atures are low (56° to 64°). The bottom is generally gravell stony, or sandy, occasionally rocky, and but seldom mudd An extensive "scollop-bank" (*Pecten tenuicostatus*) occurred 18 to 22 fathoms, south of Watch Hill, where many interestin species were found, among which was *Leptasterias compta.**

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* This species, when living, is of a beautiful violet color (rarely brown Occasionally there are six rays. It is a delicate and rather fragile species,

c. Off Block Island and south of Montauk Point, L. I., including various fishing banks or "ledges," among which is Coxe's Ledge, about 18 to 20 miles east-southeast from Block Island. Among these localities there are both hard gravelly and muddy bottoms, and some that are sandy. The greatest depths were 32 to 34 fathoms, muddy, about 10 miles southeast from Block Island (Nos. 161, 162); and 25 fathoms, sandy, about 11 miles southeast from Montauk Point (No. 116). Throughout this region the bottom temperatures were found to be low (45½° to 57°), and the fauna correspondingly arctic.

d. The eastern part of Long Island Sound, from Fisher's Island and Gardiner's Island to the mouth of the Connecticut River, the depths varying from 3 or 4 to 50 fathoms, the deepest water occurring a few miles west of Race Point (see Nos. 35, 36, 45, 46), where the tidal currents are very strong and the bottom rocky. The bottoms are variable, but mostly stony or gravelly, and not unfrequently more or less muddy, while the temperature in all the deeper localities was low (58° to 62°) and the fauna arctic.

e. Shallow water localities in the harbors and estuaries near Noank, Stonington, etc. The bottoms are generally muddy and mostly thickly covered with eel-grass (*Zostera marina*).

f. Gardiner's Bay, Long Island. The localities were mostly sandy; the depths 3 to 10 fathoms; and the bottom temperatures were higher (64° to 68°) and the fauna more southern than in the more open sounds.

g. Great Peconic and Little Peconic Bays, and Greenport Harbor, L. I. In these localities the temperatures were much higher (71° to 72½°) than those of the other localities examined, and the fauna was very decidedly southern, including some species not before observed north of Florida and South Carolina. In Little Peconic Bay the bottoms were mostly sandy and shelly (mainly *Crepidula fornicata*, both dead and living), and the depths were 4 to 13 fathoms. In Great Peconic Bay the water was shallow, 4 to 6 fathoms, and the bottoms muddy and rather barren in all the localities examined.

As the faunæ of the various kinds of bottoms and shores, both of the bays and harbors and of the outer cold waters, have been fully described, and most of the species enumerated by me, in a recent work,* it will not be necessary to give, at this time, more than a summary of those species not included in

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rays easily becoming detached. Hundreds of specimens of this hitherto rare species were obtained at this locality. At this place two fishes (a species of *Liparis* and a young hake, *Phycis*) were often found in the gill-cavity of the Pectens with *Pinnotheres maculata*.

* Report on the Marine Invertebrates of Southern New England; Appendix of 1st Report of the U. S. Commissioner of Fish and Fisheries, 1874; also a separate edition.

that report, and now for the first time recorded from the southern coast of New England.

It may, however, be well to state that the fauna of the localities included under the groups *a*, *b*, *c* and *d*, is nearly identical with, though a little more arctic than, that of the outer waters off Martha's Vineyard and Cuttyhunk Island, described in the report referred to; while that of the localities under *e*, *f* and *g*, is essentially the same as that of Vineyard Sound and the estuaries and harbors connected with it, as described in the same work, though the fauna of the Peconic Bays is a little more southern than that of Vineyard Sound or Wood's Hole.

List of species new to the fauna of Southern New England.

In this list I have included, also, a few species added to the fauna during excursions from New Haven, by myself and others, though not obtained during the explorations by the Fish Commission. A few species, marked (*), not new to the fauna, have also been introduced in order to confirm the localities, or to correct the nomenclature. But numerous species, especially of Crustacea and Sponges, added to the fauna last summer, are here omitted, because not yet sufficiently studied. Many of them are undescribed.

Arachnida.

Pycnogonum littorale Müller=*P. pelagicum* Stimpson. West of Race Point, 50 fathoms (Nos. 35, 36).

Thalassarachna Verrillii Packard. Low water and just below, and in pools, among eel-grass, ascidians, hydroids, etc.

Crustacea.

The Crustacea have been identified by Mr. S. I. Smith.

**Hyas coarctatus* Leach. Coxe's Ledge, 21 fathoms; Block I. Sd. (Nos. 85–90).

Dexamine Thea? Boeck. Noank Harbor, among eel-grass.

Melita dentata Boeck. Off Fisher's Island, 9 fathoms; off Watch Hill, 18 fathoms; off Race Pt., 42 fathoms.

Melita, sp. nov. East-southeast 10 miles from Block Island, 32 fathoms, muddy; off Isles of Shoals, N. H., 35 fathoms.

**Ampelisca macrocephala* Lilj.=*Ampelisca*, sp., Smith in Report on Invert., p. 561, pl. IV, fig. 17. Off Buzzard's Bay, 29 fathoms, 1871.

A. typica Boeck. Fisher's I. Sd., 17 fathoms; Block I. Sd., 17 to 19 fathoms (Nos. 74, 75); off Saybrook, Conn.; Noank, among eel-grass.

A. limicola Bate (Stimpson sp.). Noank Harbor, among eel-grass and mud, common; Fisher's I. Sd., 3 to 6 fathoms, sandy.

Ampelisca, sp. nov. Fisher's I. Sd., 7 to 9 fathoms; Block I. Sd., 17 to 19 fathoms (Nos. 74, 75); off Martha's Vineyard, 23 fathoms, 1871.

Xenoclea megachir Smith. Trans. Conn. Acad., 1874. Off Watch Hill, 18 fathoms.

**Hyperia medusarum*. Off New Haven; Noank; Vineyard Sound, and northward, on *Cyanea* and other jelly-fishes.

H. spinipes Boeck. Vineyard Sound.

Several other species of Amphipods, not yet determined, were obtained.

Anilocra, sp. New Haven, in mouth of *Loligo* (Clark).

Tanais vittatus Lilj. Noank Harbor, on piles at low water, and among eel-grass.

Other species of Isopods occurred, among them a curious new species allied to *Paranthura*, from off Watch Hill, 18 fathoms.

Argulus, sp. On *Fundulus pisculentus*, Noank, etc.

Several undetermined species of Lernæans occurred on the sturgeon, drum-fish (one in the gill-cavity and another on the skin and fins), orange file-fish, skate, etc.

Balanus porcatus Costa. Off Watch Hill, 18 fathoms; Long. I. Sd., 24 to 50 fathoms, off Race Point, (Nos. 35, 36, 47).

Annelida.

Sthenelais, sp. nov. Various localities in Block I. Sd.; Fisher's I. Sd.; and Long. I. Sound, 10 to 40 fathoms, on sandy bottoms.

Pholoe minuta Malmgren. Block I. Sound, 17–21 fathoms, mud.

Nephthys cæca Malmgren. Off Stonington, 4–5 fathoms, Aug. 16.

Phyllodoce Grœnlandica Œrsted. Fisher's Island Sound, 12–14 fathoms.

Procerea gracilis Verrill. Noank Harbor.

Eusyllis lucifera Verrill, sp. nov.[*] Noank, on piles, and eel-grass, July 11–31.

Syllis pallida Verrill, sp. nov.[†] Plate III, fig. 6. Noank Harbor, Aug. 15–17.

Syllis, sp. A yellowish white species, with broad obtuse palpi, and moniliform antennæ and cirri; Block Island Sound, 17–21 fathoms, Aug. 21.

Lumbriconereis obtusa Verrill. Noank Harbor, 1–1½ fathoms, mud and dead eel-grass, July 8.

L. acuta Verrill, sp. nov.[‡] Off Block Island, 14 fathoms, Aug. 19.

Ophelia denticulata Verrill, sp. nov.[§] Off Block Island, 14 fathoms, Aug. 19.

Arenicola marina Malmgren. In sand at low-water on a beach about 3 miles west of Noank, July 9.

Trophonia aspera Stimpson.

Brada, sp. Block I. Sound, 17–24 fathoms.

Polydora, sp. Burrowing in dead shells of *Pecten tenuicostatus;* off Block Island; Block I. Sound, 18–21 fathoms.

Polydora, sp. Noank Harbor, 1–1½ fathoms, mud and dead eel-grass.

Praxilla, sp. Off Sea-flower Reef, 6–9 fathoms, sand.

Ancistria capillaris Verrill. Off Block Island, 15–20 fathoms, mud.

Cistenides granulata Malmgren. Long I. Sound, off Race Point, 50 fathoms.

Thelepus cincinnatus Malmgren. Coxe's Ledge, 20 fathoms, sand and gravel.

Polycirrus, sp. A bright red species, with brilliant blue phosphorescence. Off Watch Hill, 18–22 fathoms; Coxe's Ledge, 20 fathoms; Bay of Fundy, low-water to 50 fathoms.

[*] *Eusyllis lucifera* Verrill, sp. nov.

Body rather slender, about 18mm. long; head broader than long, emarginate behind, wider and broadly rounded in front, eyes rather large, the anterior pair a little wider apart than the posterior; palpi broad and short, about one-third as long as the head, blunt or obtusely rounded in front; antennæ stout, tapering, the median considerably longer and larger than the lateral, about equal to twice the breadth of the head; tentacular cirri, like the antennæ and dorsal cirri, transversely wrinkled, the upper ones about equal to the median antennæ, the lower ones a little smaller; dorsal cirri of the first segment longer than the antennæ, the length exceeding three times the breadth of the head; cirri of the two following segments less than half as long; those on the fourth longer and on succeeding segments generally alternately longer and shorter, but mostly less than half the diameter of the body. Color yellowish white, with a dark intestinal line. Phosphorescent with a bright green light.

[†] *Syllis pallida* Verrill, sp. nov. Plate III, figure 6.

Body slender, tapering to both ends, about 15mm. long and ·5 to ·75mm. broad. Head small, length about equal to breadth, rounded behind, produced in front, with a slight antero-lateral angle on each side; palpi large, elongated, lanceolate, obtuse at tips; eyes small, the anterior very wide apart. Antennæ and cirri rather short, distinctly annulated or moniliform; the median antenna largest; dorsal cirri variable in length, or alternately longer and shorter. The longer ones about one half longer than the breadth of the body, composed of 17 or 18 annulations. Color white, with a yellowish intestinal line posteriorly. None of the setæ have setiform tips.

[‡] *Lumbriconereis acuta* Verrill, sp. nov. Plate III, fig. 5.

This is a slender species, easily distinguished by its very long acute head, which is about three times as long as broad. The lateral appendages are short, with a short obtuse upper lobe.

[§] *Ophelia denticulata* Verrill, sp. nov.

Body long and round; 9 anterior segments with short setæ; 18 with slender tapering branchiæ, denticulate on the front edge; 5 caudal segments with long setæ; anal segment with 16 to 18 slender acute papillæ, and two larger lanceolate ones below. Length, 70mm.; diameter, 6mm.

Pista cristata Malmgren. Long Island Sound, off New London, 8 fathoms, mud and sand (loc. 17), July 17.

Chone, sp. North of Block I., 18–24 fathoms, mud and sand, Aug. 6; Coxe's Ledge, 21 fathoms.

Filigrana implexa Berkeley. Fisher's I. Sound; Coxe's Ledge, 21 fathoms.

Spirorbis nautiloides (?) Lamarck. Fisher's I. Sound; Coxe's Ledge, 21 fathoms.

Spirorbis, sp. Shell bicarinated. With last.

Bdellodea, *Gephyrea*, *etc.*

Ichthyobdella, sp. Parasitic on sculpin (*Cottus*, sp.), Thimble Islands, May, 1874. Whitish, with reddish intestinal lines.

Chætoderma nitidulum Loven. Off Block I., 32–34 fathoms, mud (loc. 161, 162.)

Tristoma læve Verrill,* sp. nov. Block I., in mouth of bill-fish.

T. cornutum Verrill.† Block I., on gills of bill-fish (*Tetrapturus albidus*).

Nitzschia elegans Baer. On gills of sturgeon (*Acipenser oxyrhynchus* Mitchel).

Turbellaria.

Lineus viridis V. = *Nemertes viridis* Verrill, Rep. on Invert., p. 628. Noank, low water and 1 fathom, mud.

Tetrastemma dorsalis McIntosh. Noank harbor, on eel-grass and in 1–1½ fathoms, mud, July 8; Casco Bay.

T. elegans Verrill (= *Hecate elegans* Girard). Fisher's Island Sound.

Body about 14mm. long, slender, depressed, tapering to tail; head wider than neck, obtuse or emarginate; eyes conspicuous, nearly in a square, the anterior ones a little nearer together than the posterior. Color of body light yellow, with a broad band of deep brown on each side, leaving a wide dorsal stripe. A pale yellow variety (?), from Noank harbor, has the lateral bands rather ill-defined, consisting of more or less separated specks.

T. candida (?) Œrsted. Noank harbor, 1–1½ fathoms, mud; Casco Bay.

Amphiporus bioculatus McIntosh. Noank harbor, 1–1½ fathoms, mud, July 7; Fisher's I. Sd., 7 fathoms, mud and sand.

A. hastatus McIntosh (?). Block I. Sd., 18–45 fathoms, Aug. 6.

Cephalothrix linearis Œrsted. Noank harbor, 1–1½ fathoms, mud and eel-grass; Casco Bay, low water.

Gastropoda and *Lamellibranchiata.*

**Scalaria Grœnlandica.* Block Island Sound, 17–24 fathoms, Aug. 6.

Velutina lævigata. Off Watch Hill, 18–20 fathoms.

* *Stylifer Stimpsonii* Verrill. Block I. Sound (Nos. 85–90), 6–15 fathoms.

* *Tonicella marmorea* Carpenter. (*Chiton marmoreus* Gould.) Off Block Island.

Philine quadrata. Off Montauk Point, 20–25 fathoms, sand (Nos. 115, 116).

Montagua Bostoniensis (Couthouy, sp.). South of Fisher's Island, 32 fathoms, Aug. 11 (No. 91); off Montauk Point, Aug. 13 (No. 14).

Embletonia fuscata Gould; also var. *remigata* and var. *lanceolata* (Gould). Occurs of all shades of color, from pale flesh-color to dusky brown. Piles at Noank, common on hydroids.

* *Tristoma læve* Verrill, sp. nov.

Length about 15mm., very thin, and broad elliptical; anterior end somewhat produced and broadly rounded; posterior end emarginate; dorsal surface smooth; lower with minute granule-like papillae. Color white, translucent. Posterior sucker large, about a third of the breadth of the body, campanulate, the central area large, with seven angles, from which seven lines radiate; the edge is divided into very numerous denticles; anterior suckers also large, about one-half as broad as the posterior, separated by a space less than their diameter.

† *Tristoma cornutum* Verrill, sp. nov.

Body thin, broad elliptical, or oblong, emarginate posteriorly; anterior end narrowed, produced, and with a short, tapering, tentacle-like process at each angle; upper surface with minute rounded granules and small scale-like wrinkles; smooth beneath. Posterior sucker small, less than one-fourth the breadth of the body, its border divided into much fewer and larger teeth than in the preceding species; anterior suckers two-thirds as broad as the posterior, nearly two diameters apart. Color light red or flesh-color.

Doto formosa Verrill, sp. nov.* Off Point Judith, 10–14 fathoms, Aug. 19.

Idalia modesta Verrill, sp. nov.† Off west end of Fisher's Island, Aug. 25, 1874, on sandy and muddy bottom; Block Island Sound, 17–24 fathoms, Aug. 6; north of Little Gull Island, 40 fathoms (No. 19).

Entalis striolata Stimpson. Off Block Island, 20–25 fathoms (115, 116).

Thracia myopsis. West of Fisher's Island, 7–9 fathoms, Aug. 25.

Tunicata.

Amarœcium glabrum Verrill. Off Block Island.

Bryozoa.

* *Discoporella verrucaria* Smitt=*Diastopora patina* Verrill, in Report on Invert. S. N. E., 1873. Fisher's Island Sound.

**Tubulipora serpens* (irregular variety)=*T. flabellaris* Verrill, in Report on Inv.

Alcyonidium, sp. A smooth red species, incrusting shells. Off Watch Hill, 18–22 fathoms, on shells of *Pecten;* Casco Bay; Bay of Fundy.

Bicellaria ciliata Blainville. Fisher's I. Sound, 8–12 fathoms; Bay of Fundy, 10 to 30 fathoms; off Gay Head, 19 fathoms.

* *Biflustra tenuis* V.=*Membranipora tenuis* Desor; Verrill, in Report on Invert., p. 712. Low-water to 40 fathoms.

Membranipora unicornis Flem., and *var. Americana* (D'Orb.). New Haven, on algæ, etc.; Fisher's I. Sound; Block I. Sound; Casco Bay; Bay of Fundy, etc.

**Cribrilina puncturata* Smitt. Plate III, fig. 2,=*Escharipora punctata* V., in Rep. on Invert., p. 713. New Haven, Noank, etc.

Porina ciliata Smitt. New Haven, on red algæ; Thimble Islands; Fisher's I. Sound; Vineyard Sound, 5–8 fathoms, on shells of *Mactra.*

Escharella pertusa ?.‡ Fisher's I. Sound; Block I. Sound; eastern end of Long I. Sound, 8–40 fathoms; Bay of Fundy.

Hippothoa biaperta Smitt.§ New Haven, on red algæ; Thimble Islands, in pools; off Watch Hill, 3–5 fathoms; Vineyard Sound, abundant.

H. reversa V., sp. nov.‖ Off Gay Head.

Eschara verrucosa, var. propinqua Smitt. Off Buzzard's Bay, 25 fathoms; Nantucket Shoals, abundant; Bay of Fundy, etc.

* *Doto formosa* Verrill, sp. nov. Plate III, fig. 4.

Dorsal papillæ about 8 on each side, stout, ovate, narrowed at base, covered with numerous short, obtuse or rounded, small, white-tipped papillæ. Tentacles slender, the sheaths funnel-shaped, obliquely truncated, with slight emarginations in front and on the outer side. Body translucent white; tentacle-sheaths and dorsal papillæ covered with flake-white specks. Length about ·5 of an inch.

† *Idalia modesta* Verrill, sp. nov. Plate III, fig. 3.

Body oval, very convex. Tentacles long, about equal to breadth of body, slightly serrate or wrinkled; obtuse; cirri at their bases about half as long, very slender, with acute white tips. Branchiæ about 12, rather long, pinnate; two slender cirri close together, behind the bases of the gills on each side; four much smaller conical papillæ in a row on each side. Color of back and branchiæ deep orange-brown, mottled with yellowish and greenish white. Length about ·5 of an inch.

‡ This species has large zoœcia, with numerous coarse pores; oœcia large, subglobular, roughened with prominent granules, and perforated by small pores; apertures with a broad shallow sinus, and small lateral denticles within; avicularia lateral, rarely present, opposite the side of the aperture, broad, obtusely rounded, the point directed toward the aperture.

§ To the genus *Hippothoa*, as limited by Smitt in his recent work on Florida Bryozoa, belong *Escharella variablis* and *Mollia hyalina* of my Report on Invert. of Southern New England. The former is very nearly allied to, if not identical with *H. Isabelleana* of Smitt, Florida Bryozoa, p. 44, plate VIII, figs. 166–168, 1873.

‖ *Hippothoa reversa* Verrill, sp., nov. Plate III, fig. 1. Zoœcia oblong, the front perforated with scattered pores of moderate size; apertures rounded, with a shallow rounded sinus; a small mucro is often present in front of the aperture; avicularia narrow, elongated, acute, somewhat curved, raised above the level of the zoœcia, the point most raised, and directed outward and backward.

Cellepora avicularis Hincks. Off New Haven; Fisher's I. Sound; St. George's Bank; Bay of Fundy, etc. Common on *Sertularia*.

**Lepralia Americana* Verrill. This Journal, vol. ix, p. 415, plate VII, figs. 4, 5, =*L. Pallasiana?* Verrill, Report on Invert., p. 713, 1873. New Haven Harbor; Noank, on eel-grass, etc.; Vineyard Sound; Beverly, Mass.; Long I. Sound, low-water to 40 fathoms (loc. 29), on shells, stones, etc., with oœcia.

Discopora nitida Verrill. This Journal, ix, p. 415, pl. VII, fig. 3, June, 1875. Thimble Islands, on algæ in pool at low-water; Fisher's I. Sound, 3 to 12 fathoms; Vineyard Sound, 5 to 10 fathoms, common on shells.

Echinodermata.

Lophothuria Fabricii Verrill. Off Block Island, 20–21 fathoms (157–9).

Acalephæ.

Bolina alata Agassiz. Newport, R. I. (A. Agassiz).

Campanularia verticillata Lamarck. Block I. Sd., 17–45 fathoms; Fisher's I. Sd., 4–11 fathoms.

C. calceolifera Hincks. Plate IV, fig. 6. Noank, on bottom of scow; Wood's Hole, on piles, 1871.

Clytia noliformis (McCready). Plate IV, fig. 2. New Haven, Sept., 1874.

Gonothyræa hyalina Hincks. Off Watch Hill, R. I., 18 fathoms, July 31.

G. Loveni Allman. Noank, on piles of wharf.

G. gracilis Allman. Fisher's I. Sd., 4–13 fathoms; off Watch Hill, 18 fathoms; off Montauk Pt., 6–20 fathoms.

G. tenuis Clark, sp. nov.* New Haven, on piles of Long Wharf, June, 1875 (Clark).

Obelia bidentata Clark. sp. nov.† Greenport, L. I., on piles, Aug. 5.

O. bicuspidata Clark, sp. nov.‡ Off Thimble Islands, 4–5 fathoms, rocks, Sept., 1874.

Opercularella lacerata Hincks. New Haven, on piles, May, 1875 (Clark).

O. pumila Clark, sp. nov.§ Portland, Me., on piles; off Montauk Pt., 5–15 fathoms.

Halecium articulosum Clark, sp. nov.‖ Long I. Sound, 8–12 fathoms; Coxe's Ledge, 17–21 fathoms; Casco Bay; Bay of Fundy.

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* *Gonothyræa tenuis* Clark, sp. nov. Closely allied to *G. Loveni*, but has narrow, elongated, obconic gonothecæ, tapering gradually to the base.

† *Obelia bidentata* Clark, sp. nov. Plate IV, fig. 10.

Stems clustered, compound, nearly straight, thickly branched; mode of branching irregular, a few of the lower branches sometimes attaining a considerable length and resembling the main stem; branchlets and ends of the branches simple, slender, translucent and very graceful. Hydrothecæ very deeply campanulate with 9–12 longitudial lines, and the rim ornamented with very acutely bidentate teeth.

‡ *Obelia bicuspidata* Clark. sp. nov. Plate IV, fig. 11.

Stem erect, slender, compound, sparingly branched; branches short, slender, ascending and irregularly arranged. Hydrothecæ very deeply campanulate, narrow, very hyaline, and with 8–10 longitudinal lines extending from the distal extremity nearly to the base. The rim is armed with acutely bidentate teeth.

§ *Opercularella pumila* Clark, sp. nov. Plate IV, figs. 7, 8, 9.

Stem rather stout, erect or creeping, slightly flexuous, sparingly branched. Hydrothecæ largest in the middle, tapering very slightly toward the base and rapidly converging toward the distal end; operculum composed of a few convergent segments, not very deep. Gonothecæ fusiform, about twice the length of the hydrothecæ.

‖ *Halecium articulosum* Clark, sp. nov.

Stem stout, compound, sparingly branched, dark brown or black; branches short and irregularly arranged on all sides of the stem; branchlets few and very short; branches and branchlets divided into short, stout internodes by distinct joints placed at right angles to the stem. Hydrothecæ short and wide. Gonothecæ sessile, borne in rows on the upper side of the pinnæ. The female are obovate, and have the opening on one side, nearer the distal end. The male gonothecæ are oblong, subcylindrical.

Halecium halecinum Johnst. Off Saybrook, 14–23 fathoms; Block I. Sound, 17–45 fathoms; Fisher's I. Sound, 4–11 fathoms.

H. Beanii Johnston. Off Saybrook, 6–23 fathoms; Fisher's I. Sd., 7–11 fathoms; Block I. Sd., 17–21 fathoms. Casco Bay; Bay of Fundy.

Lafoea cornuta Lamx., 1812=*L. dumosa* Sars (Flem. sp.). Nantucket Shoals.

Cuspidella costata (?) Hincks. Fisher's I. Sd., 9–11 fathoms.

Calycella syringa Hincks. Plate IV, figs. 3, 4, 5. Fisher's I. Sd., 4–14 fathoms, many localities; Block I. Sd., 17–45 fathoms; off Saybrook, 6–8 fathoms.

Do. var. pygmœa (Hincks). Fisher's I. Sd., 5½–6½ fathoms.

Sertularella polyzonias Gray, *var. gigantea* Hincks = *Cotulina tamarisca* A. Ag., Catal. Acal. N. A. Off New London, 6 fathoms; Gardiner's Bay, 6–8 fathoms; Block I. Sound, 17–24 fathoms.

S. rugosa Gray. Noank, on piles of wharf; off Watch Hill, 17–21 fathoms.

Diphasia fallax Agassiz. Off Watch Hill, 17–21 fathoms.

D. rosacea Agassiz. Fisher's I. Sound, 9–11 fathoms, July 14, with gonothecæ.

Sertularia gracilis Hincks. Hyannis, Mass., on *Sargassum*.

Rhegmatodes Floridianus Agassiz. Greenport Harbor, L. I., Aug. 5; Florida.

Staurophora laciniata Agassiz. Fisher's I. Sound, July.

Corynitis Agassizii McCr. Greenport Harbor, Aug. 5; Charleston, S. C.

Thamnocnida spectabilis Agassiz. Noank Harbor, on bottom of vessel.

Tubularia indivisa Linn. Off Saybrook, 4–9–22 fathoms; Fisher's I. Sound, 4–11 fathoms; Block I. Sound, 17–24 fathoms; off Block Island, 14 fathoms.

Anthozoa.

Cerianthus borealis Verrill, (young specimen). Off Watch Hill, R. I., 18 fathoms, July 31.

Urticina crassicornis Ehrenberg. Fisher's I. Sound, 12–14 fathoms; Block I. Sound, 6–8 fathoms (Nos. 85–90); off Watch Hill, 18 fathoms.

Several sponges, new to the fauna, were also obtained and were studied by Prof. Hyatt. Among them is a curious very slender and delicate, bipinnately branched species from the piles, both at Greenport and Noank. Numerous ciliated Protozoa were also observed, but are mostly not accurately identified. Among them is the curious *Freia ampulla*, on eel-grass, and also on *Sertularia* from 50 fathoms. Some of the Rhizopods were studied by Dr. E. Bessels, and others by Dr. Joseph Leidy.

EXPLANATION OF PLATES.

Plate III.—Figure 1. *Hippothoa reversa* V., enlarged 32 diameters.
" Figure 2. *Cribrilina puncturata* Smitt. Off Noank.
" Figure 3. *Idalia modesta* V., enlarged about 6 diameters.
" Figure 4. *Doto formosa* V., enlarged about 10 diameters.
" Figure 5. *Lumbriconereis acuta* V., much enlarged.
" Figure 6. *Syllis pallida* V., much enlarged.

Figures 1, 2, 3, were drawn by the author; 4, by J. H. Emerton; 5 and 6, by J. H. Blake.

Plate IV.—Figure 1. *Clytia Johnstoni*, enlarged 16 diameters.
" Figure 2. *C. noliformis* V., 16 diameters.
" Figure 3. *Calycella syringa*, 32 diameters.
" Figures 4, 5. The same, showing peculiar variations.
" Figure 6. *Campanularia calceolifera*, 16 diameters.
" Figure 7. *Opercularella pumila* Clark, from Portland, Me.
" Figure 8. The same, creeping form.
" Figure 9. The same, young stems from off Montauk Point.
" Figure 10. *Obelia bidentata* Clark, 16 diameters.
" Figure 11. *O. bicuspidata* Clark, 16 diameters.

Figure 2 was drawn by the author; the rest by Mr. S. F. Clark; all with camera-lucida.

[To be continued.]

ART. XI.—*Examination of Gases from the Meteorite of Feb. 12, 1875*; by ARTHUR W. WRIGHT.

THIS meteorite fell, on the date above mentioned, in Iowa County, in the State of Iowa. By the agency of Professor N. R. Leonard, of the Iowa State University, a large amount of the meteoric mass was collected, and from him, by the courtesy of President Thacher of the same institution, a number of fragments were received by the writer, for an examination, of which a brief notice was published in the preceding number of this Journal. A description of the meteorite, and of the circumstances which attended its fall, by Prof. Leonard, will be given in the next number of the Journal.

The meteorite is of the stony kind, not greatly differing in its general appearance from others of the same class. Numerous small grains of metallic iron and of the magnetic sulphide of iron, or troilite, are scattered through the mass, the iron grains ranging in size from the finest particles, like mere powder, to those of the size of a fig-seed, with occasionally one as large as a grape-seed.

Among the fragments received, there are some which show distinct evidences of a sort of lamination or imperfect stratification, the portions at which the surfaces separated being smoothed down, as if by pressure or friction. Several minute veins are visible, which appear to be filled with material of somewhat different constitution. Their relation to the general mass cannot be distinctly made out, and it is doubtful whether they indicate anything more than that cracks formed in the mass while cooling, and that the fissures thus formed were filled up again, perhaps by the still fluid matter from the interior. They seem to indicate that the mass of which the meteorite probably once formed a part was of great size.

The recent investigations of Prof. Newton, Schiaparelli, Oppolzer, and others, in respect to some of the great meteoric streams, have resulted, on the one hand, in establishing the identity of their orbits with those of certain well-known comets, and on the other, in showing that the bodies belonging to these streams are probably of the same nature as the sporadic or occasional meteorites. It seemed probable, therefore, that an examination of the gases yielded by a freshly fallen meteorite would be likely to furnish important information respecting the tails of comets, and these anticipations were found to be not unwarranted by the results.

The examination was made in the manner described in a previous article,* and with the same apparatus. The first trial, which was made with a quantity of the iron extracted from the meteoric mass, showed that the gaseous contents differed in a marked degree from those obtained from iron meteorites hitherto examined, inasmuch as they contained a very large percentage of carbon di-oxide, with a smaller proportion of carbonic oxide, and a large residue of hydrogen, the two oxides of carbon making about one half of the gaseous mixture. The percentages obtained in the preliminary trial were, CO_2, 35; CO, 14; or 49 per cent of carbon compounds, the hydrogen not having been estimated. This was merely a rude approximation, and the amount of CO is overstated, at the expense of the CO_2. These results were obtained with the particles of iron separated from the powdered stone with a magnet. The residue, however, contained a considerable amount of iron in particles too small to enable them to lift the bits of the stony matrix in which they were enclosed. As this was found to introduce irregularities in the determinations, the portions of meteorite employed in the experiments to be described were finely pulverized in a diamond mortar, and the whole immediately placed in the glass tube to be attached to the Sprengel pump, the iron not being separated from the rest. Larger volumes of the gases were extracted than in the first trial, and the relative proportions of the different constituents carefully determined by analysis.

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* This Journal, III, ix, Apr., 1875, p. 294.

Powder formed from about four cubic centimeters of the solid meteorite was placed in the tube upon the pump, and the air very thoroughly exhausted. It was soon found that the relative amounts of the different constituents driven off by heating the tube varied with the temperature, and the experiments were so conducted that the portions separated at different temperatures could be examined separately.

On applying the heat of the hand to the tube for a short time, a small amount of gas was liberated, too small for anything more than a rude qualitative test as to its composition, which showed the presence of carbon di-oxide and some hydrogen. The tube and its contents were then brought to the temperature of boiling water by surrounding it with a wider glass tube, through which steam was passed for several hours. Gas was given off in considerable quantity, and enough was collected for an analysis. This was found to contain 95·46 per cent of carbon dioxide, and 4·54 per cent of hydrogen, the carbonic oxide, if present, being in too small amount for estimation with certainty. A moderate heat was now applied for a short time with a small Bunsen flame, raising the temperature to 200° or 250°. This separated a still greater quantity of the gases, in the following proportions: CO_2, 92·32; CO, 1·82; H, 5·86. A stronger heat was then applied for nearly an hour, the temperature, however, being kept below that of redness. About three cubic centimeters of gas were given off, which was found to consist of CO_2, 42·27; CO, 5·11; H, 48·06; N. 4·56. The tube with its contents was now brought to a low red heat, which was maintained for half an hour or so, the effect being to liberate nearly the same volume of gas as before, containing CO_2, 35·82; CO, 0·49: H, 58·51; N, 5·18. Finally it was brought to a full red heat, which caused the evolution of much more gas, yielding, on analysis, CO_2, 5·56: CO, 0·00: H, 87·53; N, 6·91. The whole amount of gas given off was about two and one half times the volume of the solid portion of the meteorite employed, but this was not the whole, for the heat was discontinued before its evolution had entirely ceased. If referred to the iron alone, it would be about twenty times its volume.

The following table gives a comparative view of the relative proportions of the gases obtained at different temperatures, the nitrogen being determined as a residue:

	At 100°.	At 250°.	Below red heat.	At low red heat.	At full red heat.
CO_2	95·46	92·32	42·27	35·82	5·56
CO	0·00?	1·82	5·11	0·49	0·00
H	4·54	5·86	48·06	58·51	87·53
N	0·00	0·00	4·56	5·18	6·91
	100·00	100·00	100·00	100·00	100·00

No hydrocarbon compounds of the olefiant series, capable of absorption by fuming sulphuric acid were found, nor any marsh gas. A very small percentage of the latter would have been readily detected. Tests were applied for sulphurous oxide, hydrogen sulphide, and chlorine, but there was no indication of the presence of these gases A small amount of water vapor was driven off by the heat, but not apparently more than the ordinary quantum of hygroscopic moisture which such a substance would absorb from the air.

It will readily be seen, on reviewing the above results, that they show a marked distinction between the iron and the stony meteorites, as to the gases which they contain. For, while hydrogen is the principal gas of the irons, in the Lenarto specimen amounting to 85·68 per cent,* in those of the stony kind, if the one examined may represent the class, the characteristic gas is carbon di-oxide, and this, with a small proportion of carbonic oxide, makes up more than nine-tenths of the gas given off at the temperature of boiling water, and about half of that evolved at a low red heat. It is probable that a portion of the carbon di-oxide is merely condensed upon the finer particles of the iron, while the hydrogen and carbonic oxide are absorbed within it, as it is a familiar characteristic of iron, platinum and some other metals, that in the state of minute subdivision they take up large portions of gas by condensation. The gas thus fixed would be more easily liberated by heat than that really absorbed. This would explain the fact that the amount of carbon di-oxide given off was much greater at a low than at a high temperature, and that the percentage of the hydrogen increased, as was observed in all the trials, with an increase in the temperature to which the material was subjected.

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* Graham, Proc. Royal Soc., xv, 502.

The spectrum of the gases was observed by means of a vacuum-tube, of the kind ordinarily used for spectroscopic work, attached to the apparatus. As was to be expected, it consisted of the hydrogen and carbon spectra together, bearing a general resemblance to those of gases from iron meteorites, but differing from them in the greater relative intensity of the parts due to carbon compounds. At a few millimeters pressure, indeed, the hydrogen spectrum was almost overpowered by them, and was relatively weak. The three middle carbon bands, those in the yellow and green, were very bright, that in the green being most intense of all. In the broad part of the tube these constituted nearly the whole of the spectrum visible, the green hydrogen line being discernible with difficulty, and the others not at all.

These are precisely the three bands observed in the spectrum of some of the comets, and they have the same relative order of intensity. This is a very significant fact, for it shows that it is quite unnecessary to assume the existence of volatile hydrocarbons for the explanation of cometary spectra, as is done by some writers, and that the presence of the two oxides of carbon in such quantity is quite sufficient to account for all that has been observed, taking into consideration the circumstance that the tension of the gases in the cometary appendage must be extremely small, and the energy of the electric discharge very feeble.

There is a high degree of probability that, if a large comet should hereafter approach near enough to the sun to have its nucleus intensely heated, the hydrogen lines will be found in its spectrum, in addition to the bands heretofore observed. One cannot help regretting that a comet like Donati's should have departed into space just early enough to escape observation with the spectroscope.

The spectrum of bright lines or bands indicates that the gas gives out some light directly, in addition to that which it reflects ' The most obvious, and also the most probable cause, for this luminosity is electricity. Certainly a disturbance of the electrical equilibrium would result from the heating effect of the solar rays, and a change of electrical potential, with consequent discharges, would be produced by the motion of the gaseous molecules from the nucleus and from each other, as also by the change in the distance from the sun, provided either of the bodies possessed an electrical charge, as can hardly fail to be the case.

There is another supposable cause for the light which suggests itself, however, in the property of gaseous bodies, that they emit light of the same character as that which they absorb. It is not altogether improbable that the solar radiations absorbed by the gaseous matter, though for the most part converted into heat, would also in part be emitted again as light, and that in the case of volumes of gas filling many cubic miles, the intensity might be sufficient to give a distinct spectrum of bright bands or lines, even though, on the scale of any possible experiment, no trace of such an action can be detected.

These results have thus an important bearing upon the theory of comets and their trains, and if this meteorite may be taken as a representative of its class, they warrant the following conclusions:

1. The stony meteorites are distinguished from the iron ones by having the oxides of carbon, chiefly the di-oxide, as their characteristic gases, instead of hydrogen.

2. The proportion of carbon di-oxide given off is much greater at low than at high temperatures, and is sufficient to mask the hydrogen in the spectrum.

3. The amount of the gases contained in a large meteorite, or a cluster of such bodies, serving as a cometary nucleus, is sufficient to form the train as ordinarily observed.

4. The spectrum of the gases is closely identical with that of several of the comets.

We may consider a comet, then, as merely a meteorite of considerable magnitude, or a swarm of many of lesser size, containing large quantities of carbon di-oxide, with some carbonic oxide and hydrogen, and giving off these gases under the influence of solar heat. The gaseous substance in streaming away forms the train, which is visible, partly by reflected sunlight, and partly by its own light due to some molecular or electrical action, which causes it to give the spectrum of the carbon compounds. The form of the train points to a repulsive influence of some kind, as has been shown by Prof. Norton,* but whether this is due to a specific action of the sun's rays, as is held by Faye, or is electrical in its nature, as maintained by Zöllner, must still be regarded as a subject for investigation.

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* This Journal, II, xxix, 79, 383.

The loss of the gaseous contents by the action of solar heat readily explains the loss of the tail and diminution of brightness observed in the case of several comets in their successive revolutions, and their final disappearance from sight will follow as an inevitable consequence, the number of revolutions necessary to deprive them of their gaseous contents depending principally upon their size and the nearness of their approach to the sun at their perihelia.

The combustion of the hydrogen and carbonic oxide contained in meteorites, when liberated by the heat caused by their entrance into the atmosphere, must contribute greatly to increase the intensity of the heat, and both in this way, and by the consequent sudden expansion of the imprisoned gases, may have much to do with the bursting of the masses, and the violent detonations which attend their appearance.

Yale College, June 16, 1875.

ART. XII.—*Discovery of two new Asteroids;* by Professor C. H. F. PETERS. (Letter to one of the Editors, dated Ashfield Observatory of Hamilton College, Clinton, N. Y., June 6, 1875.)

Two new asteroids were found by me on the night of June 3, and observed as follows:

Asteroid (144) *Vibilia.*

1875, June 3, $15^h\ 3^m\ 2^s$ m. t.	$\alpha = 17^h\ 21^m\ 8^s{\cdot}34$
" 4, 11 17 58 "	17 20 18 ·75

$\delta = -23^\circ\ 20'\ 57''{\cdot}2$
$-23\ 21\ 37\ {\cdot}3$

Asteroid (145) *Adeona.*

1875, June 3, $14^h\ 41^m\ 7^s$ m. t.	$\alpha = 17^h\ 16^m\ 6^s{\cdot}85$
" 4, 11 44 7 "	17 15 14 ·10

$\delta = -23^\circ\ 4'\ 18''{\cdot}9$
$-23\ 6\ 41\ {\cdot}0$

The magnitude of (144) is estimated as the 10th, that of (145) as the 11·5th.

ART. XIII.—*The Discovery of a method of obtaining Thermographs of the Isothermal Lines of the Solar Disc;* by ALFRED M. MAYER.

ON June the 5th, 1875, I devised a method for obtaining the isothermals on the solar disc. As this process may create an entirely new branch of solar physics, I deem it proper that I should give a short account of it in order to establish my claim as its discoverer.

In the American Journal, July, 1872, I first showed how one can, with great precision, trace the progess and determine the boundary of a wave of conducted heat in crystals, by coating sections of these bodies with Meusel's double iodide of copper and mercury, and observing the blackening of the iodide where the wave of conducted heat reaches 70° C. If we cause the image of the sun to fall upon the smoked surface of thin paper, while the other side of the paper is coated with a film of the iodide, we may work on the solar disc as we formerly did on the crystal sections.

The method of proceeding is as follows: Beginning with an aperture of object-glass which does not give sufficient heat in any part of the solar image to blacken the iodide, I gradually increase the aperture until I have obtained that area of blackened iodide which is the smallest that can be produced with a well defined contour. This surface of blackened iodide I call the *area of* maximum temperature. On exposing more aperture of object-glass, the surface of blackened iodide extends and a new area is formed bounded by a well defined isothermal line. On again increasing the aperture another increase of blackened surface is produced with another isothermal contour; and on continuing this process I have obtained maps of the isothermals of the solar image. By exposing for about 20 minutes the surface of iodide to the action of the heat inclosed in an isothermal, I have obtained thermographs of the above areas; which are sufficiently permanent to allow one to trace accurately their isothermal contours. There are other substances, however, which are more suitable than the iodide for the production of permanent thermographs.

The contours of the successively blackened areas on the iodide are *isothermals*, whose successive thermometric values are inversely as the successively increasing areas of aperture of object glass which respectively produced them.

As far as the few observations have any weight, the following appear to be the discoveries already made of this new method. (1) There exists on the solar image an area of sensibly uniform temperature and of maximum intensity. (2) This area of maximum temperature is of variable size. (3) This area of maximum temperature has a motion on the solar image. (4) The area of maximum temperature is surrounded by well-defined isothermals marking successive gradations of temperature. (5) The general motions of translation and of rotation of these isothermals appear to follow the motions of the area of maximum temperature which they inclose; but both central area and isothermals have independent motions of their own.

On projecting the enlarged image of a sun-spot on the blackened surface and then bringing a hot water box, coated with lamp-black, near the other side of the paper, one may *develop* the image of the spot in *red* on a dark ground. A similar method probably may serve to develop the athermic lines in the ultra-red region of the solar and other spectra.

South Orange, N. J., June 11th, 1875.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the Action of a Weaker Acid on the Salts of a Stronger one.*—The importance in chemical dynamics of the question, what is the condition in which several substances exist when in solution, has been oftener recognized than experimentally investigated. Bergmann advanced long ago the theory which is now generally maintained; i. e., that universally bodies combined according to the strength of their chemism. Berthollet, on the other hand, asserted that when different salts were dissolved together, as many bodies were formed as, by the exchange of acids and bases, were possible. Among the experiments made to settle this question, those of Bettendorff* are perhaps the most satisfactory. By studying the action of light on certain solutions, he was led to decide for the view of Bergmann. HÜBNER and WIESINGER, not regarding these experiments of Bettendorff's as sufficiently numerous or comprehensive, have made use of a different method of solving the problem, by making the distinct proposition: Can a dissolved acid expel a stronger acid from one of its salts in solution, without any substance separating from the solution? For these experiments, they used benzoic acid for the weaker and nitrobenzoic acid for the stronger acid. They are both monobasic, are easily obtained pure, are easily separated from each other and from their salts, and can be recognized with certainty. They differ only, apparently, in the strength of their chemism. In the qualitative experiments, barium nitrobenzoate and free benzoic acid were dissolved in a large excess of water, the solution being heated to 80° C. After cooling to 14–17° the solution contained not only the substances originally dissolved, but also free ni-

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* Zeitschr. Chem., 1866, 641.

trobenzoic acid and barium benzoate. The nitrobenzoic acid set free in the reaction, together with the benzoic acid also present, was dissolved by agitation with chloroform or benzol, in which the barium salt is insoluble. In the residue, after the solvent was distilled off, the presence of nitrobenzoic acid was proved by means of sodium. In a qualitative experiment, 1·6592 grams pure barium nitrobenzoate was mixed with the theoretical quantity, 1·1815 grams, of pure benzoic acid and dissolved in an excess of hot water. The cold solution was agitated with chloroform, the latter removed with a pipette, the solvent distilled off, and the residue freed from benzoic acid by a current of steam. The nitrobenzoic acid thus obtained was pure, and weighed 0·2341 grams, being 19·81 per cent of the acid contained in the barium salt used. Additional experiments seemed to show that the quantity of the stronger acid thus set free is dependent on the quantity of the weaker acid present.—*Ber. Berl. Chem. Ges.*, viii, 466, April, 1875. G. F. B.

2. *On Iodous Chloride.*—Brenken has examined, in the laboratory of Lothar Meyer, the formation and properties of iodous chloride, ICl_3. It was prepared by passing dry chlorine over dry iodine, the former being in excess. It is purified by repeated sublimation in a stream of dry chlorine; and then appears as a lemon-yellow crystalline sublimate upon the walls of the tube. Contrary to the statements of the text-books, it does not fuse, even in an atmosphere of chlorine; but even at 25 C. it suffers dissociation into chlorine and hypoiodous chloride, which latter it is that fuses. Under an atmosphere of increased pressure, the dissociation took place at 67°, the constituents reuniting as the tube cooled. In a sealed tube, the decomposition requires a temperature of 86°. If two pretty strong tubes containing iodous chloride be sealed and heated to 70° or 80°, their contents remain yellow. But if now the point of one of them be softened in a gas flame, it will be blown out by the interior pressure, the dissociated chlorine will escape, and the fused brownish red hypoiodous chloride will be left on the walls of the tube. At a still higher temperature, the ICl_3 in the unopened tube dissociates. Closing the opened tube again, and cooling both tubes, the unopened tube contains the yellow iodous, the opened one the brown red hypoidous chloride ICl, which remains for a long time liquid. Experiments to produce a liquid chloride resulted negatively.

In the following paper, Melikoff gives the results of his attempts to determine the vapor density of iodous chloride. He concludes that this vapor at the temperature of 77°, and under one atmosphere of pressure, even in presence of an excess of chlorine, decomposes completely into hypoiodous chloride and chlorine.—*Ber. Berl. Chem. Ges.*, viii. 487, 490, April, 1875. G. F. B.

3. *On the Paraffins of Pennsylvania Petroleum.*—Morgan, under Schorlemmer's direction, has made an examination of the normal hexane and heptane from Pennsylvania petroleum, to test the question of the presence of isomers. The normal paraffins were chlo-

rinated, and then converted into olefines by treatment with alcoholic potash. These olefines were treated with cold hydrochloric acid, each of them being thereby separated into two fractions, one of which dissolved in the acid, while the other did not. The latter fractions yielded secondary alcohols when suitably treated, that from the hexane being methyl-butyl carbinol and that from the heptane being methyl-pentyl carbinol. It hence appears that the derivative olefines are normal, and have the constitution $CH_2{=}CH—C_nH_{2n+1}$. The former olefines, i. e., those soluble in hydrochloric acid in the cold, yielded alcohols which appeared to be secondary, but which need further investigation.

In some remarks upon this paper, **Schorlemmer** says that the above results do not necessarily prove the presence of a third isomeric heptane in petroleum. Heptane when treated with chlorine yields one primary, and may yield three secondary chlorides. If the heptenes from two of these combine with hydrochloric acid in the cold, the alcohols from them would yield on oxidation ethyl-butyl ketone and dipropyl ketone. These on further oxidation would yield propionic and butyric acids. Since Morgan obtained the latter, and as the acetic acid he obtained came probably from the presence in his heptane of a lower-boiling isomer, it is probable that, owing to the method he employed, the propionic acid was overlooked. To decide the question an absolutely pure paraffin is necessary; and the author proposes to make additional experiments with hexane from mannite.—*J. Chem. Soc.*, II, xiii, 301, April, 1875. G. F. B.

4. *Action of Ammonium chloride on Methyl alcohol.*—**Weith** has satisfactorily shown that when ammonium chloride is heated with a sufficient excess of methyl alcohol, it is completely converted into methylamine chlorides. In one experiment two grams NH_4Cl and 12 c. c. $CH_3(OH)$ were heated together in a sealed tube to 280–285° for ten hours. No deposit appeared in the tube on cooling, the liquid existing in two layers. On opening the tube the lighter layer disappeared as a gas, which was proved to be methyl ether $(CH_3)_2O$. The remaining liquid did not contain a trace of ammonium chloride. It was distilled with barium hydrate, the distillate being collected in alcohol and examined. No trace of methylamine or of dimethylamine could be detected. The residue from the evaporation of the alcohol, treated with platinic chloride, gave pure trimethylamine chloroplatinate. Pushing the distillation still further, alkaline vapors were evolved, and the solution afforded on examination tetra-methyl-ammonium chloride. Quantitative experiments showed about 8 per cent of the NH_4Cl was converted into the tertiary base; the rest yielded tetra-methyl-ammonium chloride. Ethyl alcohol acts differently, scarcely a trace of amine resulting.—*Ber. Berl. Chem. Ges.*, viii, 458, April, 1875. G. F. B.

5. *On Glyceryl ether.*—This ether, first discovered by Linnemann and von Zotta, is prepared by dehydrating glycerin by means of calcium chloride. **Von Zotta** has re-investigated the

conditions of its formation. To prepare it 300 grams of dry glycerin were heated to 190° with 45 grams calcium chloride in a balloon of 900 c.c. capacity, until it frothed nearly over. The distillate, freed from the more volatile matters, was agitated with ether, which was removed and evaporated. It left about 22 grams of a product boiling between 150° and 240°. The glycerin was again treated in the same way, so that finally it yielded about $\frac{1}{4}$ its weight of a product boiling between 160° and 210°. This was agitated with potassium hydrate solution to remove the phenol, and rectified. Pure glyceryl ether was thus obtained boiling between 171° and 173°. Treated in this way 14·8 kilos. of glycerin yielded 250 grams of pure glyceryl ether and 18 grams of pure phenol. Glyceryl ether $(C_3H_5)_2O_3$, is an oily liquid miscible with water, alcohol and ether. Specific gravity at 16° is 1·16. On boiling its aqueous solution, it changes to glycerin. Treatment with bromine converts it into dibromhydrin. Solid potassium hydrate acts on it to produce phenol.—*Bull. Soc. Ch.*, II, xxiii, 310, April, 1875.

G. F. B.

6. *On the Purification of Salicylic Acid.*—The commercial importance now attaching to salicylic acid, makes it desirable to contrive a method for purifying it. The methods by which it is manufactured leave it strongly colored; and Kolbe's method for purifying it by converting it into ether, entails so much loss as to be practically useless. Since it cannot be sublimed without decomposition, alone, this method is impracticable; but RAUTERT has succeeded in subliming it completely in a current of superheated steam, and in this way readily obtaining it pure. Recrystallization from hot distilled water gave the salicylic acid in beautiful snow-white crystals.—*Ber. Berl. Chem. Ges.*, viii, 537, April, 1875. G. F. B.

7. *On a New Method of Testing Quinidine Sulphate.*—HESSE proposes a new method of testing quinidine sulphate, founded upon the action of ammonia and water upon its iodhydrate, as compared with the iodhydrates of quinine, cinchonine and cinchonidine. Half a gram of the sulphate to be tested is warmed to 60° C. with ten cubic centimeters of water, half a gram of potassium iodide is added, well stirred, allowed to cool and after an hour, filtered. If the specimen was pure, the filtrate remains clear upon the addition of a drop of ammonia. If a precipitate is produced, quinine, cinchonine or cinchonidine is present. A less delicate test, but one commercially satisfactory, is as follows: Half a gram of the material is digested at 60° for about five minutes with 40 c.c. of water, and then mixed with three grams of potassio-sodium tartrate (Rochelle salt). If the specimen contains but little or no quinine or cinchonidine, the solution remains clear; if not, a precipitate appears, when the admixture amounts to six per cent. This precipitate, after standing an hour, is filtered off, washed with a little cold water, and the filtrate after warming is mixed with half a gram of potassium iodide. If quinidine be present, a precipitate of quinidine iodhydrate is produced. In

this case, the liquid is allowed to stand an hour, filtered off, and a drop of ammonia added to the filtrate. A precipitate indicates the presence of cinchonine, in amount at least two per cent. The presence of calcium and sodium salts in the quinidine sulphate is readily detected by solution in chloroform when these are left behind. If quinine or cinchonidine are also present, one gram of the substance is to be treated with seven cubic centimeters of a mixture of two volumes chloroform and one volume 97 per cent alcohol.—*Liebig's Annalen*, clxxvi, 322, May, 1875. G. F. B.

8. *Mechanical Equivalent of Heat.*— M. H. J. PULUJ has devised an apparatus of very simple construction for measuring this constant, suitable for use in the lecture-room. It consists of a calorimetric and dynamometric portion, suitably connected with an oscillation machine, such as is found in every physical cabinet. The calorimetric part is formed by two hollow truncated cones of cast-iron, one fitting into the other, the inner not quite reaching to the bottom of the outer, and appearing a little outside of it. The outer cone can be fixed coaxially in the vertical bobbin of the machine. The inner cone contains mercury. If the machine is put in motion and the inner cone fixed, heat is generated by the friction of the surfaces.

An arrangement which is an inversion of Prony's check serves for the measurement of the work which is converted into heat. On the wooden lid of the inner cone a light wooden beam is screwed horizontally. A perforation passing through the beam and lid receives the thermometer. At some distance from the beam is a fixed pulley, on a level with it, over which a thread, to which a scale is suspended, is slung and is fastened to the end of one arm of the beam; the other arm serves as a counterpoise. When the machine is set in motion, the interior face of the outer cone rubs against the surface of the inner one and tends to turn the beam, which is fastened to the latter, in the direction of the motion. With a certain load the horizontal part of the thread and the axis of the beam will include a right angle. From the length of the beam-arm, amount of the load and number of rotations, the work converted into heat, and from the water value of the calorimeter and the rise of temperature, the quantity of heat generated, can be calculated.

The memoir contains also the development of the theory of the apparatus, taking into consideration the heat radiated from the calorimeter, and finally the numerical calculation of the values of the mechanical equivalent of heat from 28 experiments. The mean of the results, the 425·2, with the mean error ±5·4, is in excellent accordance with Joule's result, 424·9, and may be regarded not only as a fresh corroboration of it, but also as a measure of the accuracy with which the experiments can be conducted by means of this simple apparatus; these occupy but very little time, an experiment proper lasting but 30–60 seconds, on which account the apparatus may be recommended for lecture-experiments.—*Roy. Acad. Vien.*, April, 1875; *Phil. Mag.*, xlix, 416. E. C. P.

9. *Action of Magnets on Geissler Tubes.*—M. J. CHAUTARD has studied somewhat at length the effect of a magnet on rarefied gases enclosed in capillary tubes, illuminated by an induced current. The gases or vapors employed were H, N, O, CO_2, CO, C_2H_4, S, Se, I, Br, Cl, SO_2, SiF_4, $SnCl_4$. Bodies of the chlorine family are those which gave the most concordant and brilliant effects. An increase of temperature diminishes the action of the magnet. This is shown by allowing the current to pass for some time, when the magnetic influence is enfeebled or destroyed. The presence of the gas is an important element to be considered, as by varying it the current may be intercepted or the appearance of the light greatly altered. Similar effects are attained by varying the strength of the current, and are most marked when the current is feeble. The phenomena are the same whether a Holtz machine or coil are used as a source of electricity, and are independent of the direction through the coil or gas. The form of the armatures is important; they should be plain and surround the tube for the greater part of its length. The effect of a single pole is slight, or of both poles when more than five millimeters distant from the tube.

The stoppage of the current by the magnet was shown by using two tubes, one of which was greatly narrowed and placed between the poles. As long as the magnet did not act the current passed through both tubes, but on closing the circuit the light was immediately extinguished in the narrow tube. This experiment succeeds best with Cl, I, S and Se. The cessation of the light occurs in two distinct cases, when the pressure is so slight that the current will scarcely pass, or when it is sufficient for the spark to attain nearly the same limit. Under the action of the magnet the luminous thread undergoes a contraction sometimes visible to the naked eye. An alteration in the color or in the spectrum is sometimes thus produced. This effect is scarcely perceptible with H, N and CO_2. The narrowing does not extend more than a few millimeters from the poles, so that with a tube of sufficient length we can, by varying the position of the spectroscope, view first the normal spectrum, and then that due to the magnet. The change is most marked with Cl, Br, $SnCl_4$, SiF_4, SO_2. Direct measures have shown that with these gases the new rays thus developed are wholly distinct from those characterizing the normal spectrum of the same gas.—*Comptes Rendus*, lxxx, 1161. E. C. P.

10. *Effect of Electricity of high tension on Liquids.*—M. G. PLANTE has applied his secondary pile to the study of these phenomena. The gradual diminution of the current, instead of being an objection, has the advantage that, without changing the other conditions, it enables us to study the effect of currents of every strength less than that first employed. Two batteries, each of twenty cells, were charged and connected with a wire 80 cms. in length, and 1 mm. in diameter. A voltameter was also placed in the circuit with electrodes of platinum wire, the positve wire only being immersed. When the negative electrode was placed in the liquid, a sheath (*gaîne*) of light was produced around it,

without sensible disengagement of gas; the other electrode set free a little gas, and the wire was not heated to redness. After two or three minutes, however, the sheath of light disappeared, gas was set free abundantly at both electrodes, and the wire at the same instant was heated to redness. To show these effects still better, ten batteries were used, equivalent to 300 Bunsen cells, connected for tension and capable of heating to redness a platinum wire 10 meters in length and 3 mms. in diameter. These batteries are charged by two Bunsen cells in the course of an hour. Repeating the above experiment with them, the wire, on touching the liquid, fused or volatilized with a flame variously colored, according to the metal employed. If the liquid merely contained traces of acid, so as to avoid complete fusion of the metal, a continuous series of sparks were produced with a noisy crackling like the discharges of an induction coil, and lasting sometimes several minutes. The most remarkable effect was obtained with a saturated solution of common salt. The voltameter being so mounted that the wires could be very gradually immersed, the negative pole was first plunged in about a millimeter, and the positive terminal then made to touch the surface. A small luminous and perfectly spherical globe then formed around it with a loud rattling noise; raising the wire, it increased in size until it had attained a diameter of nearly a centimeter, and then, on lowering the wire, began to revolve rapidly. After attaining a certain speed, it detached itself as if attracted by the other electrode, and disappeared with an explosion and flame at the negative pole. This globule is not gaseous but liquid in a speroidal state, illuminated by the current it transmits. If instead of immersing the wire it is brought near the glass sides of the vessel, a bright flash of a curved or zigzag form darts over a distance of three or four centimeters to the electrode, with an explosion as before. Soon after another spark is formed, and so on for several minutes. These luminous balls present a marked resemblance to, and offer a probable explanation of, the curious phenomena known as globular lightning. The cloud and earth form the electrodes, and the moist air replaces the liquid. Probably the balls, in the case of lightning, are not liquid, but are formed of ponderable matter charged with electricity. These experiments suggest the probability, if it is ever possible to try the experiment, that balls of lightning are charged with positive electricity.—*Comptes Rendus*, lxxx, 1134. E. C. P.

II. GEOLOGY AND NATURAL HISTORY.

1. *Greenland Glaciers: Subglacier rivers.*—Prof. Nordenskiöld travelled thirty miles inland on the Great Greenland glacier and found its surface there to be only 2,000 feet above the sea, showing that the land over the interior must be comparatively low—and much lower than the islands bordering it, which average 2,000 feet in height In summer, the snow which covers the glacier melts and rivers of ice-cold water flow over the surface and

descend in the crevasses to unknown depths. These crevasses are exceedingly numerous, and apparently increase in number on passing toward the interior, where not a plant, or stone, or patch of earth is seen over the great ocean of ice and snow, 1,200 miles in extent from north to south and 400 in breadth. Dr. Rush believes that the outpour of the Greenland precipitation of snow and rain in the form of glacier ice amounts to only two inches, while he estimates the fall at twelve inches; so that, as the evaporation must be very small, a large portion of the remaining ten inches must be carried off by subglacier rivers.—"Nature" of April 8th, from which these notes are taken, has a geological map of the Arctic region between the meridians of longitude 20° W. and 80° W., prepared by Mr. C. E. De Rance, the author of a series of papers on the Arctic regions, in the same Journal.

2. *Bulletin of the U. S. Geological and Geographical Survey of the Territories:* Nos. 2, 3 and 4, second series. 68 pp. 8vo, with maps and plates. Washington, May 15th, 1875.—The papers contained in number 2 are: Monograph of the Genus *Leucosticte* of Swainson, or gray-crowned purple Finches, by ROBERT RIDGWAY; the Cranial and Dental Characters of Geomydæ, by Dr. ELLIOTT COUES; Relations of Insectivorous Mammals, by T. GILL; on the Natural History of the U. S. Geological Survey of the Territories for 1874 (a list of the Mollusks), by ERNEST INGERSOLL. Number 3 contains the following papers: Means of Communication between Denver and the San Juan Mines, by A. D. WILSON; on the Mines and Geology of the San Juan Country, by F. M. ENDLICH; on the Topography of the San Juan Country, by F. RHODA; on some peculiar forms of erosion in Eastern Colorado, with heliotype illustrations, by F. V. HAYDEN. No. 4 contains reports on the features of the Colorado or Front Range of the Rocky Mountains, by F. V. HAYDEN, with a number of plates; on the Tertiary Physopoda of Colorado, by S. H. SCUDDER; on a Natural Arrangement of the Falconidæ, by R. RIDGWAY.

Mr. Endlich, in his remarks on the San Juan Country, states that for more than 4500 square miles, the surface is covered continuously with volcanic rocks. The following succession of strata was observed: (1) 800 feet of trachyte, mostly light-colored; (2) 1200 feet of red to brown trachyte, containing sanidite and some mica; (3) 2000 to 2250 feet red to brown trachyte; (4) 3000 to 4000 of trachyte of various characters, making in all a thickness of 7000 to 8000 feet. Above follow doleritic beds.

The veins of the region, affording argentiferous galenite, sphalerite, tetrahedrite, brittle silver, etc., and some of them gold, occur in the trachyte, and are referred to the Cretaceous or the beginning of the Tertiary, the era of the trachytic eruptions.

Going south and west the volcanic strata thin out, revealing the rock they overlie. Near the head of Cunningham Creek the volcanic rocks form the crests of ridges, while the valleys cut through into metamorphic rocks, which are quartzyte, chloritic rocks, mica schist, staurolitic schist, gneiss, and a coarse-grained granite.

The plates illustrating the brief reports of Dr. Hayden are excellent.

Among the results of the same expedition, under the Interior Department, there have recently appeared also—(1.) A "Preliminary map of Central Colorado," showing the region surveyed in 1873 and 1874; Primary triangulation by G. T. Gardner, topography by G. R. Bechler, H. Gannett, A. O. Wilson and S. B. Ladd.

(2.) A colored Geological map of the sources of the Snake River with its tributaries, and of portions of the headwaters of the Yellowstone, by G. R. Bechler, topographer, and Frank H. Bradley, geologist.

(3.) A colored Geological map of part of Montana and Wyoming Territories, embracing most of the country drained by the Madison, Gallatin and Upper Yellowstone Rivers; Geology by F. V. Hayden, assisted by A. C. Peale; drawn by H. Gannett from notes and sketches by A. Burck, chief topographer of the Yellowstone division. It has contour lines.

3. *Second Geological Survey of Pennsylvania*, 1874. Report "B." On the Mineralogy of Pennsylvania, by F. A. GENTH. With an Appendix on the Hydro-carbon Compounds, by S. P. SADTLER; 206 pp. 8vo. Harrisburg, 1875.—Dr. Genth has included in this volume the results of his long study of Pennsylvania minerals, with many facts obtained from others who have labored in the same field. The minerals are arranged according to the system adopted in Dana's Mineralogy, and under each species is given, in a few words, its chief characteristics, and then in more detail the description of the localities in the State. Dr. Genth has added also a large number of new analyses recently executed by himself.

Report "J." On the Petroleum of Pennsylvania, by HENRY E. WRIGLEY. With a map and profile of a line of levels through Butler, Armstrong, and Clarion Counties, by D. J. Lucas; also a similar profile along Slippery Rock Creek, by J. P. Lesley. 122 pp. 8vo. Harrisburg, 1875.—Mr. Wrigley treats of his subject in all its bearings, historical, geographical, geological and economical; his report will be found of great value by all interested in the subject.

4. *Report upon the Reconnaissance of Northwestern Wyoming, including Yellowstone National Park*, made in the summer of 1873; by Wm. A. Jones, Captain of Engineers U. S. A. 326 pp. 8vo, with numerous maps.—This volume consists of a General Report of the region explored by Captain Jones; an Astronomical Report, by Lieut. S. E. Blunt; a Geological and Archæological and Philological Report, covering 200 pages, by Prof. T. B. Comstock; a Report on Mineral and Thermal waters, by Surgeon C. L. Herzmann; a Botanical Report by Dr. C. C. Parry, and an Entomological Report by J. D. Putnam.

The Report of Prof. Comstock gives much information on the features and structure of the region, with details respecting the geysers and hot springs of the Yellowstone Park. The Lower

Silurian of the Primordial and Canadian periods was observed, and perhaps Upper Silurian beds, but no Devonian, the next strata overlying being Carboniferous; whether the Subcarboniferous is included he leaves doubtful. Above the Carboniferous come the Triassic, Jurassic, Cretaceous and Tertiary. A colored geological map of the region accompanies the Report.

5. *Report of Progress of the Mineralogical, Geological and Physical Survey of the State of Georgia for the period from Sept.* 1 *to Dec.* 31, 1874; by GEORGE LITTLE, State Geologist.—This Report of progress gives brief notices of some of the mines and ores of the State, with also accounts of valuable localities of marble, roofing slate, mica for stove doors and other utensils of economical value, with a notice of some of the localities of minerals. There are also analyses of limonite, dolomite, limestone, etc.

6. *Michigan, being condensed popular sketches of the Topography, Climate and Geology of the State;* by A. WINCHELL, LL.D., late Prof. of Geology, Zoology and Botany in the University of Michigan. 122 pp. 8vo. 1873. Extract from Walling's Atlas of Michigan.—This is a valuable résumé of the results obtained by Prof. Winchell in his investigations into the Topography, Geology, and Climate of Michigan. The volume is accompanied by four maps—one a topographical map with contour lines; the second a colored geological chart of Michigan prepared from the latest observations; and the other two climatological charts, giving the isothermal lines for spring, autumn, and the year.

7. *Analysis of Ægirite from Hot Springs, Arkansas;* by J. LAWRENCE SMITH.—The occurrence of ægirite at Hot Springs (Magnet Cove), Arkansas, was first noticed by C. U. Shepard in this Journal, vol. xxxvii (p. 407), for 1864. The following are the results of my analysis of the mineral:

Silica,	51·41
Alumina,	1·82
Peroxide of iron,	23·30
Protoxide of iron,	9·45
Lime,	2·03
Magnesia,	0·31
Soda, with *trace of potash*,	11·88
Titanic acid,	·13
	100·33

The specific gravity is 3·53.

8. *Second Appendix to Dana's Mineralogy;* by EDWARD S. DANA; 64 pp. 8vo. New York, March, 1875. (John Wiley & Son.)—The first Appendix to Dana's Mineralogy was prepared in 1872, by Prof. G. J. Brush. The present Appendix follows that after an interval of three years, and is intended to make the work complete up to 1875. It includes full descriptions of 92 mineral species announced as new since March, 1872, with also explanations of a considerable number of other names new to the science but not properly belonging to distinct species. It also embraces references to all mineralogical articles published in the many

Journals since the issue of the Mineralogy in 1868; these references are placed under the name of each species, to which the article relates, the species being arranged alphabetically. This Appendix is consequently an index to all the mineralogical literature of the past seven years. A list of mineralogical books and of important general memoirs covers the few opening pages.

9. *Mineralogical Contributions*, V., by Prof. C. KLEIN, in Heidelberg.—Prof. Klein has investigated with great minuteness the crystals of octahedrite (anatase) from the Binnenthal. He distinguishes four distinct types of forms, and shows that they are remarkable for their large number of planes. He also proves it to be very probable that the mineral from this locality which was described by Kenngott as wiserine is in reality octahedrite.

E. S. D.

10. *On the presence of Vanadium in rocks.*—Dr. A. A. Hayes, in a paper read before the American Academy of Sciences, Boston, in January last, states that he had detected vanadium in many rocks associated usually with compounds of phosphorus and of manganese. His mode of examination for the detection of vanadium is described in detail, but no complete analyses of any rocks are presented. The author proposes in a future paper to give a tabulated list of the rocks. He also states the occurrence of vanadium in the well water of Brookline, near Boston.

11. *Pseudomorph after crystals of Labradorite from Verespatak;* by G. TSCHERMAK.—Feldspar-like crystals occur in the quartz-andesite of Verespatak, Transylvania, which are pseudomorphs after labradorite. Their form, however, is in many cases well retained, though the luster of the planes is gone. They were found to be twins in all cases, the several laws of twinning being similar to those often observed in orthoclase. Sometimes the crystals are very complex, being made up of a number of individuals. They have a snow white or sometimes yellowish color and earthy fracture, and though firm in the fingers are easily ground in the mortar to a soft, white powder. An analysis gave Sipöcz:

$$\ddot{\mathrm{S}}\mathrm{i}\ 55{\cdot}96,\ \overset{\cdots}{\bar{\mathrm{A}}}\mathrm{l}\ 31{\cdot}34,\ \overset{\cdots}{\bar{\mathrm{F}}}\mathrm{e}\ 1{\cdot}16,\ \dot{\mathrm{M}}\mathrm{g}\ 1{\cdot}73,\ \dot{\mathrm{C}}\mathrm{a}\ 0{\cdot}65,\ \dot{\mathrm{N}}\mathrm{a}\ 0{\cdot}18,\ \dot{\mathrm{K}}\mathrm{a}\ 4{\cdot}96,\ \dot{\mathrm{H}}\ 5{\cdot}41 = 101{\cdot}39$$

Under the microscope the mass is seen to consist of two kinds of minerals in fine scales: one colorless, the other slightly greenish and having the appearance of a potash-mica, to which it is referred by Tschermak. Deducting from the above analysis the latter, which amounts to about one-fourth of the whole, and also deducting some unchanged feldspar, limonite, etc., present in small quantities, Professor Tschermak finds that the remainder corresponds to the formula, $3\ddot{\mathrm{S}}\mathrm{i},\ \overset{\cdots}{\bar{\mathrm{A}}}\mathrm{l},\ \dot{\mathrm{H}}$, which he regards as expressing the composition of the colorless scaly mineral referred to, it being thus a hydrous alumina silicate differing from kaolinite. The change which has taken place in the original material consists essentially in an exchange of potash for soda, and of water for lime. This loss of lime has been often observed in other similar cases.—*Tsch. Min. Mitth.*, 1874, iv, p. 269.

E. S. D.

12. *Fungi; their Nature and Uses;* by M. C. Cooke, M.A., LL.D., edited by Rev. M. J. Berkeley, M.A., F.L.S.—*The International Scientific Series.* Appleton & Co., New York. 1875. pp. 299.—The earlier volumes of the International Series which have come under our notice were so excellent, that it may not be reasonable to expect all their successors to come up to the same standard. Moreover, in such a treatise as the present, we are not to look for any substantial addition to our knowledge, but rather for a good popular exposition of what is already known in the science, including the interesting and important discoveries of the last few years, much of which has not yet been made available to general students or amateurs, at least in the English language. A book upon this subject, at once plain, practical and scientific, but not over-technical, calculated to subserve the wants of students and gratify a general interest, was much wanted, and we may add, is in a good degree wanting still. The expectations which the honored name of Mr. Berkeley upon the title-page inspired are diminished by his prefatory announcement, that, although he had promised to write the book, he had been obliged, through ill health and manifold engagements, to turn over the work to Mr. Cooke; but that he had editorially read the manuscripts and the proofs, subjoining some notes. The volume is therefore substantially that of Dr. Cooke, although we may conjecture that the chapters on the uses and notable phenomena of Fungi owe much to the veteran English mycologist's store of information.

The important chapters on the nature, structure, reproduction and classification of Fungi are not so satisfactory; partly because the plan of the work breaks up and widely separates matters which must need be connected to give a clear idea of the life-history of any fungus; partly because too many topics are entered upon and dropped without adequate explanation, and too many technical terms are used without explanation; and sometimes, it would seem, from imperfect apprehension of German authorities used, perhaps only in translations and deceptive magazine extracts. At least one may infer, from some points in the treatment of the most interesting group of *Mucorini* and of the *Myxogastres*, that the author can hardly have read Brefeld's *Untersuchungen über Schimmelpilze*, although he refers to it.

"As the work is intended for students, the author has had no hesitation in repeating what has been stated in former chapters where it has been thought to prove useful." Our own opinion is that repetitions are specially to be deprecated in a text-book, and may always be avoided by a good arrangement and methodical treatment. The author, moreover, "has had no hesitation" in devoting more than half a dozen of the earliest pages of the volume to a discussion of such a vexed and wholly subsidiary question as that of the Schwendener hypothesis of the algo-fungal nature of *Lichenes;* and he pronounces confidently against it. This will hardly close the controversy, nor be of much use to the student. More to the purpose would have been some account of the methods of study adopted by modern investigators who have recently given a new and deep interest to mycology. Useful as this volume must be, and full as it is of interesting information about Fungi, it does not altogether supply the long-felt want of an elementary text-book. A. G.

13. *Æstivation in Asimina.*—The æstivation was formerly thought to be valvate in all *Anonaceæ.* In the Genera Am. Bor. Or. Illustrata, vol. i, 1848, it is mentioned that the petals of each set are more or less imbricated in *Asimina*, as also in some other genera. The petals enlarge so much before and during expansion that the proper æstivation needs to be determined in young flower-buds. A subsequent examination of these, in *A. triloba*, showed that there was hardly any overlapping in an early state. Accordingly, in the later editions of my Manual, no exception to the ordinal character, "valvate in the bud," is alluded to. In the Genera Plantarum, Bentham and Hooker distinguish their two tribes, *Uvarieæ* and *Unoneæ* by the æstivation of the petals,—more or less imbricated in the former, valvate in the latter, to which they refer *Asimina.* Last spring I had an opportunity to examine, on the living plants and flower-buds of *A. grandiflora*, the species in which the exterior petals are most accrescent, and the interior perhaps least so, the one which most resembles *Uvaria* in the appearance of the blossoms. The sepals appear to be truly valvate. The outer petals are decidedly imbricated, their tips well overlapping in the order 1, 2, 3, in the early bud, and remaining so during the great enlargement; but down the sides they do not overlap, nor are their bases contiguous. The inner petals are remote in bud: moderately accrescent, they remain proportionally small, and from first to last do not come into contact, the margins above the middle becoming revolute in anthesis, while the base grows more and more deeply concave and papillose-ridged. This portion is frequented by thrips, or such-like insects, as also is the mass of stamens as soon as the anthers open. The flowers are proterogynous, the stigmas being early in good condition, the anthers discharging pollen only when nearly ready to shrivel and fall.

On examining good fresh flower-buds of *A. triloba*, in May, I find that the sepals are truly valvate at first, but separate more or less as the bud swells. The exterior petals, a little distant at their bases, very slightly overlap as they meet at the summit, while just below the margins become a little revolute. The interior petals are similar, but rather more distant at base, and rather less obscurely, yet very slightly, overlapping at the very tips. As they increase in size they slightly assume the imbricated position which becomes conspicuous in the outer petals.

I conclude that the tribe *Unoneæ* cannot be distinguished from the *Uvarieæ*, at least upon the characters assigned, and that the one kind of æstivation passes by gradations into the other.

A. G.

14. *Text-book of Botany: Morphological and Physiological:* by JULIUS SACHS, Professor in the University of Würzburg: translated and annotated by A. W. BENNETT, M.A., and W. J. THISTLETON DYER, M.A. 858 pp. large 8vo. London. (Macmillan.)—The third and fourth editions of Sach's German treatise, and Van Tieghem's French translation of the third, have been noticed in this Journal. In point of mechanical execution, the volume now under consideration compares very favorably with its predecessors, which have received high praise. The translation into English is based, like Van Tieghem's, on the third German edition, and one is therefore tempted to first compare English with French editing. Considered as a translation merely, the former is superior. It is very close, idiomatic, and as clear as the original. In some instances the selection of English words to convey the expression involved in a compound German word is most felicitous. Considered as an annotated revision, the English edition is inferior. The solid pages, for instance those devoted to *Tension*, remain as in the German, whereas in the French, skillful paragraphing has rendered the whole far easier of reference. The use of significant headings for the paragraphs has enabled Van Teighem also to give a synoptical table of contents of much value. It has been shown in a previous notice that Van Tieghem's table presents a clear and exhaustive outline of the whole work. Nothing more than a list of sections replaces this in the present volume. This lack, having been so well met by a French editor, is all the more apparent and regretable. With this our adverse criticism is exhausted. This conscientious translation is a valuable and timely gift to botanical students; that our American fellow students may be more ready to welcome it, we present a sketch of the work.

The Text-book is divided into three parts: General Morphology, Special Morphology and Physiology. The first treats of the nature and forms of cells, the formation of cells, the structure of the cells, their constituent parts and occasional contents. Tissues defined as aggregates of cells which obey a common law of growth are next considered with reference to their formation and forms. Three systems of tissues are fully described, epidermal, fibro-vascular bundles, and fundamental. Here are presented comprehensive studies of laliciferous and vesicular vessels, sap-conducting intercellular spaces and glands. The primary *meristem*, the uniform tissues made up of cells, all of which are capable of dividing, is next treated of in connection with the terminal *point* of growth. Here a somewhat confused terminology (Hanstein's) is introduced. Frequent cross-references to illustrations further on would have made this free from the ambiguity which now characterizes it. The fault lies, however, at the door of the German abstract of Hanstein's and Reinke's work. The outward shapes of plants, differences between structural members and working organs, and metamorphosis, come next. Most of this chapter is a literal translation of the least satisfactory portion of the third edition. Much of it has been rewritten in the fourth German, and for the better.

Book second is wholly devoted to special morphology and outlines of classification. The former is based on excellent abstracts of monographs and is most serviceable. The "outlines" are of little use to our botanists except as an illustration of what systematic botany would become if it were left to the physiologists. Already since this edition was written, Prof. Sachs has changed his system; and it is still in a state of unstable equilibrium, or want of equilibrium. There is hardly a more profitable task for a young botanist than the comparison of the systematic sections of Book Second with Dr. J. D. Hooker's appendix to the English translation of De Maout's and De Caisne's work. The former is by a physiologist, the latter by a morphologist. Vegetable Physiology is given in Book Third. The excellence of this digest is apparent on a hasty perusal. It becomes more obvious when the book is used with advanced students as a hand-book in daily work. From constant use of the German and French versions in the laboratory during the past year, by students of two grades, the present writer can give it honest praise. When it is supplemented by the Experimental Physiology of the same author, the laboratory is well equipped. Vegetable Physiology is treated of under the following heads; Molecular forces in the plant; Chemical processes in the plant; General conditions of plant-life; the Mechanical laws of growth; Periodic movements of the mature parts of plants, and movements dependent on irritation; the Phenomena of sexual reproduction; the Origin of species. In addition to the copious citations made by Prof. Sachs, the translators have given many references to memoirs now accessible to English students in translations or abstracts. It seems to have been the aim of the editors to render the work of the greatest value to botanical students who are unfamiliar with German. It is a great pleasure to commend this volume, most heartily, as a good translation of the German hand-book to advanced botanical students. G. L. G.

15. *Zur Abwehr der Schwendener-Bornet'schen Flechtentheorie* (Contribution to a refutation of the Schwendener-Bornet Lichen theory); by Dr. G. W. Koerber. 30 pp. 8vo, Breslau, 1874.—In this essay Dr. Koerber, one of the first Lichenologists of Europe, has cast the weight of his authority against the theory which has received considerable currency, that Lichens are a compound of an Alga (the gonidia) and a Fungus (the hyphæ). He maintains, first, that the "nicht-gonimische," i. e., the hyphæ, and the lichens themselves are not Fungi, citing, in support of this proposition, the known chemical and other differences between the two, and the fact that many lichens are altogether destitute of hyphæ. He is curious to know how, on the theory of Schwendener, the formation of a thallus from the copulation of Fungoid hyphæ and an Alga can be explained. Secondly, he maintains that the gonidia of Lichens are not Algæ, because: 1, in true Algæ the gonidia never produce hyphæ, while this is of common occurrence in the spores of Lichens; 2, that if the contrary were true, it is strange that in every Lichen several types of Algæ are necessary for the production

of the Lichen, and still more strange that in nature these various Algæ occur without any further result; 3, because many forms of gonidia are not known to Algologists as such, because they have never been seen in a free state; 4, because the Lichen gonidia correspond in their forms only to those Algæ which reproduce themselves by division and not to those which propagate by sexual reproduction, the former process being only a physiological one common to many or all lower vegetable cells, and destitute of systematic value. The transformation of gonidia into zoöspores, "Schwärmzellen," observed by Famintzin and others, is regarded as a process also common to low vegetable cells. The so-called "asynthetic gonidia," i. e., those which occur without the thallus, are, he thinks, not algæ, but free lichen-gonidia. Thirdly, he maintains that lichens are not evidences of parasitism, because the gonidia are in no way debilitated, diseased, or destroyed by their contact with the hyphæ, but on the contrary derive from it nourishment and growth, and if this view were accepted, there would result, as Th. Fries had already observed, in Lichenographia Scandinavia, p. 8, an incredible double and mutual parasitism of hyphæ upon gonidia and of gonidia upon hyphæ.

In conclusion, Dr. Koerber gives his own views in regard to the anatomy of Lichens. He agrees with Schwendener that the gonidia are not produced from the hyphæ of the thallus, but regards the connection of the two as a simple process of nourishment. To account for the origin of the thallus he supposes that the hyphæ of a germinating spore need, for their perfect development, to come in contact with the form of gonidia belonging to their own species. He asserts that the spores of some Lichens, as in the genus *Sphæromphale*, which has muriform spores, do not produce hyphæ, but gonidia of the kind called microgonidia or leptogonidia; and finally suggests several different methods, according to which, in his opinion, the lichen thallus may be produced by asynthetic gonidia (soredia).

Dr. Krempelhüber, in a notice of this essay in *Flora* for March 11, observes that Koerber's hypothesis has not much better foundation than Schwendener's, with which it has much in common. If the observations in regard to the spores of species of *Sphæromphale* are confirmed, he thinks them against Schwendener; and that, if Koerber's arguments and observations are not conclusive against Schwendener's hypothesis, they tend to render it still more improbable.

On the other hand, in *Flora* for March 21, 1875, Dr. George Winter, in a paper entitled "Zur Anatomie einiger Krustenflechten" (On the Anatomy of some Crustaceous Lichens), disputes the assertion that some lichens are destitute of hyphæ, and gives the result of his investigations of *Secoliga abstrusa*, *Sarcogyne privigna*, *Hymenelia affinis*, and *Natrocymbe fuliginea* (which last he maintains is a Sphæriaceous fungus), and concludes that these Lichens possess undoubted hyphæ, differing in no respect from those of other Ascomycetæ, and that his observations go to confirm Schwendener's theory. His investigations are to be continued. H. W.

16. *Gustave Thuret* died suddenly of angina pectoris, on the 10th of May, at his residence at Antibes, in the South of France. We judge that he had reached the age of about 65 years. He was one of the best investigators of *Algæ*, and the discoverer of their antheridia in the *Fuci*, as well as of their phenomena of fertilization. His earliest papers were published in 1840; the most important ones followed a few years later. His researches were kept up to the last, and much was still expected from him, although he was slow to publish, being extremely conscientious and critical. He ought also to be commemorated for his services to horticulture. The garden which he established upon his grounds in the vicinity of Antibes is most remarkable. Perhaps no public collection has a greater number of species growing in the open air; it is certainly unequalled in *xerophilous* plants,—to use the name recently applied by DeCandolle to those which affect arid regions. A visit to this collection at the proper season, under the genial and accomplished founder's guidance, was a treat long to be remembered. The loss by M. Thuret's unexpected decease is most serious. A. G.

17. *An Inquiry Respecting the Reversion of "Thoroughbred" Domestic Animals;* by WILLIAM H. BREWER of New Haven, Conn. (From the Proceedings of the American Association for the Advancement of Science, Hartford meeting, 1874.)—* * * I need not try to prove (what all will admit) that in our most celebrated breeds, their special excellences are the accumulated improvement of many successive generations, and that this improvement is often accompanied with marked and important changes of form and structure. Now, as this improvement and change are unquestionably the results of man's care, it is often stated that if the care be withdrawn, then the breed will retrace its steps, and go down by the same road that it came up (only vastly faster, the downhill road being the easier travelled), and become again what it sprang from, or to use the more familiar phrases which are supposed to sound more scientific, it "*reverts*" or "*returns to the original type.*" This has been said so often that it is believed, and with a great class of scientific men has become a dogma, and as such is used to sustain certain theories regarding the permanence of "original types," and the fleeting nature of "acquired characters." As a dogma, it finds a place in the lectures of eminent scientific men, and also in our scientific literature, and in the papers read before our learned societies, and from these high authorities it spreads and descends through various channels until at last it is proclaimed by the popular lecturer or finds a lodgment in the "agricultural column" of religious weeklies. * *

At the last meeting of this association, this dogma of reversion was strongly insisted on in one of the papers, and I find on p. B, 404, of the last volume of its "Proceedings," this: "The hog has been greatly changed by domestication, and yet, when left to himself, he soon returns to the original type. During the late war *some of the most improved breeds* were turned loose and left to shift for themselves. *Three years after, I found them possessing all the physical characters of the wild boar of Europe.*" I have *italicized* in the quotation the special point to which I wish to call attention. At the reading of this paper the author verbally stated that a similar fact had been observed with "Durham cattle." About the same time the same dogma was strongly and publicly affirmed by another scientific authority who is even more eminent. In both cases the dogma was used as an argument to sustain a certain scientific hypothesis, or else to combat another hypothesis.

Now let us turn to another meeting, held scarcely a month after our last one. A herd of short-horns was to be sold at New York Mills, and stock-breeders flocked from near and far to attend this other meeting. They came from England, they came from California, they came from half the States of the Union; they even came from the very regions where these scientific authorities had but just told them that short-horns were so prone to return to their original type. There was less talk at that meeting than at the scientific one; the speeches were in fact *very* short, but they were to the point. The principal speaker was the auctioneer. He was doubtless eloquent in his way, but his eloquence alone will hardly explain the result; for in thirty minutes from the time he began, the sales amounted to a quarter of a million of dollars, and in a very short time, those reckless men, unheeding the warnings of their scientific brethren, bought one hundred and nine head of cattle for $382,000, an average of more than $3,500 per head. For the best five cows (belonging to a particularly excellent strain called the "Dutchess") they paid an average of $31,640 per head. *These prices indicate the faith of those men in the future permanence of the acquired characters of that herd.*

For some years I have been looking for the proof of the oft-repeated assertion, that thoroughbred stock of old and well established breeds ever returns to the original type (whatever that may mean), *under any conditions or circumstances.* Thus far the results have been entirely negative, that is, I often hear the general assertion, but I have not yet found the proof in scientific cases. Until within the past year, however, my inquiries have been verbal, as I had opportunity, and I kept no record of the answers received, because I thought that among practical men (so-called) the thing was settled. But the confident assertions within a year, by eminent authorities, that *such reversions would always take place under certain conditions*, and then, following so closely on the heels of these assertions, the remarkable sale of which I have spoken, have awakened a new interest in the subject, and I have resumed the inquiry, in the hope that, if there be any such great law, we may somewhere find the proof in specific examples. * *

I have therefore prepared the circular which follows, and I ask of the members any aid they may be able to give.

I append a copy of the circular, and I propose to give this association the further results of the inquiry at some future meeting.

"*Dear sir:* Will you have the kindness and patience to answer me the following questions:

1st. Have you, personally, ever known any case where *thoroughbred* short-horn cattle, because of climate, poor feed, neglect, or any other cause, have become in character anything else than short-horns—in other words, *where from any cause thoroughbred short-horns have degenerated into animals of any other breed or type?*

2d. Do you personally know of *thoroughbred* animals of any other breeds so changing or "reverting?"

3d. Have you ever heard of such a thing taking place in the experience of other breeders, so well authenticated that you believed it to be a fact?

Now please let me explain why I trespass on your time and good nature with questions that may seem to you to be simply absurd.

I think that the practical breeders of thoroughbred stock (of whatever kind) commonly believe that so long as the breed is kept pure and no other blood mingled, that although the animals may vary greatly in excellence, all of them will have the essential characters which distinguish that breed from all other breeds or "types."

On the other hand, many persons (who are not breeders of thoroughbred stock so far as I know) have asserted that if neglected, any breed will "revert to the original type," or by other words or phrases have carried the idea that, under such or other circumstances, the breed will not only lose its better qualities, but, moreover, that the animals will become of some *other breed or type* which is said to be that of the early and cruder ancestors from which the breed originally sprung, however long ago that may be. * * * * * *

That *grade* animals often "revert," that curious freaks and "sports" often attend violent crossing (and also that breeds *deteriorate* under bad management or bad conditions), are well enough known, but these facts do not effect the specific questions asked where *the blood is supposed to be kept strictly pure.*

18. *Seventh Annual Report on the Noxious, Beneficial, and other Insects of the State of Missouri*, made to the State Board of Agriculture; by Charles V. Riley, State Entomologist.—This is a very sensible report, by one of the best entomologists of the country. It gives a large amount of valuable information about various noxious insects of Missouri, and other parts of the country, and advice as to the best means of checking their ravages. Among the insects treated of, the first noticed is the Colorado potato beetle, which, the Report says, originally fed on the *Solanum rostratum* Dunal, of the Rocky Mountains, but had in 1859 reached a point 100 miles west of Omaha; in 1861 invaded Iowa and Southwest Wisconsin; in 1864, 1865 crossed the Mississippi to Western Illinois; in 1866 occupied the region west of a line from Chicago to St. Louis; in 1867 reached Southwest Michigan and

West Indiana; in 1868 had entered Ohio, and in 1874 arrived on the borders of the Atlantic at many points. Another insect treated of is the Chinch-bug (*Micropus leucopterus* Say), which attacks the grains and grasses, whose ravages among the wheat, and corn, and oats of Missouri in 1874 caused a loss to the State, according to the author, of *nineteen millions of dollars.* A third kind is the Rocky Mountain locust (*Caloptenus spretus* Thomas), which has been recently so destructive; and others are the *Philoxera*, the enemy of the grape vine, the *Chrysobothris femorata*, an apple-tree borer, and *canker worms.* The articles are illustrated by excellent figures, and a map shows the path of the locust invasion of 1874 over Missouri.

III. ASTRONOMY.

1. *The Transit of Venus, Dec.* 8, 1874.—In vol. ix of this Journal, at pages 157 and 234, has been given a synopsis of the principal stations at which successful observations of the late transit of Venus had been reported. As these reports for the Southern hemisphere were quite incomplete. we now present a fuller report from the principal stations of the Southern hemisphere. The stations are arranged in the order of latitude.

1. *Observations of the Transit in the Southern Hemisphere.*

(1.) Rodrigues, lat. 19° 4′, long. 4^h 14^m E. English station. Ingress and egress were well observed, and 441 photographic pictures were obtained.

(2.) Mauritius, lat. 20° 20′, long. 3^h 51^m E. Lord Lindsay's station. All the contacts were observed except the first, and 110 good photographs were taken.

(3.) Reunion, lat. 20° 51′, lat. 3^h 42^m E. Dutch station. Third contact observed, and a few photographs taken.

(4.) New Caledonia, lat. 21°, long. 11^h E. French station. All the contacts were observed except the third, and 100 good photographs were taken.

(5.) Windsor, lat. 33° 36′, long. 10^h 4^m E. English observatory The four contacts all observed.

(6.) Sidney, lat. 33° 51′, long. 10^h 5^m E. English observatory. Weather favorable. The four contacts were all observed, and 180 photographs taken.

(7.) Cape of Good Hope, lat. 33° 56′, long. 1^h 14^m E. English observatory. Both contacts at egress observed, and photographs taken.

(8.) Adelaide, lat. 34° 40′, long. 9^h 15^m E. English observatory. Last two contacts were well observed.

(9.) Melbourne, lat. 37° 49′, long. 9^h 40^m E. English observatory. All the contacts were observed except the first external, and 200 photographs were taken.

(10.) St. Paul Island, lat. 38° 43′, long. 5^h 10^m E. French station. Both the internal contacts were observed, and numerous photographs were obtained.

(11.) Campbelltown, Tasmania, lat. 41° 55′, long. 9^h 50^m E. American station. The second internal contact observed, and fifty-five good photographs were taken.

(12.) Hobart-town, Tasmania, lat. 43° 0′, long. 9^h 49^m E. American station. Thirty-nine good photographs were obtained, but no contacts were observed.

(13.) Christ Church, New Zealand, lat. 43° 30′, long. 11^h 31^m E. English station. Observations failed from clouds.

(14.) Chatham Island, lat. 43° 48′, long. 12^h 12^m E. American station. No contacts observed. Eight good photographs taken.

(15.) Queenstown, New Zealand, lat. 45° 0′, long. 11^h 15^m E. American station. First and second external contacts observed. Fifty-nine good photographs taken.

(16.) Kerguelen Islands, lat. 49° 54′, long. 5^h 41^m E. English, German, and American station. The Germans got both contacts at ingress and egress. The English and Americans observed the ingress, but not the egress. Twenty-six good photographs were taken by the American party.

(17.) Auckland Islands, lat. 50° 48′, long. 11^h 7^m E. German station. The sun was obscured till ten minutes after the beginning of the transit. From that time, the contacts were observed, and heliometer observations and photographs were obtained.

(18.) Campbell Island, lat. 52° 33′, long. 11^h 17^m E. French station. Venus seen before ingress only. No contacts observed.

2. *First results from the Transit.*

M. Puiseux has given the first French results for the sun's parallax, using the observations of Pekin and St. Paul, all made with object glasses of 216 millimeters (8½ English inches). The parallax is 8″·879. E. L.

2. *On the Solar structure;* by Father Secchi.—Father Secchi has made a very full reply to the article by Professor Langley, "on the comparison of theories of solar structure with observation," published in the March number of this Journal, first by notes upon the communication as it appeared in the *Memorie degli Spectroscopisti* of Palermo, and in a lecture delivered to the Tiberine Academy, which was published in the *Voce della Verita*, which has been in part reproduced by him in a subsequent number of the *Memorie*, with additional remarks.

Father Secchi accepts, without reserve, the statements of the article in question so far as they embody facts of observation, and remarks, of the illustration, that it is the very truth, rendered in minute detail. As to the interpretation of fact, there is, however, he observes, more than one point of view, and this leads him to an exposition of his own. His views, as now presented, differ from those he was understood to hold till lately, and in their present form are of such interest that their outlines are here reproduced, as nearly in his own terms as is possible in such an abridgement.

Spots are due to eruptions of metallic vapors from the interior of the sun.—Three phases in the life of a spot may be distinguished: (1) That of its formation. (2) That of a subsequent period of relative calm. (3.) That of its closure and extinction. In the first the metallic eruptions attain their maximum violence; vortical motion on an enormous scale is visible both in the photosphere and in the chromosphere; in the former (to borrow an architectural term) *in plan*, in the latter *in elevation*. These vortical motions are a consequence, not a cause—products of the eruption, admittedly associated with the spot, but not generators of it. (It is this period which Mr. Langley has taken as the subject of his article, as shown by a history of the spots' appearance in the clear sky of Rome, in the days preceding and following those employed in its study at Allegheny.)

In the second phase the spot commonly presents a crater-like formation, an obscure central mass being invaded by convergent gaseous streams, which form the luminous bridges and filaments. The eruption now is comparatively gentle, or has nearly ceased, though new matter, still extruded, checks the invasion of the surrounding photosphere, and mingling with its insetting currents, causes the radiate appearance of the penumbra. This phase may be compared to that period in the eruption of the terrestrial volcano when the lava, which has risen to the tip of the crater, is being discharged without tumultuous outbursts, or the formation of the *pine* of smoke.

In the third phase, both of the terrestrial and solar volcano, the emission ceases and all is over.

We may now understand how it is that though every great eruption generates a spot, every spot is not seen accompanied by a chromospheric eruption. To the second and third epochs alone belong that condition of the nucleus, as a deposit of more or less stagnant matter, which Mr. Langley mistakingly assumes Father Secchi to suppose it to be in, generally. M. Faye and he have attacked Father Secchi from opposite sides, not perceiving that the latter's point of view is double—as it is. More than this (and however such critics may exclaim against the apparent contradiction), he affirms the possibility of a third thing, a sequence to Galileo's and Kirchoff's views, namely, that a spot may be generated from a cloud of metallic vapors above the photosphere, or resting on and partly immersed in it; it being by no means certain that the spot is always a cavity. Let us not impose our own artificial limits on greatly varied Nature!

P. Secchi concludes that Mr. Langley's observations, though undoubtedly exact and reliable, give little which he has not himself, in considerable part, anticipated, though they may be admitted to have carried the resolution of photospheric detail farther in some respects than he has been able to do, with the smaller aperture of the Roman instrument.

Mr. Langley's impression that the observations he brings do not accord well with Father Secchi's views, is altogether owing to misapprehension of the true nature of these; on the contrary, the article and illustration bring gratifying and remarkable confirmation of Father Secchi's own observations, and on points where they have been called in question.

On the whole, then, while he must insist that longer study will modify the American observer's interpretation of the facts, he would rather be understood as admiring the Allegheny observations than as disputing them. x.

3. *Observations on Magnetic Declination made at the Trevandrum and Augustia Observatories.* Vol. I. Discussed and edited by John Allen Broun. London, 1874.—This volume is the first of the official publications of the extensive series of observations made since 1836, at Trevandrum, under the successive superintendencies of Mr. J. Caldecott and Mr. J. A. Broun. In it the author gives us the first installment of a series of volumes of magnetic observations made at the auxiliary observatory at Augustia, and the main establishment at Trevandrum during the years 1852 to 1869. In explanation of the latter date, it may be stated that although Mr. Broun departed from Trevandrum in April, 1865, yet he was so fortunate as to obtain the continuation for six years of the series of observations made by his two best assistants. He has himself, for the past ten years, therefore, resided in London, and superintended the discussion and editing of the series of volumes which are promised to the scientific world. In the present volume we have a *resumé* of most of the publications made by him during the past 30 years in the transactions of various scientific societies, which appear here as contributions to terrestrial magnetism from the Trevandrum Observatory. Pages 1 to 80 consist especially of descriptions of instruments, methods of observation and reduction, and narratives of the various explorations made into the neighboring country. Pages 80 to 184 give us the results of magnetic observations made with the improved instruments introduced by Mr. Broun, giving in fact a full, and often a new, discussion thereof, that adds materially to our knowledge of questions relating to the variations of magnetic declination. The chapter relating to the 26-day period, co-extensive with the rotation of the sun on its axis, is interesting, inasmuch as the author states that he has evidence, to be given at a future time, of a double oscillation of this period. Pages 192 to 448 give the individual hourly observations for every day of the year, with the declinometers established at Trevandrum, Augustia, Shertally, and Cape Comorin. Pages 453 to 570 are occupied with appendices, principally the annual reports of the director in reference to the observatories, the almanac, the public museum and other public duties which were imposed upon him.

The observatory at Trevandrum occupies the summit of a small hill, which is from 60 to 70 feet above the sea and within four miles of the shore. The observatory at Augustia was intended to be built at as great a height as possible, and at the same time as near as possible to the Trevandrum Observatory. Its altitude is accordingly found to be about 6,200 feet, and it is situated about 10 miles from the main station. The stations at Shertally and

Cape Comorin were occupied for a few months only in 1858 and 1859.

The appendices containing Mr. Broun's annual reports are very interesting reading, if for no other reason than as showing the tenacity with which he clung to his determination to carry out a series of most exact scientific observations in a region known only to a few savages and inhabited by ferocious animals. Every page gives us illustrations of the difficulties that he had to encounter both from the climate and the native assistants, and makes us wonder that he has accomplished so much. He certainly would not have been able to have accomplished what he has done had not his previous experience at the Makerstoun Observatory stood him in good stead. C. A.

4. *Determination of weights to be given to observations for determining time with portable transit instruments, recorded by the chronographic method;* by CHARLES A. SCHOTT. Appendix No. 12, U. S. Coast Survey Report for 1872.—In this little pamphlet Mr. Schott collects together the results of the general experience of the telegraph longitude parties of the Coast Survey for a number of years. He states that the introduction of the chronographer registration of transit of stars has considerably increased their precision, rendering it desirable to discuss the relative weights of the equations of condition. In the Coast Survey practice, two classes of portable instruments are employed for longitude purposes, the largest size having reticules of 25 threads, the smaller size having glass diaphragms of 15 lines. It appears from the probable errors deduced in Mr. Schott's paper, that for the larger instruments the gain in accuracy, between 17 and 25 threads, is very trifling, and probably more than counterbalanced by the increased fatigue of the observer, and consequent change in his personal errors; a diaphragm of 17 threads is recommended, which may be conveniently disposed in three tallies. The probable error of a transit over a single thread is found to be

for large portable transits $\pm\sqrt{(60{\cdot}063)^2+(0{\cdot}036)^2\tan^2\delta}.$
for small " " $\pm\sqrt{(0{\cdot}080)^2+(0{\cdot}063)^2\tan^2\delta}.$

In the reduction of the observations made during any one night for longitude purposes, it is recommended not to assume a constant azimuthal error, but to determine it before and after each reversal of the instrument. In general, the collimation, azimuth and clock correction should be determined independently, before and after each reversal. The criterion for the correctness of the entire work is found in the equality of the clock corrections as referred to the same moment of time. C. A.

5. *On the Meteorite of Lancé;* by DR. K. VON DRASCHE; Tschermak's Mineralogische Mittheilungen, 1875.—This meteorite fell near Lancé in July, 1872; it belongs to Rose's group called Chondrites. Dr. Drasche has given the results of a minute and careful microscopic examination.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *On the Height of the St. Louis Directrix;* by G. C. BROADHEAD. (From a letter to one of the Editors.)—I find that Mr. J. T. Gardner, in his determination of the height of the St. Louis directrix,* has omitted all reference to the careful observations of Dr. Engelmann, as given in vol. i, No. 4 of the Transactions of the Academy of Science of St. Louis (pp. 663–667), and quoted in the American Journal of Science, vol. xxx, p. 394 (1860). Humphreys and Abbot, in their work, bear testimony to the correctness of the observations of Dr. Engelmann. Dr. Engelmann and Mr. E. H. Barton, of New Orleans, carefully adjusted two barometers, compared them together, and then observed them for several months, one party being in St. Louis and the other in New Orleans. The result proved the elevation of the St. Louis directrix to be 404·9 feet above the Gulf. Mr. Gardner makes it 428·29 feet, a difference of 23·39 feet. Mr. Gardner's results are chiefly or altogether based on railroad levels. Now, if these railroad levels were run continuously from the Gulf up, they might be correct. But I have had much to do with getting railroad profiles, and have found that few of them can be relied on throughout. I have two profiles of one railroad 300 miles long in Missouri, and they widely differ; and one of these, the Missouri Pacific, claims to be correct. The profile notes of the Missouri, Kansas and Texas Railroad, from Texas to Sedalia, makes out a different elevation above the sea at Sedalia from those of the Missouri Pacific Railroad. There is not a railroad line in Missouri north of the Missouri River whose elevations can be relied on throughout. One railroad, of 150 miles long, has three or four breaks of elevations on it, each beginning at an assumed datum.

2. *Swedish Arctic Explorations.*—The Swedish Arctic Expedition to Novaya Zemlya, which will start at the beginning of next month from Tromsöe, will be occupied first with botanical, geological, and ethnological inquiries in the southern part of Novaya Zemlya, and then advance along the west coast to the northern point, which it expects to reach about the middle of August. Thence it will go to the northeast to explore this still quite unknown part of the Polar Sea, and then southward to the mouths of the Obi and the Jenisei, where the country is geologically very interesting. If the ice creates no obstacles, Prof. Nordenskiöld will here quit the vessel, and go in a boat up the river, to return home afterward by land.—*Nature*, May 20.

3. *Norwegian Exploration.*—The Norwegian Government has granted a credit of £4,000 for an expedition to be sent out next year under the scientific direction of Dr. Mohn, for the exploration of the sea between Iceland, the Faroe Islands, Spitzbergen, and Jan Mayen. The commander of this expedition will be Capt. Carr Wile, of the Royal Norwegian Navy, who is now in England gathering information as to the work done by the *Challenger*.—*Nature*, June 10.

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* See this Journal, III, vol. ix, p. 309.

4. *British Arctic Expedition.*—The ships Alert and Discovery, of the British Arctic Expedition, sailed from Portsmouth on the afternoon of the 29th of May. The Valorous accompanies them to Disco.

5. *American Association for the Advancement of Science.*—The next meeting of the American Association will be held at Detroit, Michigan, in August, commencing on Wednesday, the 11th, at 10 o'clock, A. M. The headquarters of the association will be at the Russell House on Monday and Tuesday, and afterward at the City Hall and Court House, directly opposite. That the circular of the Local Committee may be received without fail by those intending to be present, word should be sent by each to Frederick Woolfenden, Esq., Detroit, Secretary of the committee. It is expected that the Chemical Sub-section, instituted at the last meeting, will be largely represented; and also, that the departments of Ethnology and Archæology will have a prominent sub-section established, to be called the sub-section of Anthropology. The president of the year is Mr. J. E. Hilgard, of the Coast Survey; the vice-presidents, Prof. H. A. Newton of New Haven for Section A, and Dr. J. W. Dawson of Montreal for Section B. Prof. S. W. Johnson of New Haven is chairman of the Chemical Sub-section.

The new volume of the Proceedings of the Association—that for 1874—has been issued. The volumes of the Proceedings, now twenty-three in number, can be obtained from the Permanent Secretary, F. W. Putnam, Salem, Mass., at the price of $1.50 a volume; or, if ten or more volumes are ordered, for $1.00 a volume.

6. *Milligrade Thermometric Scale.*—Mr. J. Williams, in a paper before the Chemical Society of London, proposes to substitute the freezing and boiling point of mercury for those of water, and to divide the scale into a thousand parts.

7. *Dr. Horace Wells.*—A bronze statue to Dr. Horace Wells of Hartford, Connecticut, "the discoverer of Anæsthesia," who died nearly a quarter of a century since, will soon be erected in Hartford. The statue is by the sculptor, Truman H. Bartlett. The State of Connecticut appropriated five thousand dollars toward the monument, and the city of Hartford an equal amount. The expenses of the pedestal of the statue, which should also be of bronze, are not met by these appropriations, and funds are solicited of the public by the Committee of the Hartford Medical Society, of whom Dr. E. H. Hunt of Hartford is chairman, and Dr. G. W. Russell of the same city treasurer. Dr. Wells was the world's benefactor.

8. *Report of the Superintendent of the Coast Survey for the year* 1871. 220 pp. 4to, with many maps.—Besides the general report on the progress of the survey, this volume contains a paper by G. Davidson and C. A. Schott, Assistants, on a comparison of the methods of determining heights by means of leveling, vertical angle and barometric measures from observations in California; reports by Assistants G. W. Dean and C. H. F. Peters, of obser-

vations of the total solar eclipse of Dec. 22, 1870; a report by C. A. Schott on the adaptation of triangulations to the various conditions of configuration and character of the surface of country and other causes; description, by J. Homer Lane, of a new form of mercurial horizon, in which vibrations are speedily extinguished. There is also a *General Index of professional and scientific papers contained in the Coast Survey Reports from* 1851 to 1870. The volume contains 36 large charts, and among these, under the head of River and Harbor Charts, there are charts of Plattsburgh, Burlington, New Haven, New York Bay and Harbor, Nos. 1 and 2, Delaware River from Navy Yard to Fort Mifflin Light-house, Neuse River, Passes of the Mississippi, Puget Sound.

9. *The Life and Growth of Language: an Outline of Linguistic Science;* by W. D. WHITNEY, Professor of Sanskrit and Comparative Philology in Yale College. 326 pp. 12mo. New York, 1875. (D. Appleton & Co.)—Prof. Whitney writes with the clearness, depth of insight and comprehensiveness of one who has full command of his subject in all its bearings and details. His word is an admirable exposition in a brief form of the science, growth, and, as far as can be reasoned out from known facts, the origin, of language.

10. *The Recent Origin of Man, illustrated by Geology and the Modern Science of prehistoric Archæology;* by JAMES C. SOUTHALL. 606 pp. 8vo, with many illustrations. Philadelphia, 1875. (J. B. Lippincott & Co.)—The author of this work has gathered into it a large amount of information on prehistoric man, and on the valley-formations, cave deposits, and other repositories of human relics. The attempts which have been made to measure time by means of the thickness of valley-formations, peat-beds and stalagmitic deposits are discussed, and sources of error pointed out; and the conclusion reached is that indicated in the title of the work. The facts are given with much detail and are illustrated by many figures. Unfortunately the author lacks in scientific knowledge, and therefore in ability to handle well the geological questions he deals with, and steer clear of his prejudices. His chapter on the fickleness of science shows great misapprehension of the subject. In fact, the whole work exhibits a temper toward scientific writers, and a consequent misunderstanding of men and opinions, which is calculated to repel any honest searcher for truth who is not already of the author's mind.

11. *Note sur les Tremblements de Terre en* 1871, *avec Suppléments pour les années antérieures, de* 1843 *a* 1871; par M. ALEXIS PERREY, Professeur Honoraire a la Faculté des Sciences de Dijon. pp. 145. Bruxelles, 1875.—This constitutes Professor Perrey's twenty-ninth annual report presented to the Royal Academy of Belgium and is published in vol. xxiv of their *Memoires.* The first seventy-two pages are occupied with the supplementary report for the preceding years. In the remainder, constituting the report for 1871, the most noteworthy items are: Earthquakes in England, March 17; in Chili, March 25; in the Philippine Islands, May 1; and the eruption of Mauna Loa in August.

Etude du réseau pentagonal dans l'océan Pacifique, pp. 4, and *Sur les volcans de l'île de Java, et leurs rapports avec le réseau pentagonal*, pp. 3; par M. Alexis Perrey.—Two interesting and valuable papers presented to the Academie des Sciences at Paris on August 17 and November 9, 1874. C. G. R.

12. *Storms: their Nature, Classification and Laws, with the means of predicting them by their embodiments, the Clouds;* by Wm. Blasius, formerly Professor of the Natural Sciences in the Lyceum of Hanover. 342 pp. 8vo. Philadelphia, 1875. (Porter and Coates.)—The author observes in his Preface: "I am convinced that the existing theories of the nature and laws of changes of weather are intrinsically erroneous, and that at least a much nearer approximation to the truth will be found in this volume."

13. *Climate and Time in their Geological Relations: a theory of Secular Changes of the Earth's Climate;* by James Croll, Geological Survey of Scotland. 578 pp. 8vo, with many illustrations. London. 1875. (Daldy, Isbister & Co.)—This work is a learned controversial discussion of some of the most difficult questions in geology and terrestrial physics, viz: the source of oceanic currents; the origin of changes of climate and the glacial epochs in geological history; the probable age and origin of the sun; a method of determining the mean thickness of the earth's sedimentary rocks; cause of the changing water-level in the earlier half of the Quaternary; the effect on climate of a change in the obliquity of the ecliptic; theories of glacier-motion and explanations of many glacial phenomena; nature of heat vibrations; the cause of regelation, and many other related topics. It is illustrated by several plates.

14. *English Men of Science, their Nature and Nurture;* by Francis Galton, F.R.S., author of Hereditary Genius, &c. 206 pp. 12mo. London, 1875. (New York, D. Appleton & Co.)—This work, already well known, is a valuable contribution to the subject of heredity, as well as to that of the history of science.

15. *The Aerial World. A popular account of the phenomena and life of the Atmosphere;* by G. Hartwig, M. and P.D. 556 pp. with numerous illustrations. New York, 1875. (D. Appleton & Co.)—A popular and handsome work, full of interesting matter and for the most part good in its science.

16. *French Academy of Sciences.*—General Sabine has been elected a corresponding member of the Academy of Sciences.

OBITUARY.

John Edward Gray.—This veteran naturalist died on the 7th of April, at the residence in the British Museum which he has long occupied, and which, having recently retired from the keepership, he was about to vacate. Naturalists from all parts of the world have pleasant memories of the liberal but unostentatious hospitality there dispensed by Dr. Gray and his surviving consort, and will miss him at the Museum, which he has served most assiduously and ably for more than half a century. He was

Dr. Leach's assistant almost in boyhood, was officially connected with the Museum in 1824, when Mr. Children became keeper of the Natural History collections, and succeeded to the keepership of the Zoological collections in 1840, upon Mr. Children's retirement. The whole development of this noble collection has therefore taken place under Dr. Gray's superintendence. To it he brought a persistent ardor, indomitable energies, and great practical powers; and he worked unflaggingly down nearly to the close of the year 1869, when he was suddenly prostrated by paralysis. Even then his mental activity was hardly or briefly interrupted, although he lost the use of his right side, and his scientific contributions have continued to appear down even to the beginning of the present year. As some evidence of his activity, it may be mentioned that his contributions to scientific journals and transactions alone—as indexed in the Royal Society's Catalogue, had reached 500 in number in 1863, and his independent publications, before and since that date, are far from few. He published also upon penny postage and postage stamps, decimal coinage, prison discipline, sanitary reforms, the arrangement and utilization of museums, and many other topics. These were the bye-plays or recreations of a busy life; but he was pre-eminently a zoologist,—a typical zoologist of "the old Linnean school," and the last of the race. The biographical notices in the various English journals, most of them familiar to our readers, give good general views of the amount and character of his zoological work. He was also a botanist, and it was in this field that he entered upon his scientific career. In this department he was not of the Linnean school. For he wrote "the systematic part of the 'Natural Arrangement of British Plants,' the work that first introduced the natural system of plants to the student of English history." Although published in the name of his father, Samuel Frederic Gray, the son reclaimed it as his own, stating that he was responsible for all but the introductory portion. It was a very elaborate work for a young man, and several hands were probably engaged in it. But it failed of success, partly because it was not very well thought of by the leading botanists of Jussiæan school, and partly on account of a pitiful personal opposition on the part of the Linnæans, who subjected the young reformer "to something very like persecution." If Dr. Gray was "overgiven to controversy," perhaps this early ill-treatment may have fostered the disposition. When the Royal Botanical Society of London, which established the exhibitions and conservatories in Hyde Park, was founded, Dr. Gray accepted the presidency. In 1864 he published a neat little Handbook of British Water-weeds or Algæ. In 1865 he caused to be printed—as a matter of historical interest—a portion of Salisbury's manuscript "Genera of Plants" (pp. 143, 8vo, Van Voorst), which, with other MSS. and papers, was made over to him by Mr. Burchell's executor, after the death of the latter in 1864. Salisbury had bequeathed all his papers to Burchell, who made no use of them. Dr. Gray's interest in reclaiming them, and in arranging for their preservation in the British Museum, grew out of memory of assistance rendered by Salisbury, who lent him these manuscripts when he was preparing the "Natural Arrangement of British Plants," and of former propositions of Salisbury, that he should devote himself to botany and edit any unpublished works that the latter might leave in manuscript. Dr. Gray declined the proposition, and with it tempting pecuniary offers. But, after forty years interval, he printed a "fragment of the Genera Plantarum, exactly as it was left by the author, for the purpose of showing the kind of work that he intended to produce."

Dr. Gray married, in 1826, the widow of his cousin, Francis Edward Gray, "a fitting helpmate to share and encourage him in all his undertakings," an excellent algologist and conchologist, a lady of remarkable powers and graces. A. G.

ADMIRAL SHERARD OSBORN died on the 6th of May, in his 54th year. He entered the Royal navy in 1837, became lieutenant in 1846, and three years afterward was selected as a volunteer for the Arctic expedition sent in search of Sir John Franklin: again, in 1852, he took command of the Pioneer on a second extended Arctic expedition, having the same purpose in view. He was a member of the committee on equipments of the expedition which has just sailed.

SAMUEL HEINRICH SCHWABE, the astronomer, died on the 11th of April, in his 86th year. To Schwabe's observations on the sun's spots science owes the discovery of their periodicity, and that the length of the period was ten to eleven years.

A. G. FINDLAY, the geographer, and member of the Council of the Geographical Society of London, died at Dover on the 3d of May, in his 64th year.

M. DESHAYES, Professor in the Paris Museum of Natural History, died on the 9th of June.

JOSEPH WINLOCK, Director of the Observatory of Harvard College, died suddenly on the 11th of June.

Sir WILLIAM LOGAN, long at the head of the Geological Survey of Canada, died at Ontario, Canada, on the 28th of June.

Memoirs of the Geological Survey of India. Palæontologiæ Indicæ. Fauna of the Indian Fluviatile Deposits, vol. i, ser. x; I, Rhinoceros Deccanensis, by R. B. FOOTE, 18 pp., fol, with three plates.—Also vol. x, part 2, and vol. xi, pt. 1 of the Memoirs, and vol. vii, Parts 1 to 4 of the "Records" of the Survey.

Practical Hints on the selection and use of the Microscope, by John Phin. 131 pp. 12mo. 1875. New York. (The Industrial Publication Co.)—A small meager work, very inadequately illustrated.

A Brief Essay on Heat, Light, Electricity and Magnetism, by Charles Skelton, M.D. 75 pp. 8vo. Trenton, New Jersey, 1875.—The author, objecting to the present theories, makes light the rapid wave motion of imponderable matter; heat, this matter in excess in ponderable matter; electricity and magnetism, this imponderable matter in flowing currents.

Die Idee der Entwickelung; eine sozial philosophische Darstellung; von Leopold Jacoby. Erster Theil, 134 pp. 8vo. Berlin, 1874. (H. E. Oliven.)

What Young People should Know. The Reproductive function in Man and the lower animals; by Burt G. Wilder. 212 pp. 12mo, with 26 illustrations. Boston, 1875. (Estes and Lauriat.)

THE

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[THIRD SERIES.]

ART. XIV.—*Historical Note on the Observation of the Corona and Red Prominences of the Sun;* by EDWARD S. HOLDEN.

So much interest attaches to the phenomena of the corona and red prominences, as observed during total solar eclipses, and correct views of their nature and of the proper means of observing them are so recent, that I feel it proper to give here a brief account of what I believe to be the first attempt to see these, under ordinary conditions, with an uneclipsed sun.* This account is contained in the private diary of the late G. P. Bond, formerly Director of the Observatory of Harvard College, which has become known to me through the kindness of his daughters.

Bond observed the total solar eclipse of July 28th, 1851, at Lilla Edet in Sweden, and his report is published in the Memoirs of the Royal Astronomical Society, vol. xxi, page 97.

From Sweden, Bond went to Geneva, where he arrived in September, 1851, and from this point I may transcribe from his diary, making no changes except the occasional insertion or omission of unimportant words.

"Geneva, Sunday, Sept. 14, 1851.

I think I must go to Chamounix to try whether it may be possible to discern the red flames on the sun's disk by occulting all but the very edge, upon one of the lofty peaks. It seems to me not altogether impossible. Certainly an experiment worth trying and a new application of the '*Aiguilles.*'

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* Airy, Nasmyth, Baden-Powell, Piazzi-Smith and others experimented in this direction, about this time, with various results. See Edinburgh Astr. Obs., vol. xi, p. 279; Mem. R. A. S., vol. xvi, p. 301, etc.

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Geneva, Sept. 15, 1851.

* * * The weather looks dark and lowering with an uncomfortable northeast wind, but M. Plantamour thinks it is likely to be fine weather, and on this recommendation I took a place in the diligence for Chamounix. * * * *

Chamounix, Sept. 18, 1851.

Last evening the stars were shining through the opening clouds, giving promise of improving weather, but a glance out of the window, this morning, dispels all such anticipations.

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Chamounix, Sept. 19, 1851.

I woke this morning at five and my first impulse was to go to the window to see the signs of the weather. Last night I had hopes of an improvement. But I was surprised to find a clear sky; some clouds were resting round the '*aiguille,*' but the summit of Mt. Blanc was clear. Started for Montanvert at 7.15 with a guide. * * * * * * * *

Mer de Glace.

* * * Attempted two or three times to hide the sun's disk by projecting rocks to try to see the red prominences, but could not get a station far enough off. * * * *

Chamounix, Sept. 20, 1851.

* * * Snowing fast in morning. Weather desperately bad. But before going to bed it was quite clear. * * *

Chamounix, Sept. 21, 1851.

* * * The fine prospects of last night were effectually put aside by another snow storm. * * * * * *

Chamounix, Sept. 22, 1851.

The morning bad as usual. * * * * * *

Chamounix, Sept. 23, 1851.

This morning still cloudy, yet the prospect for an improvement was encouraging. Soon after breakfast the sun appeared struggling in the clouds, and I hurried off with a spy-glass not to lose the slightest chance of seeing the phenomena I wished to; * * * I spent two or three hours in the wet fields to no purpose. In the afternoon there was an effort at clearing again.

Chamounix to Martigny, Sept. 24, 1851.

The clouds this morning still hung on the mountains, but overhead there seemed some signs of clear sky. To make sure of losing no chance I took an early breakfast and left for the fields with the ordinary spy-glass belonging to the hotel under my arm. Sometimes it would be almost clear, and then again it began to rain, and I was undecided whether to give up and start for Martigny or to stay another day. At last I saw the sun's disk and took up my station on the edge of the shadow of the '*Aiguille de Blettière.*' It was still cloudy, but I was satisfied from the nature of the experiment:—

1st. That a very clear air is necessary.

2d. Plenty of time to choose projections, affording views of as large a portion of the circumference of the disk as possible while the rest is hidden.

And lastly, a good achromatic telescope easily moved.

I did not expect to find it so easy an experiment, nor to find a mass so well-fitted for the purpose as the '*Aiguille de Blettière*' which has a smooth edge, inclined, so as to allow the sun to disappear slowly behind it.

The naked eye easily bears a small portion of the sunlight. From 7 to 9¼ I followed the shadow over the valley. It was nearly clear for a few moments before it reached the woods on the side of the mountain, but there were still some light clouds over the sun and nothing could be seen certainly of the corona; the clouds and mist would account for what I did see, and on the other hand, the color of the telescope supplied too much red just at the edge for one to be able to see any of the red flames, if they existed there.

On the whole, I am more than ever sure that the experiment can be made, and I think will be by some one more fortunate than I."

ART. XV.—*Walker's Statistical Atlas of the United States.**

A CENSUS report consists essentially of statistical tables. In these, certain results are stated numerically, and, like other tables of abstract numbers, the generalizations, which often constitute the most important features of their value, can be seen only after long study and calculation, much of which must be performed by each individual inquirer. Moreover, but few persons have the peculiar mathematical and intellectual training necessary to perceive other than the most obvious generalizations when given in that form of detail. Consequently, in the illustration of facts where numerical tables are involved, much study has been expended in devising other methods of presenting the facts to the mind. In various branches of science, great gain has resulted by representing quantities in other ways than by series of numerals, especially by the use of comparative areas, geometrical figures, curves, shades and colors.

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* Statistical Atlas of the United States, based on the results of the Ninth Census, 1870, with contributions from many eminent men of science and several departments of the Government Compiled, under authority of Congress, by Francis A. Walker, M.A., Superintendent of the 9th Census, Professor of Political Economy and History, Sheffield Scientific School of Yale College. Julius Bien, Lith. 1874.

Until our last census, the results have been published in the numerical form only. In these reports, the Superintendent introduced a few plates by which certain generalizations were shown by comparative shadings or geographical maps. Their general value was readily seen and they were received with great favor. In fact, they made the desirability of more maps devoted to further illustration so obvious that Congress directed Professor Walker, the late Superintendent of the census, who had prepared them, to prepare a "Statistical Atlas." Several portions of this work have already been announced in this Journal as the work progressed, and the complete result is now at hand in a large folio Atlas, consisting of 60 plates and about the same number of letter-press pages.

On the plates, all the methods named for graphic illustration have been used. The compiler divides them into two classes, the Geographical and the Geometrical. The former embraces 65 maps, variously shaded, colored or lined, filling 38 sheets. Six of these maps are double-page size (22×30 inches), 25 of full single-page, and 34 of smaller size.

Sixteen full-page plates are occupied by the geometrical illustrations, on which a variety of devices are used and with most excellent effect. Sometimes by comparative areas and colors in squares and rectangles, sometimes by polygons of ingenious but simple construction, sometimes by circles and circular areas, and in a few cases by lineal curves, the whole disposed in 1208 figures.

The letter press is of the same folio size, and besides the title, index and explanatory preface, consists of eleven "memoirs and discussions" of two to nine pages each, prepared by nine authors and mostly upon subjects treated in the plates. They contain also a few cuts.

It is not too much to say that this is one of the most instructive publications ever issued by our government, and yet it is impossible to satisfactorily treat it in any written article. Precisely as a painting cannot be described in words, so these sixty plates cannot be. Any such description will bear about the same relation to the original that a written description of a person (on his passport, for instance) does to his photographic portrait. Sometimes these plates show an arrangement of facts in such a way that their significance may be easily seen by many, whereas if the same facts were merely stated in the usual tabular form they would be understood by but few. Sometimes they exhibit facts that absolutely refuse to be shown by mere numerical tables. In short, as already suggested, the very reason of their being is because words and numbers cannot or will not tell the whole truth.

The principles involved in the construction of the various illustrations are fully discussed in the text or on the plates. Suffice it here to say that ten of the maps are prepared from data not derived from census returns, but which are of special interest in such a work. The shadings of 55 maps and the construction of more than 1,200 figures are based on computations from census data. Some of these computations were very elaborate; in other cases, where simple, the actual labor necessary was very large. For instance, in the preparation of crop maps it is not easy to show at the same time "the importance of the crop to the county, and of the county to the crop of the country." Now the rule adopted, arbitrary to be sure, but perhaps the best yet devised, is as follows: "The number of bushels, bales, tons, or pounds (the unit of quantity appropriate to the special crop) produced in each county is divided, first by the number of inhabitants, and second by the number of acres of improved land in the county; the two quotients thus obtained are multiplied together, and the square root of the product is taken as the measure of the productive power, in respect to that crop, of the county." When we consider that the county is the nominal geographical unit used (this even being subdivided where accuracy of detail seemed to require it), and that there were about 2200 organized counties when the census was taken, we get some idea, vague though it must be, of the vast labor of computation expended in the production of the entire work. It is probable that over 200,000 computations were performed.

The work is divided into three parts. The first, relating to the "Physical Features of the United States," contains ten maps, five of which are double page. The map of the River Systems (by Gen. von Steinwehr) is the best yet published to illustrate the drainage areas. It has, moreover, combined with this, all the more important general facts of population and production as related to those areas. One map is of the "Distribution of Woodlands;" one is "Hypsometric;" two are Geological; and five relate to Climate, three of these by the Signal Service Bureau and two by the Smithsonian. The relations of these maps to each other are particularly instructive. For instance, the relation between woodlands and rainfall and other climatic conditions has of late been the subject of much dogmatic theorizing. A comparison of these maps shows that the forests of Washington Territory (perhaps the heaviest in the world), are in regions having an annual rainfall of 60 inches and upward. The magnificent forests found from Minnesota to Maine are in regions of 28 to 40 inches, a rainfall precisely identical with that of the nearly treeless prairies which extend westward from Chicago. The northern part of the Michigan peninsula, with its heavy timber, is marked with precisely the same rainfall as large portions of southern Minnesota lying in the same latitude, and nearly treeless. Interrupted prairies extend across Mississippi and Alabama, where we have the heaviest rainfall east of the Sierra Nevada.

Again, compare the woodland map with that of "storm centers" and prevailing winds. The regions of numerous storm centers in northern Michigan, New York and Maine, are heavily timbered; the similar region of eastern Nebraska is almost treeless. A comparison of the regions of more or less winds show similarly diversified facts. While there is undoubtedly a very intimate relation between forests and rainfall when the latter is less than 25 inches annually, where the quantity is above that, certain relations which are zealously claimed to exist, are shown by these maps either to not exist at all, or else that the relative dependence has been vastly overstated. The map of the Coal measures is especially impressive, and we hope to see the excellent map of "Geological formations" struck off in a separate edition for the use of students and travelers.

The five memoirs of this part are by as many authors. On the one hand, they are all too comprehensive to make satisfactory extracts from, and at the same time too condensed to epitomize. The one on "The Physical Features of the United States" (by Prof. J. D. Whitney) is a model of comprehensive condensation. Further notice of this physical part is reserved for another number.

Part 2d, devoted to "Social and Industrial Statistics," contains forty-nine maps, four plates of illustration by geometrical methods, and four memoirs. Two of the memoirs and one map relate to the acquisition, distribution and political division of our territory. Eleven maps show the density and distribution of the population at various periods of our history. One plate and twenty-two maps illustrate the elements of population, nine maps relate to agriculture, six maps and three plates to the distribution of wealth, public indebtedness, taxation, illiteracy, occupation, and church accommodation.

The memoir by Prof. Walker, on the "Progress of the Nation," is the completest in the volume, and is specially illustrated by thirteen maps, eleven of which illustrate density and distribution of population. In his discussion of the subject, a region assumes the dignity of a *settlement* when the census-taker can find a population of two per square mile. The white population of a region more sparsely inhabited than that are scarcely permanent enough to be called settlers. He traces the line surrounding such areas, and also of each of the adopted degrees of density. The following table gives some of the more simple features of this calculation.

Date.	Total area of settlement, in square miles.	Population.	Average density of settlement. Persons to a sq. mile.
1790	239,935	3,929,214	16·4
1800	305,708	5,308,483	17·3
1810	407,945	7,239,881	17·7
1820	508,717	9,633,822	18·9
1830	632,717	12,866,020	20·3
1840	807,292	17,069,453	21·1
1850	979,249	23,191,876	23·7
1860	1,194,754	31,443,321	26·5
1870	1,272,239	38,558,371	30·2

It is noticed here that in the 40 years following 1790, the area of settlements increased 163 per cent, and the density 24 per cent, while in the next 40 years the area increased 101 per cent and the density 44 per cent. While this increased ratio of density is in part due to the denser settlement of rural districts, it is mostly due to an increase in the city and village populations. We find that in 1800 there were but six cities of over 8,000 inhabitants, and in 1840 but 44; the number had increased to 226 in 1870. He says: "Speaking roundly, it may be said that in 1790 one-thirtieth of the population was found in cities; in 1800 one twenty-fifth; in 1810 and also in 1820 one-twentieth; in 1830 one-sixteenth; in 1840 one-twelfth; in 1850 one-eighth; in 1860 one-sixth; in 1870 more than one-fifth."

That popular subject of so many writers, "the center of population," is also fully discussed. In 1790 it was "about 23 miles east of Baltimore." It has traveled westward, keeping curiously near the 39th degree of latitude, never getting more than 20 miles north nor two miles south of it. In the 80 years it has traveled only 400 miles and is still found nearly 50 miles eastward of Cincinnati.

The intimate and varied relations between the density and spread of population as shown by the maps of the second part, and the physical features of the country as shown by the maps of the first part, are intensely interesting and varied, but the subject is too fertile to be entered on here.

Part 3d consists of two memoirs and eighteen plates, six of which are maps. They may be said to cover three classes of subjects, one relating to age, sex and birth, the next to mortality, the third to the "afflicted classes." The relative distribution of the population by age and sex in each of the States is shown by the geometrical method; the geographical distribution of the predominating sex, and of the birth-rate is shown by maps. The relations of the birth-rate map to various others of the atlas are as interesting to the student in physical geography and anthropology as to the political economist. Considered as a whole, the newer and agricultural regions very naturally have a higher birth-rate than the older and denser population. There are several curious areas where a low birth-rate accompanies a high relative number of women, as, for example, a belt in southern New York and another in Ohio. Possibly this fact may be true in the country as a whole, but it is by no means universal. While the chief controlling conditions of the birth-rate are unquestionably social, some interesting and curious relations to physical causes are seen. There is a belt of rather high birth-rate accompanying the Appallachian system from New Jersey to Alabama, cut through by a belt of lower intensity where the Susquehanna River cuts through. It has also other peculiarities, and a comparison of this map with those relating to density of population, the geographical distribution of sex, distribution of wealth, elevation, extremes of climate, and mortality by certain diseases, show that, along this belt at least, physical causes are a most important or controlling condition.

The next series relates to mortality. Four maps show the geographical distribution of relative mortality by certain classes of diseases. Two charts (by geometrical methods) show the distribution of deaths by age, sex, race, nationality, and month of death for each State and Territory, and also the same for certain special diseases. These are accompanied by two memoirs. The work closes with eight charts, showing (by geometrical methods) the distribution by age, sex, nationality, &c., of each of the "afflicted classes" (the blind, mute, insane and idiotic) for each State and Territory. These (prepared by Mr. F. H. Wines) contain methods of illustration which have some very desirable features.

Any commendatory notice of this work which did not speak of its mechanical execution would be most unjust. When we consider the intrinsic difficulties of the case, and moreover that it is the first work of the kind yet made in the United States, we must accord high praise to Mr. Julius Bien, who has done the lithographic work.

It is to be hoped that Congress will allow this atlas to be printed and sold at as low a price as the cost of manufacture will admit.

W. H. B.

ART. XVI.—*On the Chondrodite from the Tilly-Foster Iron Mine, Brewster, New York;* by EDWARD S. DANA. With plates V, VI and VII.*

THE method of occurrence of the Chondrodite at the Tilly-Foster iron mine, near Brewster, Dutchess Co., New York, has been fully described by Professor Dana in a memoir on the Serpentine and other pseudomorphs of the region.† It may be of interest, however, to review the subject again in this place.

The chondrodite forms the gangue of the magnetite, being everywhere disseminated through it in varying proportions. In the parts of the mine where the ore is purest and perfectly firm and solid,- -the so-called "blue ore"—the associated chondrodite is sparsely sprinkled through it in small yellow grains, showing no trace of crystalline form.

In the larger portion of the mine as now opened, however, the soft "yellow ore" predominates: the chrondrodite is present in it in much larger quantities, and, like the other associated minerals, it has almost universally suffered extensive alteration A large number of these products of alteration have been described by Prof. Dana in the memoir alluded to. The chondrodite in this "yellow ore" is generally massive; but occasionally fragments of large coarse crystals have been found, some of which measured five or six inches in length. These are always more or less altered; moreover the material of which they are formed is far from homogeneous, masses of magnetite, and also chlorite, being often enclosed. Dolomite is the most constantly associated mineral and occurs in rhombohedrons of considerable size: these, as well as the crystals of chondrodite, are often coated with magnetite.

Better crystals of chondrodite than those just mentioned are sometimes found in what were once cavities in this massive material. Unfortunately these have all suffered from the general alteration and now have little or no luster, and often are not even smooth. These crystals vary in size, being sometimes an inch or two in length. A crystallographic examination of them is seldom possible, but a few of the crystals found allow of it, and the results are described beyond. The form is usually very simple, and the color varies from a deep red to a light yellow.

Material much better adapted for crystallographic study also occurs, though this is rarely true. Narrow veins are sometimes met with, two or three inches across, which were origin-

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*** Abstract of a memoir, published in full in the Transactions of the Connecticut Academy of Sciences, vol. iii, pp. 67 to 96, prepared for this place by the author.**

† This Journal, III, viii, 371, 447, 1874.

ally lined with more or less perfectly crystallized chondrodite, and also with dodecahedrons of magnetite, crystals of ripidolite. and rarely apatite, and then subsequently filled in with dolomite Where this has been the case and the dolomite has remained intact the chondrodite has been protected and the crystals have retained perfectly their brilliant luster and garnet-red color.

The species chondrodite is of especial interest because of its relation to the Vesuvian humite. As shown by Scacchi, and confirmed by vom Rath, the humite crystals are of *three types*, alike in the ratio of their vertical axes but differing in the length of the vertical axes. Chondrodite is identical with humite in chemical composition, and, as shown by the investigation of the Brewster mineral, alike in crystalline form. Until now the correspondence of the two minerals had been proved only for the *second* type. But the Tilly-Foster mine has afforded crystals of each of the three types, and I am therefore enabled to announce a close relation between the two for all the types.

1. *Description of Crystals belonging to Type II.*

Some difficulty was found in obtaining the value of the fundamental angles from the fact that many crystals, though faultless in luster. yet gave uncertain measurements. This was due to the fractured condition of many of the planes, which, though often not very apparent even under a magnifier, yet gave rise to a variety of reflected images in the goniometer, no one of which could be accepted as trustworthy. The presence of these cracks gave the crystals the appearance of having suffered sudden contraction, by which the planes had been irregularly drawn inward, forming re-entrant angles; in fact, in this respect, as in general appearance, this chondrodite might be aptly compared to a resin.

The smallest crystals proved to be free from this cause of irregularity, and one of them, on which the faces were exceptionably brilliant, was chosen for careful measurement. It may be added that all the measurements were made with an Oertling goniometer, provided with two telescopes; but using in the second telescope, in place of the ordinary spider lines, a cross cut in tin-foil illuminated brilliantly by a gas burner —a device for which I am indebted to Prof. Schrauf of Vienna.

The mean of 30 measurements of A (O=001) on r^1 gave: 135° 18′ 50″. The maximum variation from the mean given was ±45″. The mean of 30 measurements of A on e^a gave: 149° 55′ 48″. Maximum variations ± 45″. These were accepted as the fundamental angles. Calculated from these angles the parameters are:

$$a\ (\text{vert.})=1{\cdot}57236;\ b=1;\ c=1{\cdot}08630;$$

and the angle for the fundamental prism is

$$I \wedge I\,(110 \wedge 1\bar{1}0)=58^\circ\ 15'\ 46''\ \text{or}\ 94^\circ\ 44'\ 14''.$$

Table I.

			Chondrodite.			*Humite.*
			$C=i\text{-}\breve{i}$ (010) Calculated.	A$=O$ (001) Measured.	Calculated.	A (v. Rath) Calculated.
i	1-ĭ	011	147° 32′ 39″	122° 44′ (ap.)	122° 27′ 21″	122° 27′ 49″
e^a	$\frac{2}{5}$-ĭ	205	90	*149 55 48	149 58 48	
e^1	$\frac{2}{3}$-ĭ	203	90	135 59	136 1 17	135 52 15
e^2	2-ĭ	201	90	109 4	109 3 24	108 57 50
r^1	$\frac{4}{7}$-$\bar{2}$	$\bar{2}47$	129 42 9	*135 18 50	135 18 50	135 17 40
r^2	$\frac{4}{5}$-$\bar{2}$	245	137 25 45	125 52	125 50 6	125 49 0
r^3	$\frac{4}{3}$ $\bar{2}$	$\bar{2}43$	146 27 42	113 25½	113 25 36	113 24 45
r^4	4-$\bar{2}$	241	154 2 9	98 14	98 13 6	98 12 47
m^2	6-$\frac{\breve{3}}{2}$	$\bar{6}41$	125 43 56	95 22	95 19 40	95 17 59
n^1	$\frac{2}{3}$	$\bar{2}23$	127 1 31		125 3 49	125 2 47
n^2	2	221	135 45 24	103 11	103 10 4	103 9 35

The preceding table* (I) includes the principal angles measured on the same crystal, and also those calculated from the above parameters; in addition, the corresponding angles for humite, type II, are also given, as calculated by vom Rath. The angles of the macrodomes agree very closely, it will be observed, in chondrodite and humite; in the brachydomes, on the other hand, there is a divergence of 6 or 7 minutes.

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* Both the symbols of Naumann (in the form used in Dana's "Mineralogy") and also of Miller are given; the signs belonging to each plane are omitted here, as the relations of the planes are shown with sufficient clearness on the spherical projection, Plate VII.

The fundamental form employed is the same as that adopted in Dana's Mineralogy; for the reasons for retaining it, as also its relation to those of other authors, reference must be made to the complete memoir. The letters used are those of Scacchi, with this change, that for the planes of the *third* type the corresponding Greek letters, and for the *first* type the corresponding capital letters, are employed.

TABLE II.

$A = O(001)$.

			Calculated.	II.	III.	IV.
i^1	$1\text{-}\bar{\imath}$	011	57° 33′	57° 28′		
e^a	$\frac{2}{5}\text{-}\bar{\imath}$	205	30 4			
e^1	$\frac{2}{3}\text{-}\bar{\imath}$	203	43 58½			
e^2	$2\text{-}\bar{\imath}$	201	70 57			
r^1	$\frac{4}{7}\text{-}\bar{2}$	$\bar{2}47$	44 41	44 40½ 44 40	44° 37′	44° 41′
r^2	$\frac{4}{5}\text{-}\bar{2}$	$\bar{2}45$	54 10		54 12	
r^3	$\frac{4}{3}\text{-}\bar{2}$	$\bar{2}43$	66 34	66 39 66 32	66 25	66 35
r^4	$4\text{-}\bar{2}$	241	81 47	81 52		
n^1	$\frac{2}{3}$	$\bar{2}23$	54 56			
n^2	2	221	76 50			
m^2	$6\text{-}\frac{\breve{3}}{2}$	$\bar{6}41$	84 40			

TABLE III.

$C = i\text{-}\bar{\imath}(010)$.

			Calculated.	II.	III.	IV.
i^1	$1\text{-}\bar{\imath}$	011	32° 27′	32° 28′ 32 32		
e^a	$\frac{2}{5}\text{-}\bar{\imath}$	205	90			
e^1	$\frac{2}{3}\text{-}\bar{\imath}$	203	90			
e^2	$2\text{-}\bar{\imath}$	201	90			
r^1	$\frac{4}{7}\text{-}\bar{2}$	$\bar{2}47$	50 18	50 7 50 30	50° 13′ 50 30	50° 12′
r^2	$\frac{4}{5}\text{-}\bar{2}$	$\bar{2}45$	42 34	42 39	42 32	42 35
r^3	$\frac{4}{3}\text{-}\bar{2}$	$\bar{2}43$	33 32	33 31 33 32	33 30	33 27
r^4	$4\text{-}\bar{2}$	241	25 58		26 0 25 57½	
n^1	$\frac{2}{3}$	$\bar{2}23$	44 14½		44 11	
n^2	2	221	52 58			
m^2	$6\text{-}\frac{\breve{3}}{2}$	$\bar{6}41$	54 16		54 18	

In tables II and III are given the angles on A and C as measured in a series of crystals here numbered II, III, IV. (The measured angles of other crystals are given in the memoir.) They are important as showing how far the angles are constant. Some considerable variations from calculated angles in a few instances are to be explained by the cause already mentioned—the occurrence of irregular fractures across the planes.

Of the planes which occur, according to Scacchi and vom Rath, on type II of humite, I have identified all but *m* of Scacchi ($\frac{6}{5}\text{-}\frac{3}{2}$) and $\frac{3}{5}$-*e* of vom Rath ($\frac{6}{5}\text{-}\breve{\imath}$). Of new planes I have found the following which fall in the old vertical or horizontal zones, and many others to be described later; $o\,(i\text{-}\breve{2}=210)$, $i^{2}\,(2\text{-}\bar{i}=021)$, $i^{\alpha}\,(\frac{4}{7}\text{-}\bar{i}=047)$, $i\beta=(\frac{2}{5}\text{-}\bar{i}=025)$, $e^{a}\,(\frac{2}{5}\text{-}\breve{i}=205)$, $r^{a}\,(\frac{8}{9}\text{-}\bar{2}=\bar{4}89)$. Of these the most interesting is the prism $i\text{-}\breve{2}$, as hitherto no vertical prism has been found on either the 2d or 3d types.

Hemihedrism.—The crystals of chondrodite should show an entire correspondence to humite in hemihedral characters. Taking the same position for the crystals as vom Rath, r^{2} and r^{4} appear uniformly in the positive (or upper) quadrants, r^{1} and r^{3} always in the negative (or lower) and n^{2} is both + and —, but where occurring alone is generally negative; n^{1} is generally, and m^{2} is always, negative. Of the brachydomes it may be said that they are often holohedral, but this is not always the case. The various figures on the two plates will show the true relation better than words. It is to be said, however, that when the brachydomes are ± they are still distinguished from each other physically. Thus the + series may be largely developed and rough, destitute of any semblance to polish, when the negative series is as lustrous as the pyramidal planes.

Habit.—With regard to the general habit of the crystals, it is interesting to note the wide variation which is shown. Figures 1, 2, 6, 9, 10 are intended to give some idea of the crystals, as drawn symmetrically, and figures 7, 10, 14, 15, 16, 17, 18, 19, of their actual appearance. As will be seen, the figures are drawn with *C* in front: this was necessary in order to give a true idea of their real appearance. Since the prism $i\text{-}\bar{2}$ is so acute (49½°) toward the eye the projection gives it but little width. It is hence clear that while fig. 3 is an almost exact reproduction of an actual crystal, fig. 5, by the other method of projection, gives a wrong idea of its appearance.

The crystals from which the partial figures, 7, 10, 16, 17, were drawn, along with others quite as diverse, were all in one single group only half an inch in length. The crystals drawn in figures 16 and 19 also occurred closely conjoined in the same group; and other examples of like diversity in associated crystals might be mentioned. One crystal of a very prismatic appearance (when placed in an inverted position) is shown in figure 19.

Presence of minute planes.—The most remarkable feature of the mineral from this locality is the multitude of minute planes which modify many of the solid angles. One single case will be discussed in detail, as the planes admitted of more than usually exact determination; it serves well to illustrate the subject. A horizontal projection of a portion of the crystal is shown in fig. 14. The crystal itself was small, and unfortunately so imbedded in dolomite that it was for the most part rough and beyond even approximate measurements. The part available showed C faultless, also r^3 good; and less satisfactory r^1, r^2 and r^4. On the solid angle between C, r^3, and r^2, a large number of minute planes were observed; they were so extremely small (all covering a surface not ·03 of an inch in breadth) that any exact measurements seemed at first hopeless. They were sharply defined, however, and brilliant, and when the attempt was made it was found that they gave perfectly distinct though faint reflections.

TABLE IV.*

			$C = i\text{-}\bar{i}(010)$.		$r^3 = \bar{2}43$.		i^2=201.	
			Meas.	Calc.	Meas.	Calc.	Meas.	Calc.
i^2	2-ī	021	17° 37′	17° 38½	23° 58′	23° 49′		
$+x^1$	$\frac{12}{7}$-6	2·12·7	22 9	21 53	31 4	31 12	8° 21′	8° 37′
$+x^2$	$\frac{26}{9}$-13	2·26·9	12 55	12 58	29 32	29 27	6 36	6 35
$+x^3$	$\frac{26}{7}$-13	2·26·7	10 34	10 30	30 37	30 42	8 50	8 54
$+x^4$	$\frac{34}{7}$-$\frac{17}{2}$	4·34·7	9 42	9 39	33 41	33 41	12 0	11 53
$-x^6$	$\frac{12}{7}$-24	$\bar{1}$·24·14	20 21	20 28	21 11	21 6	3 25	3 24
$-x^5$	$\frac{13}{12}$-$\frac{13}{5}$	$\bar{5}$·13·12	34 32	34 27	7 32	7 15	21 15	21 8
$-x^7$	$\frac{13}{7}$-13	$\bar{1}$·13·7	18 50	19 17	19 40	19 47	4 5	4 3
$-x^8$	$\frac{24}{13}$-4	$\bar{6}$·24·13	22 24	22 31	12 15	11 59	12 22	12 22
$-x^9$	$\frac{5}{2}$-$\frac{15}{2}$	$\bar{2}$·15·6	16 5	15 47	18 51	19 7	7 39	7 34
$-x^{10}$	$\frac{13}{7}$-$\frac{13}{4}$	$\bar{4}$·13·7	24 22	23 59	9 26	9 47	15 25	15 3
$-y^2$	$\frac{25}{9}$-$\frac{25}{7}$	$\bar{7}$·25·9	18 54	19 2	14 45	14 38	14 40	14 52
$-y^1$	3-$\frac{8}{3}$	$\bar{9}$·24·8	21 55	22 4	13 18	13 14	19 32	19 29
$-y^4$	9-$\frac{9}{2}$	$\bar{2}$91	11 43	12 13	23 31	23 14		
$-y^3$	24-$\frac{24}{9}$	$\bar{9}$·24·1	18 45	19 5	25 34	25 27		

The measurements were all taken with the greatest care; and after the calculations had been made, they were repeated; and rarely was a variation found greater than two minutes. The preceding table contains the supplement angles for each of these minute planes as measured on C and r^3, and also on i^2.

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* Table VI in the Memoir.

The calculated symbols are also given, with the angles which belong to them. It will be noticed that i^2 is itself one of the minute planes, of the same character as those surrounding it; and its presence gives a reality to them which they would not otherwise have, and shows what degree of reliance is to be placed on the angles.

The symbols* calculated for this series of planes are certainly not simple; and yet a moment's consideration will show that this was exactly what was to be expected. Crowded together so closely, they would be abnormal if occurring on crystals of any species, while this becomes still more true for a mineral like chondrodite. The constantly recurring common planes have ratios which in any other species would be considered next to impossible: thus, in type II, $1 : \frac{1}{3} : \frac{1}{5} : \frac{1}{7}$; and in type III, $1, \frac{1}{3}, \frac{1}{5}, \frac{1}{7}, \frac{1}{9}, \frac{1}{11}$. It is not surprising, then, that these minute planes should themselves have symbols totally at variance with the accepted law of simplicity of the indices.

It will be noticed, however, that, lawless as they appear at first, there is an attempt at system in the symbols given. Thus, in the ratio of the brachydiagonal to the vertical axis, we have:

x^1	12 : 7	y^2	24 : 13	x^7	12 : 7	x^3	26 : 7
x^6	12 : 7	y^1	24 : 8	x^{10}	13 : 7	x^2	26 : 9
		y^3	24 : 1	x^5	13 : 12		

and so on.

Almost all of the twenty and more smaller crystals examined showed some of these secondary planes. I have measured, in fact, upward of one hundred of them. Examples of a number of them are shown in the figures on the plates and their angles are given in several tables in the original memoir. The description of the one case above given is sufficient for this place.

Twins.—The humite crystals of Vesuvius, as well as the Swedish chondrodite, has been shown by vom Rath to possess so great a tendency to twinning that it is a little remarkable that the contrary should be true of the mineral from Brewster. Figs. 20, 21, show the only method of twinning which has been found. The axis of revolution here is the vertical axis of the crystal, and the composition-face the basal plane A. Unfortunately the crystal in question was quite imperfect, and all that was available is shown in the figure. A revolution of the kind mentioned (in a perfectly symmetrical crystal) would, so far as this half of the crystal goes, have the effect only of making it holohedral, giving no re-entrant angles; but, in case of any irregularity, it might give, as here, a re-entrant angle in the planes which are hemihedral in their occurrence.

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* In the symbols given in the tables (i. e., those according to Naumann) the mark over the second figure, or fraction, has been omitted (in order to simplify the work of the printer). This has also been done in all the following tables, being made possible by the fact that all these planes, with one or two exceptions, belong to the *macrodiagonal* series.

The measurement of the supplement re-entrant angles here observed gave for $m^3 \wedge \bar{m}^3$, 10° 38′ and 10° 40′; required 10° 39′.

A noteworthy fact in the crystal is the occurrence of the prism o (i-2 = 210), the first time that any of the vertical prisms has been observed in the 2d type of either humite or chondrodite. It lies in both the zones $e^3(201)$, $n^3(2\bar{2}1)$, and $e^3(22\bar{1})$, $n^3(2\bar{2}1)$ which answers sufficiently to determine what it is; and the result thus obtained is fully sustained by good measurements. This plane is distinctly present on one side only of B; on the other side its presence is barely indicated. Its place here is taken by a well polished and conspicuous plane Φ, which is another striking instance of the peculiar nature of this species. The index was calculated for each pair of measurements 38° 14′, 39° 24′ on e^3(201 and 20$\bar{1}$) and 4° 31, 6° 31 on m^3(641 and 64$\bar{1}$), and the results obtained were identical. From the first pair of measured angles $\Phi \wedge B$ was found to be 34° 30½′, and from the second $\Phi \wedge B$=34° 31′ (required 34° 32′ 38″). The index obtained was 60-$\frac{38}{10}$ or 60·38·1, and, abnormal as it certainly is, it expresses the exact position of the plane.

An interesting crystal is shown in fig. 4. It is conspicuously hemimorphic, as far as the form goes. It is large, and admits only of approximate measurements; but there is no doubt that the planes as given have been determined correctly. It is altogether probable that a revolution parallel to the basal plane would form an ample explanation of what is observed.

Chemical composition.—I am glad to be able to add here the results of a chemical examination of the chondrodite of the 2d type from this locality, by Mr. G. W. Hawes of the Sheffield Scientific School.

The material analyzed by Mr. Hawes consisted of fragments of crystals of the 2d type, selected with great care to avoid the presence of any altered material. It had a deep garnet-red color and a brilliant vitreous luster. Its specific gravity, as determined by Mr. Hawes, was 3·22. Two analyses gave:

	Analysis I.	Analysis II.
Silica	34·10	34·05
Magnesia	53·17	53·72
Ferrous oxide	7·17	7·28
Alumina	·48	·41
Fluorine	4·14	3·88
	99·06	99·34

Following the view of Rammelsberg (that, in consequence of the unavoidable loss in the course of the analysis, the higher value of each constituent comes nearest to the truth,) Mr. Hawes's analysis becomes as follows. For comparison the results obtained by vom Rath for 2d type crystals from Vesuvius and from Sweden are added.

	CHONDRODITE.		HUMITE.
	Brewster, N. Y., *Hawes.*	Sweden, *v. Rath.*	Vesuvius, *v. Rath.*
Silica	34·10	33·96	34·02
Magnesia	53·72	53·51	59·23
Ferrous oxide	7·28	6·83	1·78
Alumina	0·48	0·72	0·99
Fluorine	4·14	4·24	2·74
	99·72	99·26	98·76
Silicon	15·91	15·85	15·88
Magnesium	32·23	32·11	35·54
Iron	5·66	5·31	1·38
Aluminum	0·26	0·38	0·53
Fluorine	4·14	4·24	2·74
Oxygen	39·78	39·58	41·54
	97·98	97·47	97·61

Transforming the iron into an equivalent of magnesium, as also the alumina (2Al=3Mg), Mr. Hawes obtains further:

Silicon 15·91, Magnesium 35·00, Fluorine 4·14, Oxygen 39·78.

From these values a formula is deduced, which is essentially that of the Swedish mineral according to v. Rath,

$$20(Mg_5Si_2O_9)+Mg_5Si_2Fl_{18}.$$

It would have been extremely interesting to have added analyses also of crystals of the 1st and 3d types; but, as will be apparent from what follows, the material was not to be obtained.

In completing the description of this variety of the mineral, it may be repeated that it occurs usually in narrow veins, and when free from alteration has uniformly a deep *garnet-red* color. A *cleavage* such as exists in humite (parallel to the basal plane), and has been observed by Kokscharow on chondrodite from Pargas, could in no case be discovered. The fracture is always conchoidal.

2. *Description of Crystals of Type III.*

The crystals of the 3d type are exceedingly rare, three or four specimens being all that have thus far been found, and from these only two individual crystals could be obtained which allowed of measurement. Fortunately these two crystals are very satisfactory, being small and brilliant, and establish the fact as well as hundreds could do. Figures 11 and 12 show one of the crystals, and figure 13 the other. The appearance of the first crystal is best shown in the second of these figures. As will be seen, the planes are the same as in humite, and they are for the most part hemihedral and situated in the same way. No brachydomes are visible, the edge being rounded and rough.

TABLE V.*

			Chondrodite.			Humite.
			$A = O$ (001). Calc.	Meas.(XX).	Meas.(XXI)	v. Rath. Calculated.
ι^4	$4\text{-}\bar{\imath}$	041	100° 1′ 7″			
ι^3	$2\text{-}\bar{\imath}$	021	109 27 35			109° 27′ 54″
ι^2	$1\text{-}\bar{\imath}$	011	125 14 49			125 15 18
ι^1	$\frac{2}{3}\text{-}\bar{\imath}$	023	136 40 4			136 40 34
ρ^3	$\frac{8}{11}\text{-}\bar{2}$	$\bar{2}811$	131 25 57	131° 46′	131° 24′	131 24 49
ρ^4	$\frac{8}{9}\text{-}\bar{2}$	289	125 50 6	125 37 125 47	125 48	125 49 0
ρ^5	$\frac{8}{7}\text{-}\bar{2}$	$\bar{2}87$	119 19 18	119 35 119 15	118 36	119 18 19
ρ^6	$\frac{8}{5}\text{-}2$	285	111 51 38	111 44 111 49	112 0	111 50 50
ρ^7	$\frac{8}{3}\text{-}\bar{2}$	$\bar{2}83$	103 32 4	103 41	103 38	103 31 33
ρ^8	$8\text{-}\bar{2}$	281	94 35 15	94 31 94 13	94 48	94 35 4
ν^1	$\frac{4}{7}$	447	132 17 48		132 16	132 16 43
ν^2	$\frac{4}{5}$	445	123 1 8		122 32	123 0 8
ν^3	$\frac{4}{3}$	443	111 18 7		111 5	111 17 23
ν^4	4	441	97 24 20		97 29	97 24 3

The second crystal is of very different form, and was imbedded in brucite, and eutirely free in it. It was perfectly formed on all sides, being almost as perfect as the drawing, with the exception, however, of the acute (brachydiagonal) edge, which was mostly broken. When only the upper part of the crystal is considered, it will be seen that the hemihedrism is like that in the other case, except that ρ^8 is holohedral. For macrodomes there are $\imath^1(\frac{2}{3}\text{-}\bar{\imath}=023)$, $\imath^2(1\text{-}\bar{\imath}=011)$, $\imath^3(2\text{-}\bar{\imath}=021)$, $\imath^4(4\text{-}\bar{\imath}=041)$; the last has not been observed on humite. On measuring the planes below, it was found that they were not distributed as was expected in accordance with the monoclinic character of the crystal; instead, either extremity of the brachydiagonal axis was differently developed. There are present also at one extremity $\pm\varepsilon'(\frac{4}{7}\text{-}\bar{\imath}=407)$, though the plane could be only approximately measured. This is probably also to be explained as having resulted from a revolution parallel to the basal plane. The crystal was very small and not at all adapted to experiments having in view the discovery of any proper hemimorphic development. Some of the angles measured on both these crystals are contained in the preceding table.

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* Table XII of the memoir. Other angles of these crystals are given in Table XIII.

Unfortunately the inclination to C on no one of the pyramidal planes could be measured with perfect accuracy; the measurements are good, yet not entirely trustworthy. These planes, though brilliant, are uniformly fractured in the manner already explained, and this made all the angles a little uncertain. The calculated angles as given have as their basis the prismatic angle $I \wedge I = 94° 44' 14''$, and the macrodome angle $C \wedge \imath^2 = 144° 45' 11''$, following the analogy of humite in which the vertical axes of types II and III have the ratio 10 to 9.

The corresponding parameters are:

$$a\,(\text{vert.}) = 1{\cdot}41512\,;\; b = 1\,;\; c = 1{\cdot}08630.$$

Very little further can be said in regard to the crystals of the 3d type. Those observed had a somewhat different color from those of type II; that is, the color was more yellowish, less of a pure garnet-red—though this may be accidental. No analysis was possible of course. The method of occurrence was much like that of the brilliant crystals of the second type; and the associated minerals were the same, with, probably as a later formation, brucite.

3. *Description of Crystals of Type I.*

The occurrence of large coarse crystals of quite impure chondrodite, imbedded in the massive material, has already been described. These belong, at least in part, to the *first* of Scacchi's types. As has been remarked, the crystals of this character do not often admit of exact determination, but in two cases they were so good as to allow of their crystallographic relations being accurately made out. The accompanying wood-cuts, figures 22 and 23, give faithful representations of their appearance and size. It will be seen that they are both quite imperfect, and it was on this account that no attempt was made to make a symmetrical drawing of either of them.

Fig. 22.

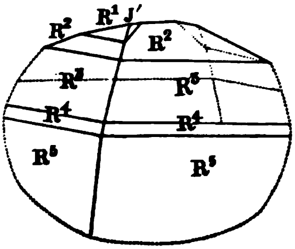
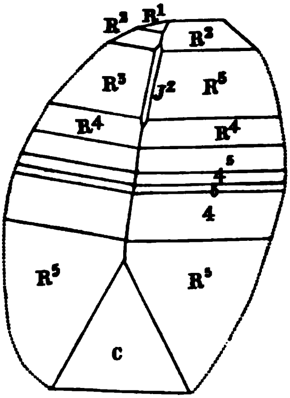


Fig. 23.



R^3 on R^3 (behind) gave measurements varying, in a series of trials, from 78° to 79°: required 79° 4′.

R^5 on R^5 (behind) gave 62½°, required 63° 1′.
R^3 on R^5 (behind) gave 72, required 71 17½.
R^5 on R^3 (behind) gave 72, required 71 17½.

These angles on both crystals were identical within the allowed error of observation (say 30′). The above are the best angles afforded by any of the planes.

These angles can be referred only to the 1st type of humite. Decisive proof that this is right is found in the fact that both crystals are *holohedral*, the planes on both sides being identical, with the exception of R^1.

The measured angles of C on R^5, right and left, were identical, though not obtainable with exactness; the measurements gave 152½°–154°: this is also true for C on R^3, right and left, =140½°–142½°.

The following table includes the most important angles for the occurring planes, calculated from the fundamental form of the second type on the assumption that the lateral axes are equal and the vertical axes have the ratio of 14 : 15. The measured angles are also added, though only approximate; in the form given they were obtained immediately from the measurements over the top of the crystals (see above).

The two crystals described are the only ones which could be positively identified. It is very probable, however, that of those found others also belong here, as they have much the same appearance and habit. These crystals are all considerably altered, being generally soft enough to be cut with a knife, and for this reason a chemical analysis would be of little value.

TABLE VII.*

			Chondrodite.			Humite.
			$C = i\text{-}\breve{\imath}$ (010). Calculated.	$A = O$ (001). Meas.	Calc.	v. Rath. Calculated.
J²	1-ĭ	011	145° 43′ 44″		124° 16′ 16″	124° 16′ 45″
J¹	$\frac{3}{5}$-ĭ	035				138 38 38
R¹	$\frac{3}{5}$-$\bar{2}$	3·6·10	129 12 57	135°	135 53 35	135 52 23
R²	$\frac{3}{4}$-$\bar{2}$	368	134 28 38	129½	129 32 3	129 30 52
R³	1-$\bar{2}$	122	140 30 10	121½	121 45 28	121 44 23
R⁴	$\frac{3}{2}$-$\bar{2}$	364	147 6 34		112 25 28	112 24 37
R⁵	3-$\bar{2}$	362	152 49 49	101	101 39 30	101 39 2

The color of the crystals is gray to grayish-yellow, and the material of which they are composed is never pure, and often quite heterogeneous.

4. *On the Optical Properties of Chondrodite.*

In the preceding pages the question of the orthorhombic or clinorhombic crystallization of the chondrodite has not been discussed. In fact, nothing was detected by the measurements sustaining any other conclusion than that of Scacchi and vom Rath, that the crystals were fundamentally orthometric. Still the hemihedral character of the second and third types seem to point to a clinometric form, and this is apparently supported by the optical characters obtained. The material available for optical investigations was very scanty, and, with the exception of one crystal, poorly adapted for the purpose.

The crystal referred to was, properly, but the fragment of what was originally a specimen of considerable size and beauty; when unbroken it must have been nearly an inch in length. In the condition in which it was found it showed only the brachydomes e^1 and e^2, with the pyramids n^1, n^2, and m^3; it had the deep garnet-red color of crystals of the second type, and with the exception of the universally present fractures was perfectly clear and transparent.

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* Table XIV of the Memoir.

Guided by the observations on the optical properties of humite made by Descloizaux and given in his Mineralogy (p. 14), a section was cut from the crystal described parallel to *C*, i. e., perpendicular to the brachydomes present. The examination of this section showed: 1st, that the acute bisectrix is normal to *C* (i-$\breve{i}$, 010); 2d, that this bisectrix is positive; 3d, that the optic-axial angle is large, the axes being seen only when oil is used; but 4th, that *the axes do not lie in the basal plane*, but in a plane making an angle of about 154° with it. This last point was so unexpected and anomalous that every effort was made to explain the measurements in some other way, but with no success. By means of a stauroscope, made by Fuess in Berlin after the excellent pattern of Groth, the position of the two axes of polarization, as referred to e^1, and also to e^3 in plane *C*, were carefully determined. The measurements were repeated twenty times, the error arising from an imperfect adjustment of the Nicols being eliminated in the usual manner. The result was as follows:

Supplement angle made by the plane of the axes—

with e^1 ($\tfrac{2}{3}$-$\breve{i}$=203), 18° 9′; hence with the basal plane, *C*, 25° 50′.
with e^3 (2-$\breve{i}$=201), 45° 9′; " " " *C*, 25° 46′.

In order to confirm these results, other crystals were sought, which would admit of like determinations. None could be found which would serve for measuring the axial angle; but two small ones, on which the plane *C* was naturally developed, proved to be clear enough to allow of measurements with the stauroscope. The first alone gave accurate results; on it the angle of the same plane with e^a($\tfrac{2}{5}$-$\breve{i}$=205) was determined with equal care. The results were:

4° 55′ for the angle with e^a; and hence 25° 59′ with *C*.

The agreement with the angles given above is as close as could be desired. In the other case, the rather rare plane *B*(i-$\breve{i}$=100) was present: the crystal was minute, however, and the determination only approximate. It was found that the normal to the axial plane made with *B* an angle of 65°–70°, and hence with the normal to the basal plane 20°–25°.

With so ample confirmation the point made cannot be even questioned, and it remains to reconcile it with the crystallographic properties of the species. It will be seen at once that the position of the optic axes is totally at variance with the accepted orthorhombic character of the crystals; but it conforms to the rule of monoclinic crystals, as one axis of polarization is normal to the plane of symmetry *C*, and the others lie in it, or in other words, the optic-axes lie in a plane perpendicular to the axis of symmetry. The angles measured and calculated, given in the various tables, show that the variation from the rectangular type, if it really exist, must be very slight, as the agreement between the angles measured and those calculated on the assumed prismatic basis is very close—it being remarked that some considerable variation in the angles given in the tables are simply explained by the imperfection of the crystals. Note the angles measured for $m^2 \wedge \bar{m}^2$ on the twin crystals described on page 96. It was not to be expected that the variation in the optical character of the crystals would be so decided in view of the slight divergence which is possible in the crystalline form. I reserve for the future the careful revision of the angles of this species, when I shall hope to be able to command a more abundant supply of satisfactory material. It may be added that the hemihedral character of the second and third types of humite long ago suggested the idea that they were oblique in form; but all the crystallographic investigations thus far have seemed to deny this. In the Mineralogy of Brook and Miller, the form is made oblique, but this seems to be due to a misunderstanding of the planes occurring on the crystals.

The axes, as already mentioned, do not appear distinctly except in oil; in the first mentioned section they admitted of good measurements. The mean of thirty determinations of the angle for red rays gave—

$2\ H\alpha = 88^\circ\ 48'$: the extremes being $88^\circ\ 36'$ and $89^\circ\ 0'$.

With a yellow light (sodium) the angle was essentially the same, but the mean was 10′ or 15′ smaller, which would indicate that the dispersion is $\rho > v$, but the matter cannot be considered to be beyond doubt.

The index of refraction of the oil employed, as determined by Professor Wright and myself, was 1·466.

In conclusion, I have to express my very great obligations to Prof. Allen for his kindness in giving me free use of all the specimens in his valuable cabinet. Both of the crystals of the third type, as well as several others mentioned, came from his collection; in fact it was Prof. Allen who first made known the special interest connected with the locality. To Mr. Cosgriff, the superintendent of the Tilly-Foster Iron Mine, I am also much indebted for his uniform kindness and courtesy to me at the several occasions when I have visited the mine, and also for the gift of several fine specimens.

ART. XVIa.—*On an easy method of producing Di- and Trinitrophenetol;* by PETER TOWNSEND AUSTEN.

THE usual way of producing these nitrophenetols is either by digestion of ethyl iodide with sodium di- or trinitrophenylate in a closed tube,* or by direct nitrizing of the phenetol.† The following procedure will be found more satisfactory.

Di- or trinitrochlorbenzol is dissolved in absolute alcohol and about twice the calculated amount of sodium necessary for the reaction,

$$C_6H_3(NO_2)_2Cl+C_2H_5ONa=C_6H_3(NO_2)_2OC_2H_5+NaCl$$

gradually added in small pieces. The liquid becomes deep red, and under violent evolution of hydrogen finally boils from the heat of the reaction, while a brown crystalline precipitate separates. After solution of the sodium, water is added and the liquid then acidified with hydrochloric acid. After filtering and washing with water, the nitrophenetol may be obtained perfectly pure by treatment with animal charcoal and recrystallization from boiling absolute alcohol. The purity of the di- and trinitrophenetol thus obtained was established by fusing point and analysis.

It is very striking in this case that the nitro-groups are not reduced by the violent evolution of hydrogen, or that they are not azoxized by the hot alcoholic alkaline solution.

Mononitrochlor-benzol treated in this manner yielded only the known dichlorazoxybenzol.‡ From higher alcohols with which the hydrogen evolution was not so violent, dinitrobrombenzol gave no satisfactory results, black slimy bodies, apparently products of a partial reduction being all that I obtained. With benzylalcohol I obtained also no dinitrophenolbenzylester, but a yellow substance separating from a boiling alcoholic solution as an oil, and slowly solidifying to an imperfectly crystalline mass. I have not yet been able to obtain a sufficient amount for a thorough examination, but from a preliminary analysis it appears to be a dinitrodibromazoxybenzol.

Royal Laboratory of Berlin, May 10th, 1875.

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* Körner, unpublished research; Kekule's Org. Chem., iii, 77.

† Cahours, Ann. Chem. Pharm., lxxiv, 299; lxix, 236.

‡ Hermann, Deutschen chem. Ges., 1872, 910.

ART. XVII.—*On a fœtal Manatee and Cetacean, with remarks upon the affinities and ancestry of the Sirenia;*[*] by Prof. BURT G. WILDER, of Cornell University.

A fœtal Manatee.—The fœtal Manatee here described was obtained by Professor James Orton[†] at Pebos,[‡] Peru, upon the Marañon, a tributary of the Amazonas.

Detailed measurements are reserved for a more extended article, but the following will be found useful.

	Grams.	Meter.
Present weight,	11·	
Original weight (estimated by comparison with a fœtal pig of nearly the same size, preserved in spirit),	22·	
Present apparent length, vertex to root of tail, (2·3 inches)		·055
Original apparent length (estimated as above), (2·6 inches)		·059
Tip of muzzle to ear		·020
Ear to point opposite anus		·037
Point opposite anus to tip of tail		·028
Real length, as if extended, (3·7 inches)		·085
Tip of muzzle to depression between eyes		·011
Between eyes to vertex		·015
Total length of head		·026
Greatest width of tail		·011

It is not very easy to state the dimensions of this fœtal manatee so as to permit accurate comparison with other fœtal or adult individuals. This is owing less to the distortion of the trunk by lateral pressure in packing for transportation, than to the non-conformity of the axes of head, trunk and tail, which, in the adult, nearly coincide.[‖]

With adult animals a common measurement is from "tip-to-tip;" from the muzzle to the end of the tail. It is evidently inappropriate to measure directly between these points in this or any other young fœtus; and almost equally so to follow the outer curves of the head, trunk and tail. But while we may feel sure that the truth lies between these two extremes, it is not easy to decide upon the points which should be traversed by a straight line representing the total length. The measurements here given may therefore be regarded as only approximately true to nature.

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[*] Abstract of portions of a communication to the Boston Society of Natural History, April 7th, 1875.

[†] Knowing me to be engaged in the dissection of a fœtal dugong (2½ feet long, obtained through Prof. H. A. Ward of Rochester), Prof. Orton very generously transferred the little manatee to the Cornell University.

[‡] Murray (3, 302) states that this species "ascends the rivers Orinoko and Amazon for great distances." Upon Map LI the coloring indicates a westward limit in Brazil at about 65° west longitude. The locality above named is about 72° west longitude and 3° south latitude.

The first number of references indicates the number of the work in the list at the end of this paper; the last the number of the *page;* the middle one, when it occurs, the *volume.*

[‖] More desirable than even the adoption of a simple (metric) system of weights and measures is the acceptance of a uniform method of weighing and measuring animals and their organs. The aphorism that "figures do not lie" is nowhere less true than in zoölogy.

The head is sharply bent upon the chest. The armi (anterior limbs) are likewise pressed flat upon the chest, the left manus overlapping the right. (In the photograph the manus is represented as projecting forward). The tail forms nearly a right angle with the trunk. Upon its ventral border near the tip is a minute median papilla, which does not appear to have been observed in larger specimens; but there is no trace of the notch or depression described by Dr. Murie in both of his specimens (1, 128); neither does there appear the raised border described by the same author (1, 133).

The abdomen is closed but the umbilicus is large, ·004 in diameter. The umbilical cord is not present, but there projects a loop of intestine about ·040 long, together with a delicate membraneous tube which is apparently connected with the intestine and is probably the remains of the yolk sack. (This will be fully described when the dissection is made).

The clitoris is large, ·003,2 in length. By its caudal deflection it covers the genital and anal orifices.

The brain was softened and its parts hardly distinguishable. It will be described hereafter. (The photograph was taken after its removal by a longitudinal incision). The extent of the cavity is approximately indicated by the dotted line.

There is no external ear, and the orifice of the auditory meatus is a minute round hole* with wrinkled borders as in the adult. A pigmentary deposit in the surrounding skin is all that can be seen by the unaided eye. The skull is prominent at this point, as shown in fig. 4.

The upper and lower eyelids are separated by an elliptical opening, the long axis of which is oblique to that of the head. Its length is is ·001,2. The third eyelid, if it exists, does not appear. By blowing between the lids there is revealed a space surrounding the orifice about ·004 inch diameter; how far this represents the size of the globe can only be known by dissection.

The nostrils have the same form as in the adult, but owing to the squareness of the muzzle, they do not appear upon a front view.

***At the time this communication was made the writer thought he had found an anteverted triangular pinna. It proves to exist upon only one side, and has probably been produced artificially. Its position is just behind that of the meatus, and it closely resembles the early stage of the pinna in swine.**

A fœtal Cetacean.—Fig. 6 represents, of natural size, a fœtal cetacean kindly loaned by Mr. Alex. Agassiz, Curator of the Museum of Comparative Zoölogy.

The specimen is labelled *Talcahuano, Chili*, and was given to the late Prof. Agassiz upon the Hassler expedition. The donor, an old whaler, said it was from the "Hump-back whale," (*Megaptera*). But, aside from its small size, the blow-hole is a single transverse aperture as in Delphinidæ; so that unless we assume that a transformation could occur so as to divide this into two holes, longitudinally or obliquely placed, we must regard it as the embryo of a porpoise or dolphin.

	Meter.
Length from vertex to root of tail opposite anus, (2·3 inches)	·055
Tip of muzzle to supposed location of auditory meatus......	·015
Auditory meatus to opposite anus........................	·042
Opposite anus to tip of tail....	·018
Real length as if extended (2·9 inches)	·075

The smallest cetacean fœtus of which I have found record (Gervais, 4,323, pl. XVII) was ·102 (about four inches) long; it is assigned to the common dolphin, *Delphinus delphis*. The general aspect is similar in the two, but Gervais' figure has the upper and lower jaw of equal length, while in the specimen here figured the lower projects about ·001 beyond the upper.

The blow-hole is a transverse aperture ·004 long. Its lips are rounded and tumid, and the posterior has a distinct hinder border. The eyelids are closed.

The tail is narrow and lancet-shaped, with no trace of a terminal notch No trace of dorsal fin is apparent, but as the cuticle of the back is somewhat abraded by friction during transportation, it is not impossible that a rudimentary fin existed as in Gervais' specimen. The vertical crest, mentioned by Wyman (6), exists above and below the tail.

Existing views respecting the affinities of the Sirenia.—At the present day no zoölogist follows Rondeletius (7) in enumerating the manatee and dugong with the fishes, and very few adopt Cuvier's arrangement of them as "Herbivorous Cetacea."

Yet the hiatus between the Ungulata and the adult manatee and dugong is so great as to lead to the general recognition of the latter as a distinct order, Sirenia (Brandt, 2 *fere;* Murray, 3, 196; Owen, 12, ii, 281; Huxley, 13, 387); and three recent authors, by a kind of taxonomic reversion, seem to be again forcibly impressed with the striking outward resemblances of the adult Sirenia to the Cetacea. The late compiler of the Catalogue of Seals and Whales in the British Museum includes the manatee and dugong among the Cetacea; Gray, 15, 62. Hæckel (14, 545, 556) recognizes their affinities with the Ungulata, but makes the Cetacea the descendants of the Sirenia.

The most recent writer concludes an able anatomical description of the manatee with a diagram, in which equal weight seems given to the cetacean and ungulate relations of the Sirenia (Murie 1, 190).

Dr. Murie continues as follows: "Is it (the manatee) a retrograde, dwarfed or undeveloped Elephant? a 'true embryonic type of Pachyderms,' as the elder Agassiz puts it (9). Is it a partially converted Cetos? Is it the reflex of unknown and antedated swarms of mammals of intermediate organization which would fill up the chasms of structural differentiation, yielding lines of demarcation to modern systematists? Such interrogations, to be answered satisfactorily, require a more comprehensive knowledge of the embryology of Pachyderms and Cetacea, a far greater acquaintance with allied fossil forms, a better appreciation of what constitute transitional links, and a further profound investigation into the principles of the doctrine of evolution."

It will be noted that, although Dr. Murie uses the word *retrograde* in his general query respecting the affinities, the idea is not distinctly enunciated. For while embryo Pachyderms and Cetacea are mentioned as likely to throw light upon the problem, those of Sirenia are not alluded to. This is not so strange in view of the fact that the smallest fœtus then known (Daubenton, 22) was about ten inches long (·254) and already unmistakably a manatee.

Affinities of the Sirenia.—The likeness of the fœtal manatee's head to that of some ungulate quadruped is as obvious as is its unlikeness to the head of the fœtal cetacean. The following considerations seem to warrant our attaching more importance to these resemblances than to those features of the trunk and limbs which, in the adult forms, seem to separate the Sirenia from the Ungulata and to unite them with the Cetacea. "In the eyes of most naturalists, the structure of the embryo is even more important for classification than that of the adult. In two or more groups of animals, however much they differ from each other in structure and habits, if they pass through closely similar embryonic stages, we may feel assured that they are all descended from one parent-form and are therefore closely related." (Darwin, 24, 403).

It is to be borne in mind that resemblances may be of two kinds. 1. Those features which are common to the earlier stages of development with all members of larger groups. 2. Those which are peculiar to smaller groups. The former are relatively *negative;* the latter, relatively *positive.*

In the case of the manatee the large tail, the absence of hinder limbs, the pinniform manus, the flexion of the head upon the chest, and the absence of external ear; these characterize a certain stage with all mammalian embryos, and have merely a general significance as indicating a remote common or similar origin within the mammalian class. Nor does the resemblance they may involve with the Cetacea require us to regard the latter as nearly related to the Sirenia.

But all mammalian embryos have, in the earlier stages, a high and rounded head with a short facial region. With an "embryonic type" like the Sirenia we should expect to find in the embryo an especially high and rounded head and a short face. Whereas the height of the head is by no means excessive, and the facial region is greatly prolonged. Now this is what exists in the adult pig, horse, and hippopotamus.

Among human beings, likeness to relatives and to ancestors, more or less remote, is usually recognized in the form of the head, the shape or expression of the features. The same value can be attached to resemblances among animals only in so far as there is community of structure underlying the external resemblance.

It is admitted by nearly all that the anatomical resemblances upon which affinities are recognized are much greater between the Sirenia and certain Ungulata than between either of these groups and the Cetacea.

There would seem to be reason, therefore, for attaching very considerable taxonomic value to the fact that the head and face of this fœtal manatee resemble not those of cetaceans, but those of certain Ungulata; notably hippopotamus, pig and horse.

Probable ancestors of the Sirenia.—In the discussion of this question several matters must be considered.

1. Few now dissent from the doctrine of Darwin, that "descent is the hidden bond of connection which naturalists have been seeking under the term of the natural systems." (24, 403.)

By all zoologists the Sirenia are regarded as an inferior type, whether as an order among Mammals, or a sub-order or family of the Ungulata in general, or of the more restricted groups Pachyderms or Perissodactyla.*

The group is well named by Dana "*Urosthenic aquatic* Herbivores;" (21, 160); and it is a source of gratification to all that that eminent naturalist now admits that the terms above used, with others introduced in his works, may have an actual in place of a purely ideal signification.

It is an almost universally accepted rule that the earlier stages of animals resemble the permanent conditions of *lower* forms. This is stated by Dana as follows: "As a species in development passes through successive stages of progress, relative grade in inferior species may often be determined by comparing their structure with these embryonic stages. * * * As the young of the frog (tadpole) has the tail and form of a salamandrian, therefore the salamanders are higher than the frogs." (20, 592.)

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* I use these terms for convenience and not because I regard them as expressing natural relations.

In accordance with the above rule, and upon the hypothesis of evolution, we should expect the embryo hippopotamus to resemble a manatee, and the embryo manatee to look "very like a whale." But since the young manatee has a head like an adult hippopotamus, we must either reverse the usual opinion as to the relative rank of the aquatic and terrestrial Ungulates, or qualify the rule above stated so as to meet the present case.

This apparent discrepancy between generally accepted views as to the coincidence between rank and stages of development seems to be accounted for by the doctrine of Retrograde metamorphosis, alluded to by Darwin (24) and Hyatt (26) and perhaps by other authors.

The following passages from the "Origin of Species" are especially applicable.

"The embryo in the course of its development generally rises in organization; I use this expression, though I am aware that it is hardly possible to define clearly what is meant by the organization being higher or lower. But no one probably will dispute that the butterfly is higher than the caterpillar. In some cases, however, the maturer animal must be considered as lower in the scale than the larva, as with certain parasitic crustaceans." (24, 396.)

"Recent forms are generally looked upon as being on the whole higher in the scale of organization than ancient forms; * * * this fact is compatible with some forms having retrograded in organization, by having become at the last stage of descent better fitted for changed and degraded habits of life." (24, 426, also 402 and 397.)

"Slight variations generally appear at a not very early period of life and are inherited at a corresponding not early period." (24, 399 and 316.)

"Whatever influence long-continued use or disuse may have had in modifying the parts of animals will chiefly or solely have affected them when mature * * * and the effect thus produced will be transmitted to the offspring at a corresponding mature age." (24, 401.)

"This process, while it leaves the embryo almost unaltered, continually adds, in the course of successive generations, more and more difference to the adult. Thus the embryo comes to be left as a sort of picture, preserved by nature, of the ancient and less modified condition of the animals." (24, 316 and 403.)

In other words, the idea of evolution, whether of individuals or groups, *primarily* involves an increase in complexity and an advance in rank; but it is perfectly compatible with a subsequent retrograde metamorphosis, more or less extensive according to the rank already attained. Using the terms in their literal rather than their usually accepted sense, evolution primarily involves *ascent*, but it is perfectly compatible with *descent.*

Upon the derivative hypothesis all evidences of a parallelism or concomitancy between individual metamorphosis and the evolution of types toward a more perfect condition are equally cogent in favor of the conclusion that the retrograde metamorphosis of an individual indicates that the group to which it belongs is upon the downward rather than the upward path.

Upon the hypothesis of evolution we may regard exceptions to the rule quoted from Dana as to the resemblance between the earlier stages of higher forms and the permanent condition of lower, as strictly in accordance with the more universal law that *the earlier stages of animals resemble their more or less remote ancestors;* if these are lower in rank, as is usually the case, then the commonly accepted rule will hold good; but if higher, as is here suggested of the Sirenia, then that rule would fail, while the more universal one would still be kept.

The idea that existing Sirenia are the result of a retrograde evolution is supported by the very arguments which Brandt urges against the ordinary view of progressive development. He says: "To the supposition that *Halitherium* has given rise to the other and later genera are opposed many considerations. For the *Halitheria*, in addition to the well-developed pelvis provided with acetabula, had rudiments of hinder limbs (femorum), in which respect they approximated the more perfect animals more nearly than do the existing genera; while on the contrary, according to the theory, we should look for a perfection of body gradually evolved and a more complex structure, in *Halicore*, *Manatus* and *Rhytina*, rather than in the *Halitheria.*" (Brandt, 2370.)

If *Deinotherium* * be regarded as a Sirenian with limbs yet more developed then in *Halitherium*, then the series is at least provisionally intelligible.

Of course the above considerations in no way account for the prior existence of the hypothetical stem-form of quadrupeds

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*** By Owen (12, ii, 282); Huxley (13, 431); Haeckel (14, 560); Brandt (2, 190); Gill (23, 90), the Deinotherium is regarded as a Proboscidian; but it is included among the Sirenia by De Blainville and St. Hilaire (Murray, 3, 198), Dana, 20, Murray, 3, 190, and L. Agassiz. According to the placental classification of Huxley and Haeckel, the Proboscidia are in one great group Deciduata, while the Sirenia are in the other, but provisionally, since their placentation is not known. It would seem that osteological comparisons are not as yet conclusive in this case.**

from which the Sirenia may be supposed to have retrograded. Neither do they preclude the idea that they and the other mammals may have been evolved from cetacean forms.

But it must be admitted even by those who may not recognize the ungulate affinities of the Sirenia, that the comparison of these two embryos does not materially lessen the gap between the Cetacea and the other Mammals, upon which Murray has so strongly insisted. 3, 55, 196 and 203.*

As to the zoölogical status of the Sirenia, if the views above advanced are correct, there would seem to be no more ground for their removal from the Ungulata than for the separation of the Pinnipedia as an order apart from the Carnivora. Nor would the comparison be invalidated by the view that the seals may be upon the upward rather than the downward path.

The limits of this prėliminary paper will not allow the discussion of the relations of the Sirenia with special ungulate families and genera, or of the relative position of the sirenian genera.†

But there is one point of resemblance between the Sirenia and the Proboscidia upon which, it seems to me, undue stress is liable to be placed, namely: that in both groups the mammary glands are pectoral or axillary, while in the hippopotamus they are inguinal.

The consideration of the variations as to number and position of these organs in other groups shows, however, that in recent times they have never been regarded as of sufficient taxonomic value for the determination of ordinal or even family affinities. (For instances, see Owen, 12, iii, 775–780.)

It may at first seem strange that there are no traces of hinder limbs in this fœtus, and that the front limbs are not more like the legs of its supposed quadrupedal ancestors.

It is by no means *impossible* that an embryo just forming would present rudimentary hinder limbs in accordance with the usual vertebrate type.

As to the manus, it is to be borne in mind that the vast majority of existing vertebrates ‡ have anterior limbs which vary but slightly from the pad-like form which they present in the embryos, and which may be supposed to have prevailed in past times. The quadrupedal pattern is comparatively recent, and has affected so few mammals, especially such as may be regarded as in the line of sirenian descent, that we may regard its influence as almost inappreciable as compared with the other.

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* With the Cetacea it is said that "the apex of the heart is sometimes indented," (Owen, 12, iii, 521). But we need further information upon this point and also respecting the statement of Gratiolet, (16,) that a like condition prevails in the new-born hippopotamus.

† I am inclined to think that a careful study of embryos and brains will be more satisfactory than even such exhaustive osteological comparisons as those of Brandt.

‡ Nearly all fishes, many batrachians, some reptiles, all birds, and a very large number of mammals.

Further discussion of the subject is deferred until the fœtal manatee has been dissected,* and until the writer is able to prepare a longer paper in which the various questions involved may be more fully presented. It is to be hoped that no opportunity will be lost for securing still earlier sirenian embryos and for preserving the membranes.

SUMMARY.

1. The specimens here described are probably the smallest fœtal sirenian and cetacean upon record; measuring, if extended, ·085 (3·7 inches), and 0·75 (2·9 inches), respectively.

2. The head of the manatee is strongly flexed upon the chest, and the tail forms a right angle with the trunk.

3. The general aspect of the head and face of the manatee is ungulate rather than cetacean.

7. To this extent the embryo of a lower form resembles the adult of a higher.

5. This, while contrary to the usually accepted rule, may be really an exemplification of a more comprehensive law; namely, that the *young of animals resemble their ancestors.*

6. This retrograde metamorphosis of the manatee points to a like retrograde evolution of the Sirenia from prior ungulate forms.

7. This idea is confirmed by what is known of the geological succession of sirenian forms.

8. The determination of the affinities of the Sirenia, is likely to be accomplished by the study of brains and embryos rather than by minute osteological comparisons.

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* This youngest specimen ought to throw light upon the development of the compound stomach and of the bifid heart; and may solve the question respecting the number of cervical vertebræ, upon which present opinions are wholly contradictory (Murie, 1, 137).

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EXPLANATION OF FIGURES.*

Fig. 1.—Partly grown male Manatee, $\frac{1}{18}$ of natural length. Reduced copy of Murie's figure. The eye and ear appear as black spots; *a*, anus; I, M, A, the nails of the index, medius, and annularis digits.

Fig. 2.—Fœtal Manatee, natural size. *n*, nostril; *a*, ancon (elbow); *c*, carpus.

Fig. 3.—Head of fœtal Manatee, enlarged two diametors; the muzzle being raised a little so as to expose the lower lip. The dotted line indicates the extent of the cranial cavity. The external meatus is represented by a black spot.

Fig. 4.—Head of fœtal manatee, seen from above and in front; natural size.

Fig. 5.—Tail of fœtal Manatee, seen from the ventral side; natural size. *cl*, clitoris turned toward the right; *v*, generative opening; *a*, anus; *cp*, caudal papilla.

Fig. 6.—Fœtal cetacean, natural size. *n*, nostril; *uc*, umbilicus; *cl*, clitoris; *a*, anus. The smaller figure represents the lance-shaped tail from the ventral surface.

Fig. 7.—Head of fœtal cetacean, natural size, from above; *n*, nostril.

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* The figures, excepting fig. 1, were drawn from nature and from photographs and engraved by Mr. Philip Barnard.

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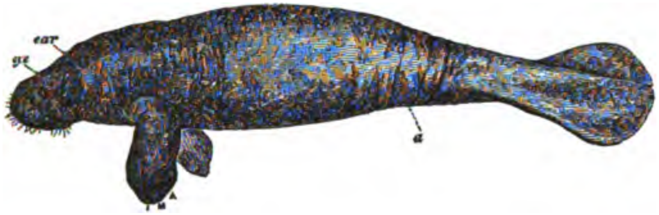

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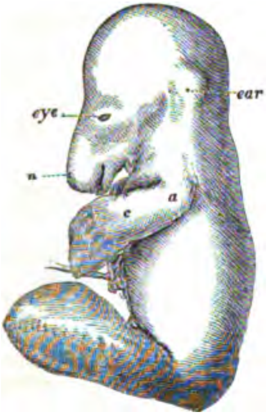

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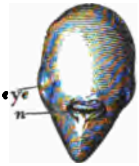

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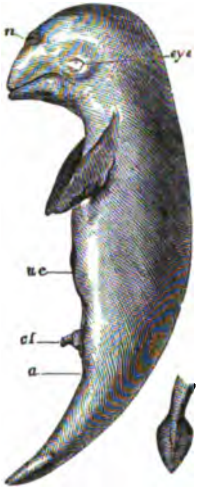

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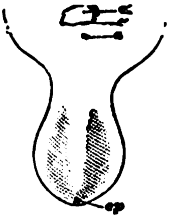

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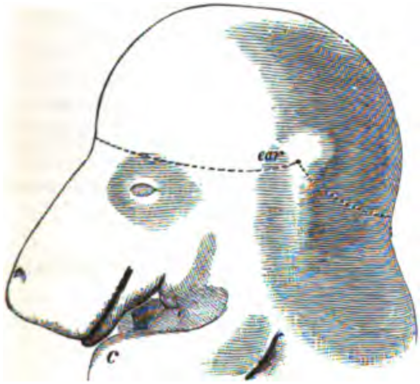

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ART. XVIII.—*On Tidal Waves and Currents along portions of the Atlantic Coast of the United States;*[*] by J. E. HILGARD, Assistant in the Coast Survey, Washington, D. C.

1. MOVEMENT OF TIDAL WAVES.

EARTHQUAKE waves originating with an impulse at one definite point, and propagated freely through the ocean in every direction with a velocity depending upon the square root of the depth of the sea, may serve as good illustrations of the manner in which tides are propagated through sounds, bays and rivers. The following table gives the rate of motion for different depths:

Depth in feet.......	10	Miles per hour.......	12·2
" "	60	" "	30·0
" "	100	" "	38·7
" "	1,000	" "	122·3
" "	6,000	" "	299·5

The movement of the ocean designated by the name of tide-wave, does not partake of the nature of a wave in the common acceptation of the term, but is rather to be conceived as a general movement of the water toward a point under the attracting body, and again away from it. Its periodicity is strictly dependent upon that of the attracting body. The velocity of the movement is about 1,000 miles per hour on the equator; it extends to the bottom of the ocean, the depth of which is inconsiderable compared with the radius of the earth. It is not attended by a sensible elevation of the water in mid-ocean; and, in this respect, the characteristic of what we call a wave is absent. The movement may be likened to that of an impulse given to a very long rigid bar, as of iron. In this case, a sensible time will be required for the transmission of the impulse from one end to the other, and during its transmission the particles will successively approach to each other, by which an infinitesimal elevation and subsidence, after the manner of a wave, will be produced. In the same way the transmission of the movement through the incompressible water of the sea, is attended with an infinitesimal elevation and recession; but when the movement reaches shallow water, in approaching the shores, the horizontal motion is partly translated into vertical motion upon the sloping bottom; and it is thus that the tides attain sensible vertical height. Now, where a bay or indentation of the coast presents itself, opening favorably to the tide-wave thus developed, and decreases in width from its entrance toward its head, the tide rises higher from the mouth upward. This is due to the concentration of the wave by the approach of the shores, and to the gradual shoaling of the bottom.

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[*] Extract from a lecture by Mr. Hilgard before the American Institute, January 27th, 1871.

This effect is strikingly illustrated by a generalization of the heights of the tides on the Atlantic coast of the United States. That coast presents, in its general outline, three large bays: the Great Southern, from Cape Florida to Cape Hatteras; the Great Middle, from Cape Hatteras to Nantucket: and the Great Eastern, from Nantucket to Cape Sable, now known as the Gulf of Maine. It will be seen that the tide-wave arrives at about the same time at the headlands, Cape Florida, Cape Hatteras, Nantucket and Cape Sable, and that at those points the height is inconsiderable compared with the rise at the head of the several bays. Thus, at Cape Florida the mean rise and fall is only one and one-half of a foot; at Hatteras, but two feet; while at the intermediate entrance to Savannah it reaches seven feet, declining in height toward both capes. Again, at the head of the Middle bay, in New York harbor, it reaches five feet, while on the southeast side of Nantucket Island it is little over one foot. The configuration of the Eastern bay is less regular, and the correspondence of heights is not so obvious. The recess of Massachusetts bay is well marked, the increase in height reaching ten feet at Boston and Plymouth. Rolling on eastward along the coast of Maine it constantly increases. But the most striking effect of the convergence of shores is exhibited in the bay of Fundy. At St. Johns the mean height of the tide is nineteen feet, and at Sackville, in Cumberland Basin, thirty-six feet, attaining to fifty feet and more at spring-tides.

When the wave leaves the open sea its front slope and rear slope are equal in length and similar in form, but as it advances into a narrow channel, bay or river, its front slope becomes short and steep, and its rear slope becomes long and less inclined. Hence arises the fact that at a station near the sea the time occupied by the rise is equal to that occupied by the descent; but at a station more removed from the sea the rise occupies a shorter time than the descent. Thus, in Delaware bay and river, we have the following relations of the duration and height of rise and fall.

STATION.	Mean rise and fall.	Luni-tida interval.	MEAN DURATION OF	
			Flood tide.	Ebb tide.
	Feet.	h. m.	h. m	h. m.
Delaware breakwater	3·5	8· 0	6·13	6· 3
Egg Island light	6·	9· 4	5·56	6·30
Newcastle	6·5	11·53	5·24	7· 2
Philadelphia	6·	13·44	4·52	7·34

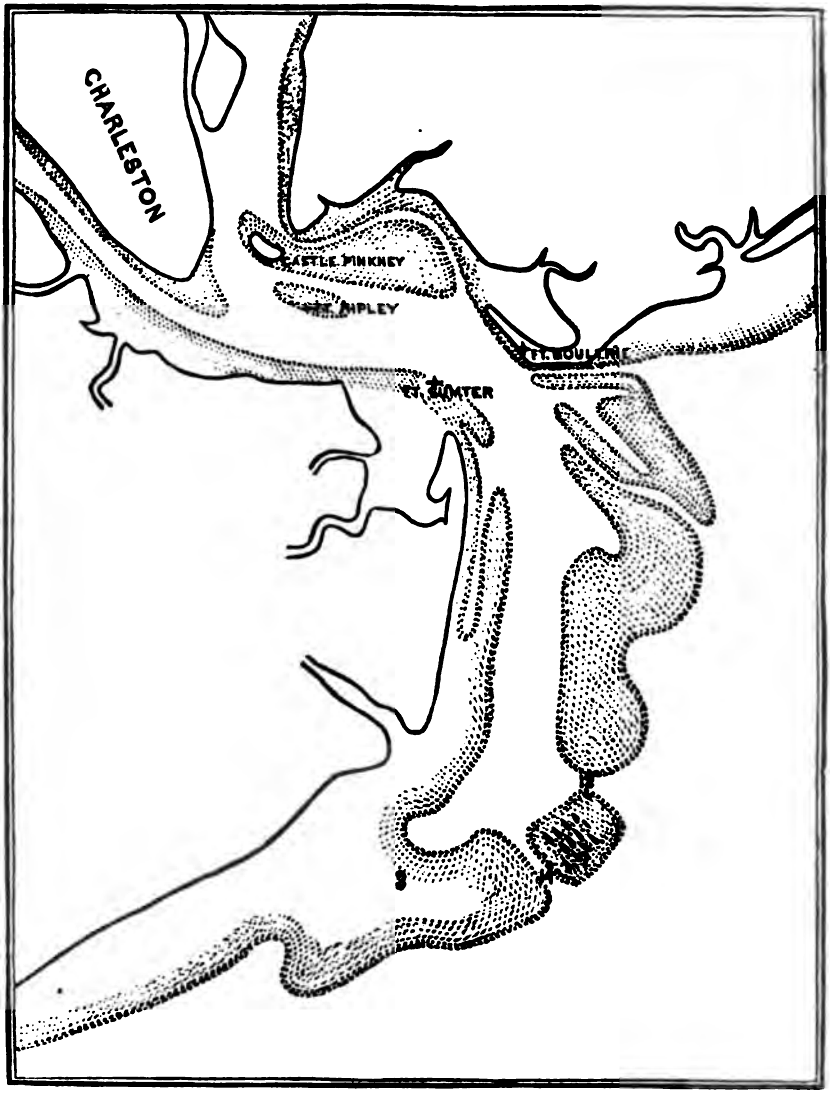
An examination of this table will show, besides the marked increase in the height of the tide due to the contraction of the shores from the capes up to Newcastle, a subsequent loss from friction in a narrow channel of nearly uniform character, and correspondingly a rapid propagation of the tide-wave through the deep water of the bay, and a comparatively slow movement along the narrow channel of the river. At the mouth of the bay the duration of the flood tide is equal to that of the ebb, while at Philadelphia it is less by two hours forty-two minutes. When the tide is very large compared with the depth of water, this inequality becomes very great; thus, in the Severn river at Newnham, above Bristol, England, the whole rise of eighteen feet takes place in one and a half hours, while the fall occupies ten hours.

2. TIDAL CURRENTS.

The agency of tidal currents in producing changes in the entrances of bays and harbors, is a subject of the first importance to commerce and navigation, and has received full attention in the prosecution of the American Coast Survey. The laws according to which the changes takes place require to be studied by long-continued observation, and when the change is for the worse, the means of counteracting it must be pointed out.

Since on the average the same amount of water moves inward and outward with the flood and ebb tides, we might readily suppose that the same amount of material is transported either way, and that no important change would take place in the configuration of the bottom. But the operation of the flood stream is very different from that of the ebb stream. We have, as a general feature, an interior basin of some extent, communicating with the sea by a comparatively narrow passage. The flood stream, therefore, running with considerable velocity through this channel, will, as it enters the basin, spread out and become slow, depositing the sand and mud it is charged with, and making extensive flats or shoals opposite the entrance. The ebb stream runs slowly over the flats from all directions toward the opening, without removing much of the deposit, and gradually concentrates in definite narrow channels, which it scoops out, and the depth of which will depend in a great degree on the proportion of the area of the basin to the outlet, or, in other terms, on the difference of level which will be reached during the ebb between the basin and the ocean, which determines the greatest velocity and transporting power reached by the ebb stream.

On the bars of most of the sand-barred harbors on our southern coast, the place and direction of the channel are frequently changed during violent storms, when the direction of the waves happens to be oblique to that of the channel; or, when the sea runs directly upon the channel, the depth of water



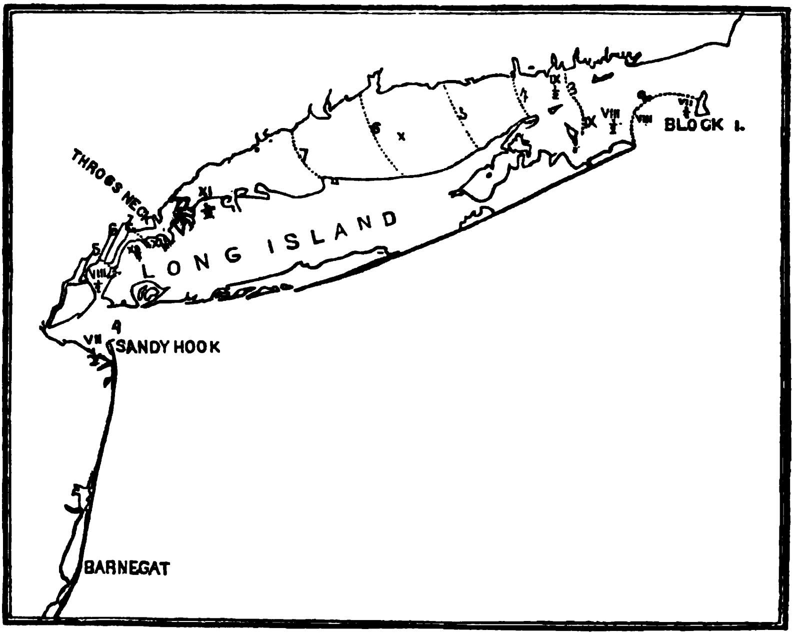
EFFECT OF SINKING STONE-FLEET ON CHARLESTON BAR.

may be considerably diminished, for the time being, by the sand rolled up by the waves. But in all these cases it is found that the normal depth is speedily restored by the scour of the ebb tide, which depends upon the unchanged factors of area and form of basin, height of tide, and character of the material forming the bar.

Charleston bar.—An interesting instance of this maintenance of the depth of channels from a determinate tidal basin is furnished by the effects of the obstructions placed in the channel over Charleston bar during the war of the rebellion. On the accompanying diagram is seen the "stone fleet" sunk in the main channel, which at that time had twelve feet of water at low tide where the figure 7 indicates the present depth. There was, moreover, another channel making out more to the southward, with nine feet of water where the figure 3 indicates the present depth. The vessels were placed checkerwise in such a manner as to impede navigation while interfering least with the discharge of the water. The effect, nevertheless, was the formation of a shoal in a short time, and the scouring out of two channels, one on each side of the obstructions, through which twelve and fourteen feet can now be carried at low water. The increased water-way thus given to the ebb tide caused it to abandon the old nine foot channel on the less direct course to deep water. We have here the total obstruction of a channel, which was of considerable importance to the southward trade, by new conditions introduced at a point four miles distant from where the effect was produced; and we are warned how carefully all the conditions of the hydraulic system of a harbor must be investigated before undertaking to make any change in its natural conditions, lest totally unlooked for results be produced at points not taken into consideration.

New York Harbor and Long Island Sound.—Approaching now more closely to the consideration of the tidal conditions in New York harbor, we will examine the progress of the tide-wave through Long Island Sound from the eastward to its meeting with that entering New York bay at Sandy Hook.

We see from the following diagram that about seven and a half hours after the transit of the moon high water has advanced just within Block Island with an elevation of two feet, and, at the same time, has passed Sandy Hook with an elevation of four and a half feet. Traversing the sound at a rate indicated by the Roman figures, with increasing heights indicated by the Arabic numerals, it reaches Sands' Point eleven and a half hours after the transit of the moon with a height of seven and seven-tenths feet. The observed time of transmission from the Race to Sands' Point is two hours and one minute, and the time computed from the depths, according to the law developed by Airy, is two hours fourteen minutes; a very good approximation when we consider the irregularities in the configuration of the Sound which could not be taken into account. Advancing still farther, the height somewhat declines in consequence of the changes of direction in the channel and its shallowness.



At Hell Gate this tide-wave is met by that which had entered at Sandy Hook, and had advanced more slowly owing to the narrowness and intricacies of the channel, especially in the East river.

These two tides which meet and overlap each other at Hell Gate, differing from each other in times and heights, cause contrasts of water elevations between the sound and harbor which call into existence the violent currents that traverse the East River. The conditions of the tidal circulation through Hell Gate are such that if there were a partition across it, the water would sometimes stand nearly five feet higher, and at other times five feet lower, on one side than on the other. In the actual case of the superposition or compounding of the two tides, the difference of level existing at any time, is, of course, much less, but the difference of one foot is often observed within the space of 100 feet in the most contracted portion of Hell Gate off Hallett's Point. Referring, now, more particularly to the diagram representing New York bay and harbor (p. 125), it is important to observe that the entrance from Long Island Sound is a natural depression or arm of the sea which is not changed by the forces now in operation. The tidal currents which flow through it do not change the channel, but are obliged to follow it in its tortuous course. The Sandy Hook entrance, on the contrary, is characterized by a cordon of sands extending from Sandy Hook to Coney Island, intersected by channels, which are maintained against the action of the sea, that tends to fill them up, by the scour of the ebbtide from the tidal basin of New York harbor.

Unlike Hell Gate passage, where permanence is the leading characteristic, the bar and channels of Sandy Hook have undergone continual changes within the brief period of our history. The advance of Sandy Hook upon the main ship channel is among the notable and important instances of the effect of tidal currents. Within a century it has increased a mile and a quarter. In the place where the beacon on the end of the hook now stands there were forty feet of water fifteen years before it was built. The cause of this growth is a remarkable northwardly current along both shores of the Hook, running both during the flood and the ebb tides with varying rates, and resulting from those tides directly and indirectly.

The best water over the bar is about two miles east of Sandy Hook light, in a direct line with the Swash channel, which is the second opening, shown on the sketch, above the Hook ; the shoal lying between the main or Hook channel and the Swash channel being known as Flynn's knoll. The greatest depth over the bar is twenty-two feet at mean low water; and very nearly the same depth can now be carried through the Swash channel, which formerly was three feet shallower, but has deepened since the cross section between the Hook and Flynn's knoll has been diminished by one-third its area by the growth of the Hook. This relative change in the capacity of the channels has not, however, affected the depth on the outer bar, which, according to the principles above laid down, is dependent mainly upon the area of the tidal basin within.

The depth of twenty-two feet at mean low water, which is now maintained at the entrance, through the sands constantly thrown up by the waves of the sea, may be considered as depending upon the following elements:

1st. The large basin between Sandy Hook and Staten Island, including Raritan bay, which furnishes more than one-half of the whole ebb scour.

2d. What is called the Upper bay, including the Jersey flats and Newark bay.

3d. The North river, perhaps as far as Dobb's Ferry, maintaining the head of the ebb current, although not directly taking part in the outflow ; and,

4th. A portion of the Sound tide, which flows in through Hell Gate.

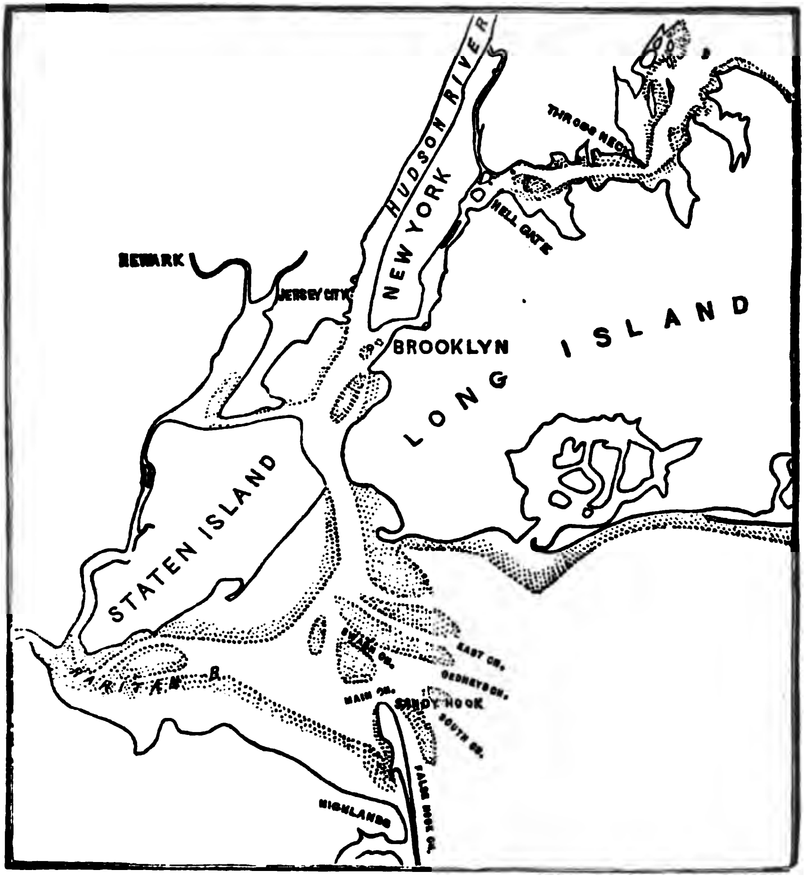
The proportion of the three first divisions in producing the depth of channel, may be approximately estimated by a comparison of the areas and distances from the bar. In order to maintain the depth which we now have, it is important that the area of the tidal basin should not be encroached upon. In proportion as that is diminished the depth of the channels will decrease.

The flats, just bare at low water, but covered at high tide, form as important a part as any other portion, for it is obvious that it is only the volume of water contained between the planes of low and high water, the "tide prism," that does the work in scouring the channels. The water on the flats is especially useful by retarding the outflow, thus allowing a greater difference of level to be reached between the basin and the ocean.

When we yield to the demands of commerce any portion of the tidal territory, to be used for its wharves and docks, we must do so with full cognizance of the sacrifice we are about to make in the depth of water over the bar; and in order to form any well-founded judgment in regard to the effect of such encroachments, it is necessary to be in possession of the fullest knowledge of all the physical facts involved in the problem, and no measure of encroachment should be determined upon except in pursuance of the advice of scientific experts.

A proposition, frequently mooted by men of enterprise, and resisted by those interested in the welfare of the city of New York, is the occupation of the Jersey flats, from Paulus Hook to Robbins Reef, for docks and wharves. Without expressing any opinion as to the relative value of the gain of accommodation for shipping and the loss of depth in the channel, I venture to say that the withdrawal of that area from the domain of the tide would occasion a loss of not less than one foot in the depth of the bar off Sandy Hook, and certainly not more than two feet.

The part which the fourth division in our classification of the basin of New York, that of the East river and Hell Gate passage, plays in the outflow of the ebb-tide through the Sandy Hook channels depends less upon the area involved than upon the difference in point of time and height of tide in Hell Gate already adverted to. The westerly current, usually called the ebb stream since it falls in with the ebb stream of New York harbor, taking place when the sound tide is highest, starts from a level of three and half feet higher than the easterly, and thus a much larger amount of water flows out through the Sandy Hook channels than through the narrows at Throg's Neck. It is apparent, then, that this portion of the ebb stream re-enforcing as it does the ebb stream of the harbor proper, at the most favorable times, performs a most important part in maintaining the channels through the Sandy Hook bar. It may be estimated that the closing of Hell Gate would cause the loss of certainly not less than three feet in the depth of those channels.



ENTRANCE TO NEW YORK HARBOR.

From what has been said with regard to the meeting of the tides in Hell Gate, it will be seen that the violent currents experienced in that locality are due to causes beyond our control. The dangers to navigation arising from these currents, however, by their setting vessels upon the rocks and reefs, may, in a great measure be done away with by the removal of the obstructions, in which work considerable progress has already been made. The removal of the reef at Hallett's Point, the work upon which is now in progress, will doubtless, in a great degree, do away with the eddies and under-currents produced by the sharp turn which the channel now takes at that point. It is not improbable that the successful removal of those obstructions will yet cause the sound entrance to be used in preference to the other by the fleets plying between European ports and the great commercial metropolis of America.

NOTE.—The lecturer desires to express his indebtedness to the discussions of tides and currents by Prof. A. D. Bache and H. Mitchell, published in the reports of the U. S. Coast Survey.

ART. XIX.—*On some of the Ancient Glaciers of the Sierra Nevada;* by JOSEPH LECONTE, Professor of Geology in the University of California.

LAST summer I had again an opportunity of examining the pathways of some of the ancient glaciers of the Sierra. It will be remembered, by those interested in this subject, that two years ago I published a paper with the above title.* One of the grandest of the glaciers there mentioned was one which I called the *Lake Valley Glacier.* Taking its rise in snow fountains among the high peaks in the neighborhood of Silver Mountain, this great glacier flowed northward down Lake Valley, and, gathering tributaries from the summit ridges on either side of the valley, but especially from the higher western summits, it filled the basin of Lake Tahoe, forming a great "mer de glace," 50 miles long, 15 miles wide, and at least 2,000 feet deep, and finally escaped northeastward to the plains. The outlets of this great "mer de glace" are yet imperfectly known. A part of the ice certainly escaped by Truckee Cañon (the present outlet of the lake); a part probably went over the northeastern margin of the basin. My studies during the summer were confined to some of the larger tributaries of this great glacier.

Truckee Cañon and Donner Lake Glaciers.—I have said that one of the outlets of the great "mer de glace" was by the Truckee River Cañon. The stage road to Lake Tahoe runs in this cañon for fifteen miles. In most parts of the cañon the rocks are volcanic and crumbling, and therefore ill adapted to retain glacial marks; yet in some places where the rock is harder these marks are unmistakable. On my way to and from Lake Tahoe, I observed that the Truckee Cañon glacier was joined at the town of Truckee by a short but powerful tributary, which, taking its rise in an immense rocky amphitheatre surrounding the head of Donner Lake, flowed eastward. Donner Lake, which occupies the lower portion of this amphitheatre, was evidently formed by the down-flowing of the ice from the steep slopes of the upper portion near the *summit.* The stage road from Truckee to the summit runs along the base of a *moraine* close by the margin of the lake on one side, while on the other side, along the apparently almost perpendicular rocky face of the amphitheatre, 1,000 feet above the surface of the lake, the Central Pacific Railroad winds its fearful way to the same place. In the upper portion of this amphitheatre large patches of snow still remain unmelted during the summer.

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* This Journal, III, v, 125. Proc. Acad. Sci. Calif., IV (part 5), 259.

My examination of these two glaciers, however, was very cursory. I hasten on, therefore, to others which I traced more carefully.

As already stated in my former paper, Lake Tahoe lies countersunk on the very top of the Sierra. This great range is here divided into two summit ridges, between which lies a trough 50 miles long, 20 miles wide, and 3,000–3,500 feet deep. This trough is Lake Valley. Its lower half is filled with the waters of Lake Tahoe. The area of this lake is about 250 square miles, its depth 1,640 feet, and its altitude 6,200 feet. It is certain that during the fullness of Glacial times this trough was a great "mer de glace," receiving tributaries from all directions except the north. But as the Glacial period waned—as the great "mer de glace" dwindled and melted away, and the lake basin became occupied by water instead, the tributaries still remained as separate glaciers flowing into the lake. The tracks of these lingering smaller glaciers are far more easily traced and their records more easily read, than are those of the greater but more ancient glacier of which they were once but the tributaries.

Of the two summit ridges mentioned above the western is the higher. It bears the most snow *now*, and in glacial times gave origin to the grandest glaciers. Again: the peaks on both these summits rise higher and higher as we go toward the upper or southern end of the lake. Hence the largest glaciers ran into the lake at its *southwestern* end. And, since the mountain slopes here are toward the northeast and therefore the shadiest and coolest, here also the glaciers have had the greatest vitality and lived the longest, and have, therefore, left the plainest record. Doubtless, careful examination would discover the pathways of glaciers running into the lake from the eastern summit also; but I failed to detect any very clear traces of such, either on the eastern or on the northern portion of the western side of the lake; while between the southwestern end and Sugar Pine Point, a distance of only eight or ten miles, I saw distinctly the pathways of five or six. North of Sugar Pine Point there are also several. *They are all marked by moraine ridges running down from the summits and projecting as points into the lake.* The pathways of three of these glaciers I studied somewhat carefully, and after a few preliminary remarks, will describe in some detail.

Mountains are the culminating points of the scenic grandeur and beauty of the earth. They are so, because they are also the culminating points of all geological agencies—igneous agencies in mountain *formation*, aqueous agencies in mountain *sculpture*. Now, I have already said that the mountain peaks which stand above the lake on every side are highest at the southwestern end, where they rise to the altitude of 3,000 feet above the lake surface, or between 9,000 and 10,000 feet above the sea. Here, therefore, ran in the greatest glaciers; here we find the profoundest glacial sculpturings; and here also are clustered all the finest beauties of this the most beautiful of mountain lakes. I need only name Mt. Tallac, Fallen Leaf Lake, Cascade Lake, and Emerald Bay, all within three or four miles of each other and of the Tallac House. These three exquisite little lakes (for Emerald Bay is also almost a lake), nestled closely against the loftiest peaks of the western summit ridge, are all perfect examples of glacial lakes.

South of Lake Tahoe, Lake Valley extends for fifteen miles as a plain, gently rising southward. At its lower end it is but a few feet above the lake surface, covered with glacial drift modified by water, and diversified, especially on its western side, by débris ridges, the moraines of glaciers which continued to flow into the valley or into the lake long after the main glacier, of which they were once tributaries, had dried up. On approaching the south end of the lake by steamer, I had observed these long ridges, divined their meaning, and determined on a closer acquaintance. While staying at the Tallac House I repeatedly visited them and explored the cañons down which their materials were brought. I proceed to describe them.

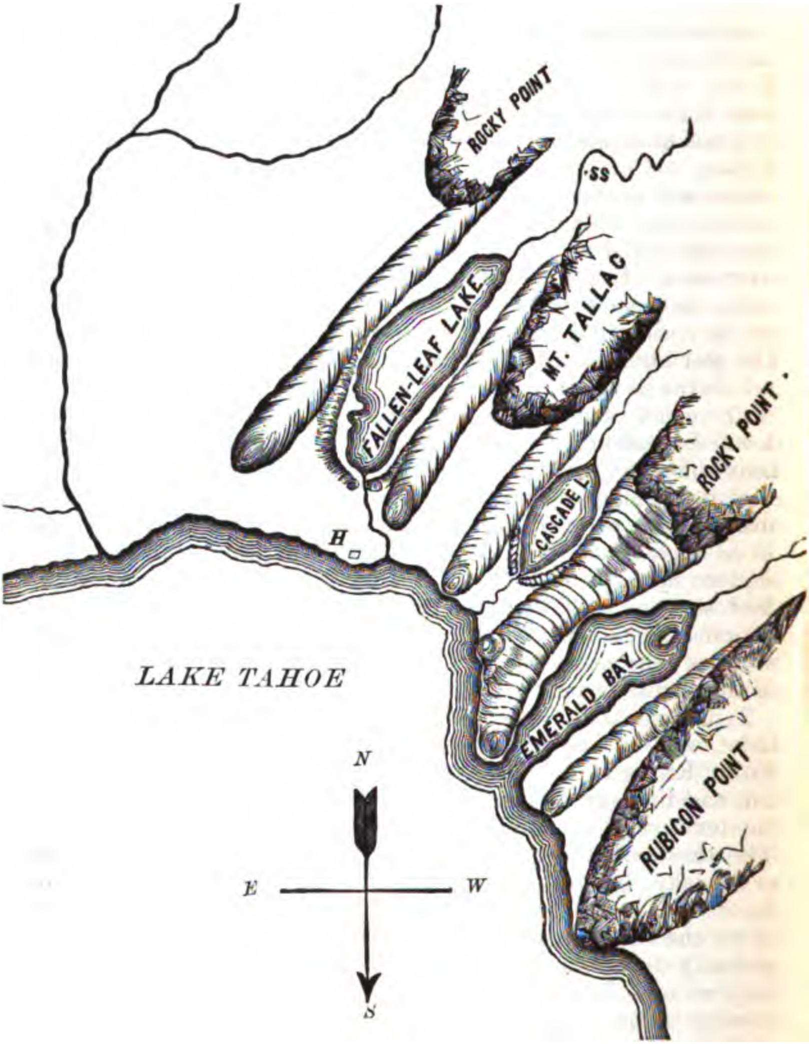
Fallen Leaf Lake Glacier.—Fallen Leaf Lake (see map, p. 130) lies on the plain of Lake Valley, about one and a half miles from Lake Tahoe, its surface but a few feet above the level of the latter lake, but its bottom far, probably several hundred feet, below that level. It is about three to three and one-half miles long and one and one-fourth miles wide. From its upper end runs a cañon bordered on either side by the highest peaks in this region. The rocky walls of this cañon terminate on the east side at the head of the lake, but on the west side, a little farther down. The lake is bordered on each side by an admirably marked débris ridge (moraine) three hundred feet high, four miles long, and one and one-half to two miles apart. These moraines may be traced back to the termination of the rocky ridges which bound the cañon. On one side the moraine lies wholly on the plain; on the other side its upper part lies against the slope of Mt. Tallac. Near the lower end of the lake a somewhat obscure branch ridge comes off from each main ridge, and curving around it forms an imperfect terminal moraine through which the outlet of the lake breaks its way.

On ascending the cañon the glaciation is very conspicuous, and becomes more and more beautiful at every step. From Soda Springs (map, *ss*) upward it is the most perfect I have ever seen. In some places the whole rocky bottom of the cañon, for many acres in extent, is smooth and polished and gently undulating, like the surface of a glassy but billowy sea. The glaciation is distinct also up the sides of the cañon 1,000 feet above its floor.

There can be no doubt, therefore, that a glacier once came down this cañon filling it 1,000 feet deep, scooped out Fallen Leaf Lake just where it struck the plain and changed its angle of slope, and pushed its snout four miles out on the level plain, nearly to the present shores of Lake Tahoe, dropping its débris on either side and thus forming a bed for itself. In its subsequent retreat it seems to have rested its snout some time at the lower end of Fallen Leaf Lake, and accumulated there an imperfect terminal moraine. The outlines of this little lake with its bordering moraines are shown in the diagram-map on the following page.

2. *Cascade Lake Glacier.*—Cascade Lake, like Fallen Leaf Lake, is about one and one-half miles from Lake Tahoe, but, unlike Fallen Leaf Lake, its discharge creek has considerable fall, and the lake surface is, therefore, probably 100 feet above the level of the greater lake. On either side of this creek, from the very border of Lake Tahoe, runs a moraine ridge up to the lake, and thence close along each side of the lake up to the rocky points which terminate the true mountain cañon above the head of the lake. I have never anywhere seen more perfectly defined moraines. I climbed over the larger western moraine and found that it is partly merged into the eastern moraine of Emerald Bay to form a medial at least 300 feet high, and of great breadth (see map). From the surface of the little lake the curving branches of the main moraine, meeting below the lake to form a terminal moraine, are very distinct. At the head of the lake there is a perpendicular cliff over which the river precipitates itself, forming a very pretty cascade of 100 feet or more. On ascending the cañon above the head of the lake,

for several miles, I found, everywhere, over the lip of the precipice, over the whole floor of the cañon, and up the sides 1,000 feet or more, the most perfect glaciation.



There cannot, therefore, be the slightest doubt that this also is the pathway of a glacier which once ran into Lake Tahoe. After coming down its steep rocky bed, this glacier precipitated itself over the cliff, scooped out the lake at its foot, and then ran on until it bathed its snout in the waters of Lake Tahoe, and probably formed icebergs there. In its subsequent retreat it seems to have dropped more débris in its path and formed a more perfect terminal moraine than did Fallen Leaf Lake Glacier.

Emerald Bay Glacier.—All that I have said of Fallen Leaf Lake and Cascade Lake apply, almost word for word, to Emerald Bay. This beautiful bay, almost a lake, has also been formed by a glacier. It also is bounded on either side by moraines, which run down to and even project into Lake Tahoe, and may be traced up to the rocky points which form the mouth of the cañon at the head of the bay. Its eastern moraine, as already stated, is partly merged into the western moraine of Cascade Lake, to form a huge medial moraine. Its western moraine lies partly against a rocky ridge which runs down to Lake Tahoe to form Rubicon Point. At the head of the bay, as at the head of Cascade Lake, there is a cliff about 100 feet high, over which the river precipitates itself and forms a beautiful cascade. Over the lip of this cliff, and in the bed of the cañon above, and up the sides of the cliff-like walls, 1,000 feet or more, the most perfect glaciation is found. The only difference between this glacier and the two preceding is, that it ran more deeply into the main lake and the deposits dropped in its retreat did not rise high enough to cut off its little rock basin from that lake, but exists now only as a *shallow bar* at the mouth of the bay. This bar consists of *true moraine matter*, i. e., intermingled boulders and sand, which may be examined through the exquisitely transparent water almost as perfectly as if no water were present. Some of the boulders are of large size.

All that I have described separately and in detail, and much more, may be taken in at one view from the top of Mt. Tallac. From this peak nearly the whole course of these three glaciers, their fountain amphitheatres, their cañon beds, and their lakes enclosed between their moraine arms, may be seen at once. The view from this peak is certainly one of the finest I have ever seen. Less grand and diversified in mountain forms than many from peaks above the Yosemite, it has the added beauty of extensive water surface, and the added interest of several glacial pathways in a limited space. The observer sits on the very edge of the fountain amphitheatres still holding large masses of snow; immediately below, almost at his feet, lie glistening, gem-like, in dark rocky setting, the three exquisite little lakes: on either side of these, embracing and protecting them, stretch out the moraine arms, reaching toward and directing the eye to the great lake, which lies, map-like, with all its sinuous outlines perfectly distinct, even to its extreme northern end, twenty-five to thirty miles away. As the eye sweeps again up the cañon-beds, little lakes, glacier scooped rock basins, filled with ice-cold water, flash in the sunlight on every side. Twelve or fifteen of these may be seen.

From appropriate positions on the surface of Lake Tahoe, also, all the moraine ridges are beautifully seen at once, but the glacial lakes and the cañon beds, of course, cannot be seen. I have attempted, in the rough sketch accompanying this paper, to express the combined results of observations from many points. The outlines of the great and small lakes are accurate, as these have been taken from reliable maps. Also the general position of the rocky points, and the moraine ridges, are tolerably correct. But, otherwise, the sketch is intended as an illustrative diagram rather than a topographical map. The view is supposed to be taken from an elevated position above the lake surface, looking southward.

There are several questions of a general nature suggested by my examination of these three glacial pathways, which I have thought best to consider separately.

a. Evidences of the existence of the Great Lake Valley Glacier.—In my former paper I have already given some evidence of the former existence of this glacier in the glacial forms detectable in the upper part of this valley. I will now give some additional evidence, gathered last summer.

On the south shore of Lake Tahoe, and especially at the northern or lower end of Fallen Leaf Lake, I found many pebbles and some large boulders of a beautiful striped agate-like slate. The stripes consisted of alternate bands of black and translucent white, the latter weathering into milk-white, or yellowish, or reddish. It was perfectly evident that these fragments were brought down from the cañon above Fallen Leaf Lake. On ascending this cañon I easily found the parent rock of these pebbles and boulders. It is a powerful outcropping ledge of beautifully striped siliceous slate, full of fissures and joints, and easily broken into blocks of all sizes, crossing the cañon about a half mile above the lake. This rock is so peculiar and so easily identified that its fragments become an admirable index of the extent of the glacial transportation. I have, myself, traced these pebbles only a little way along the western shores of the great lake, as my observations were principally confined to this part; but I learn from my brother, Professor John LeConte, and from Mr. John Muir, both of whom have examined the pebbles I brought home, that precisely similar fragments are found in great abundance all along the western shore from Sugar Pine Point northward, and especially on the extreme northwestern shore nearly thirty miles from their source. I have visited the eastern shore of the lake somewhat more extensively than the western, and nowhere did I see similar pebbles. Mr. Muir, who has walked around the lake, tells me that they do not occur on the eastern shore. We have, then, in the distribution of these pebbles, demonstrative evidence of the fact that Fallen Leaf Lake glacier was once a tributary of a much greater glacier which filled Lake Tahoe.

The only other agency to which we could attribute this transportation is that of shore ice and icebergs, which probably did once exist on Lake Tahoe; but the limitation of the pebbles to the western, and especially the northwestern shores, is in exact accordance with the laws of glacial transportation, but contrary to those of floating ice transportation—for lake ice is carried only by winds, and would, therefore, deposit equally on all shores.

Again: I think I find additional evidence of a Lake Tahoe "mer de glace" in the contrasted character of the northern and southern shores of this lake.

All the little glacial lakes described above are deep at the upper end and shallow at the lower end. Further, all of them have a sand beach and a sand flat at the upper end, and great boulders thickly scattered in the shallow water, and along the shore at the lower end. These facts are easily explained, if we remember that while the glacial *scooping* was principally at the upper end, the glacial *droppings* were principally at the lower end. And further: that while the *glacial* deposit was principally at the lower end, the *river* deposit, since the glacial epoch, has been wholly at the upper end.

Now the great lake, also, has a similar structure. It also has a beautiful sand and gravel beach all along its upper shore, and a sand flat extending above it; while at its lower, or northern end, thickly strewed in the shallow water, and along the shore line, and some distance above the shore line, are found in great abundance *boulders of enormous size.* May we not conclude that similar effects have been produced by similar causes—that these huge boulders were dropped by the great glacier at its lower end? Similar boulders are also found along the northern portion of the eastern shore, because the principal flow of the ice-current was from the southwest, and in the fullness of glacial times the principal exit was over the northeastern lip of the basin.

b. Origin of Lake Tahoe.—That Lake Tahoe was once wholly occupied by ice, I think, is certain; but that it was scooped out by Lake Valley glacier is perhaps more doubtful. All other Sierra lakes which I have seen certainly owe their origin to glacial agency. Neither do I think we should be staggered by the size or enormous depth of this lake. Yet, from its position, it may be a plication-hollow, or a trough produced by the formation of two parallel mountain ridges, and afterward modified by glacial agency, instead of a pure glacial-scooped rock-basin. In other words, Lake Valley, with its two summit ridges, *may well be regarded as a phenomenon belonging to the order of mountain-formation and not to the order of mountain sculpture.* I believe an examination of the rocks of the two summit ridges would probably settle this. In the absence of more light than I now have, I will not hazard an opinion.

c. Passage of slate into granite.—From the commencement of the rocky cañon at the head of Fallen Leaf Lake, and up for about two miles, the cañon walls and bed are composed of *slate.* The slate, however, becomes more and more metamorphic as we go up, until it passes into what much resembles *trap.* In some places it looks like *diorite* and in others like *porphyry.* I saw no evidence, however, of any outburst. This latter rock passes somewhat more rapidly into *granite* at Soda Springs. From this point the cañon bed and lower walls are granite, but the highest peaks are still a dark, splintery, metamorphic slate. The glacial erosion has here cut through the slate and bitten deep into the underlying granite. The passage from slate through porphyritic diorite into granite may, I think, be best explained by the increasing degree of metamorphism, and at the same time a change of the original sediments at this point; granite being the last term of metamorphism of pure clays, or clayey sandstones, while bedded diorites are similarly formed from ferruginous and calcareous slates. Just at the junction of the harder and tougher granite with the softer and more jointed slates, occur, as might be expected, cascades in the river. It is probable that the cascades at the head of Cascade Lake and Emerald Bay mark, also, the junction of the granite with the slate—only the junction here is covered with débris. Just at the same junction, in Fallen Leaf Lake Cañon, burst out the waters of Soda Springs, highly charged with bicarbonates of iron and soda.

d. Glacial Deltas.—I have stated that the moraines of Cascade Lake and Emerald Bay glaciers run down to the margin of Lake Tahoe. An examination of this portion of the lake shore shows that *they ran far into the lake*—that the lake has been filled in, two or three miles, by glacial débris. On the eastern margin of Lake Tahoe, the water, close along the shore, is comparatively shallow, the shore rocky, and along the shore-line, above and below the water, are scattered great boulders, probably dropped by the main glacier. But on the west margin of the lake the shore-line is composed wholly of moraine matter, the water very deep close to shore, and the bottom composed of precisely similar moraine matter. In rowing along the shore, I found that the exquisite ultramarine blue of the deep water extends to within 100 to 150 feet of the shore-line. At this distance, the bottom could barely be seen. Judging from the experiments of my brother, Professor John LeConte, according to which a white object could be seen at a depth of 115 feet, I suppose the depth along the line of junction of the ultramarine blue and the emerald green water is at least 100 feet. The slope of the bottom is, therefore, nearly, or quite, 45°. It seems, in fact, a direct continuation beneath the water of the moraine slope. The materials, also, which may be examined with ease through the wonderfully transparent water, are exactly the same as that composing the moraine, viz: earth, pebbles, and boulders of all sizes, some of them of enormous dimensions. It seems almost certain that *the margin of the great Lake Valley glacier, and of the lake itself when this glacier had melted and the tributaries first began to run into the lake, was the series of rocky points at the head of the three little lakes, about three or four miles back from the present margin of the main lake; and that all lakeward from these points has been filled in and made land by the action of the three glaciers described.* At that time Rubicon Point was a rocky promontory, projecting far into the lake, beyond which was another wide bay, which has been similarly filled in by débris brought down by glaciers north of this point. The long moraines of these glaciers are plainly visible from the lake surface; but I have not examined them. Thus, all the land, for three or four miles back from the lake-margin, both north and south of Rubicon Point, is composed of *confluent glacial deltas*, and on these deltas the moraine ridges are the *natural levées* of these ice-streams.

e. Parallel Moraines.—The moraines described above are peculiar and almost unique. Nowhere, except about Lake Tahoe and near Lake Mono, have I seen moraines in the form of *parallel ridges* lying on a level plain and terminating abruptly *without any signs of transverse connection* (*terminal moraine*) *at the lower end.* Nor have I been able to find any description of similar moraines in other countries. They are not terminal moraines, for the glacial pathway is open below. They are not lateral moraines, for these are borne on the glacier itself, or else stranded on the steep cañon sides. Neither do I think moraines of this kind would be formed by a glacier emerging from a steep narrow cañon and running out on a level plain; for in such cases, as soon as the confinement of the bounding walls is removed, the ice stream spreads out into an *ice lake.* It does so as naturally and necessarily as does water under similar circumstances. The deposit would be nearly transverse to the direction of motion, and, therefore, more or less crescentic. There must be something peculiar in the conditions under which these parallel ridges were formed. I believe the conditions were as described below.

We have already given reason to think that the original margin of the lake, in Glacial times, was three or four miles back from the present margin, along the series of rocky points against which the ridges abut; and that all the flat plain thence to the present margin is made land. If so, then it is evident that at that time the three glaciers described ran far out into the lake, until reaching deep water, where they formed icebergs. Under these conditions, it is plain that the pressure on this, the subaqueous portion of the glacial bed, would be small, and become less and less until it becomes nothing at the point where the icebergs float away. The pressure on the bed being small, not enough to overcome the cohesion of ice, there would be no spreading. *A glacier running down a steep narrow cañon and out into deep water, and forming icebergs at its point, would maintain its slender, tongue-like form, and drop its débris on each side, forming parallel ridges, and would not form a terminal moraine because the materials not dropped previously would be carried off by icebergs.* In the subsequent retreat of such a glacier, imperfect terminal moraines might be formed higher up, where the water is not deep enough to form icebergs. It is probable, too, that since the melting of the great "mer de glace" and the formation of the lake, the level of the water has gone down considerably, by the deepening of the Truckee Cañon outlet by means of erosion. Thus not only did the glaciers retreat from the lake, but also the lake from the glaciers.

As already stated, similar parallel moraine ridges are formed by the glaciers which ran down the steep eastern slope of the Sierras, and out on the level plains of Mono. By far the most remarkable are those formed by Bloody Cañon Glacier, as described in my former paper. These moraines are six or seven miles long, 300 to 400 feet high, and the parallel crests not more than a mile asunder. There, also, as at Lake Tahoe, we find them terminating abruptly in the plain without any sign of terminal moraine. But higher up there are small, imperfect, transverse moraines, made during the subsequent retreat, behind which water has collected, forming lakes and marshes. But observe: these moraines are also *in the vicinity of a great lake;* and we have abundant evidence, in very distinct terraces described by Whitney* and observed by myself, that in glacial times the *water stood at least six hundred feet above the present level.* In fact, there can be no doubt that at that time the waters of Mono Lake (or a much greater body of water of which Mono is the remnant) washed against the bold rocky points from which the débris ridges start. *The glaciers*

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* Geological Survey of California, vol. i, 451.

in this vicinity, therefore, must have run out into the water six or seven miles, and doubtless formed icebergs at their point, and, therefore, formed there no terminal moraine.

That the glaciers described about Lake Tahoe and Lake Mono ran out far into the water and formed icebergs I think is quite certain, and that parallel moraines open below are characteristic signs of such conditions I also think nearly certain.

f. Glacial Erosion.—My observations on glacial pathways in the high Sierra, and especially about Lake Tahoe, have greatly modified my views as to the nature of glacial erosion. Writers on this subject seem to regard glacial erosion as mostly, if not wholly, a *grinding* and *scoring;* the débris of this erosion as rock-meal; the great boulders, which are found in such immense quantities in the terminal deposit, as derived wholly from the crumbling cliffs above the glacial surface; the *rounded* boulders, which are often the most numerous, as derived in precisely the same way, only they have been engulfed by crevasses, or between the sides of the glacier and the bounding wall, and thus carried between the moving ice and its rocky bed, as between the upper and nether millstone. In a word, all boulders, whether angular or rounded, are supposed to owe their *origin* or *separation* from their parent rock to atmospheric agency, and only their *transportation* and *shaping* to glacial agency.

Now, if such be the true view of glacial erosion, evidently its effect in mountain sculpture must be small indeed. *Roches moutonneés* are recognized by all as the most universal and characteristic sign of a glacial bed. Sometimes these beds are only imperfectly *moutonneés*, i. e., they are composed of *broken angular surface with only the points and edges planed off.* Now, *moutonneés* surfaces always, and especially angular surfaces with only points and edges beveled, show that the erosion by grinding has been only very superficial. They show that if the usual view of glacial erosion be correct, the great cañons, so far from being *formed*, were only *very slightly modified* by glacial agency. But I am quite satisfied, from my own observations, that this is not the only *nor the principal* mode of glacial erosion. I am convinced that a glacier, by its enormous pressure and resistless onward movement, is *constantly breaking off large blocks* from its bed and bounding walls. Its erosion is not only a grinding and scoring, but also a *crushing and breaking.* It makes by its erosion not only rock-meal, but also large *rock-chips.* Thus, a glacier is constantly breaking off blocks and making angular surfaces, and then grinding off the angles both of the fragments and the bed, and thus forming rounded boulders and *moutonneés* surfaces. Its erosion is a constant process of alternate *rough hewing* and *planing.* If the rock be full of fissures, and the glacier deep and heavy, the rough hewing so predominates that the plane has only time to touch the corners a little before the rock is again broken and new angles formed. This is the case high up on the *cañon walls*, at the head of Cascade Lake and Emerald Bay, but also in the *cañon beds wherever the slate is approached.* If, on the other hand, the rock is very hard and solid, and the glacier be not very deep and heavy, the planing will predominate over the rough hewing, and a smooth, gentle billowy surface is the result. This is the case in the hard granite forming the beds of all the cañons high up, but especially high up the cañon of Fallen Leaf Lake, where the cañon spreads out, and extensive but comparatively thin snow sheets have been at work. In some cases *on the cliffs*, subsequent disintegration of a glacier-polished surface may have given the appearance of angular surfaces with beveled corners; but, in other cases, in the *bed of the cañon*, and on elevated level places, where large loosened blocks could not be removed by water nor by gravity, I observed the same appearances, under conditions which forbid this explanation. Mr. Muir, also, in his Studies in the Sierra, gives many examples of undoubted rock-breaking by ancient glaciers.

Angular blocks are mostly, therefore, the ruins of crumbling cliffs, borne on the surface of the glacier and deposited at its foot. Many *rounded* boulders also have a similar origin, having found their way to the bed of the glacier through crevasses, or along the sides of the glacier. But *most of the rounded boulders* in the terminal deposit of *great glaciers* are fragments *torn off by the glacier itself.* The proportion of angular to rounded boulders—of upper or air-formed to nether or glacier-formed fragments, depends on the depth and extent of the ice-current. In the case of the universal ice-sheet (ice-flood) there are, of course, no upper formed or angular blocks at all—there is nothing borne on the surface. The moraine, therefore, consists wholly of nether-formed and nether-borne severely triturated materials (*moraine profonde*). The boulders are, of course, all rounded. This is one extreme. In the case of the thin moving ice-fields, the *glacierets* which still linger among the highest peaks and shadiest hollows of the Sierra, on the other hand, the moraines are composed *wholly of angular blocks.* This is the character of the terminal moraine of Mt. Lyell glacier, described in my previous paper. These glacierets are too thin and feeble and torpid to break off fragments—they can only *bear* away what falls on them. This is the other extreme. But in the case of ordinary glaciers—*ice streams*—the boulders of the terminal deposit are mixed; the angular or upper-formed predominating in the small existing glaciers of temperate climates, but the rounded or nether-formed greatly predominating in the grand old glaciers of which we have been speaking. In the terminal deposits of these, especially in the materials pushed into the lake, it is somewhat difficult to find a boulder which has not been subjected to severe attrition.

ART. XX.—*Certain Methyl and Benzyl Compounds containing Selenium;* by C. LORING JACKSON.

METHYL COMPOUNDS.

Methyl Monoselenide $(CH_3)_2Se$.—The only previous research on this compound is one by Wöhler and Dean,[*] but it is evident from the properties and oxidation product described by them, that they obtained the methyl diselenide $(CH_3)_2Se_2$, and not the methyl monoselenide $(CH_3)_2Se$. In fact Rathke, some years later,[†] showed that in the ethyl series their process yielded principally $(C_2H_5)_2Se_2$, and but a comparatively small quantity of $(C_2H_5)_2Se$. Under the direction of Professor Hofmann, in the Berlin Laboratory, I have prepared the methyl monoselenide according to the process by which Rathke succeeded in obtaining ethyl monoselenide, and find that it differs entirely from the substance described under that name by Wöhler and Dean, but is analogous to $(C_2H_5)_2Se$ prepared by Joy[‡] and Rathke. Further, I find that in the benzyl series this process yields $(C_7H_7)_2Se$, while that adopted by Wohler and Dean yields principally $(C_7H_5)_2Se_2$.

Methyl Monoselenide $(C_4H_3)_2Se$ was prepared by distilling a mixture of strong solutions of NaOH and CH_3KSO_4 with P_2Se_5. The product freed from water was purified by fractional distillation.

	Calculated for $(CH_3)_2Se$.[§]	Observed.
Carbon..........	22·22	22·07
Hydrogen......	5·55	5·51

It is a colorless liquid with a brilliant luster and most offensive odor. Boiling point 58·2° (uncorr.). It burns with a blue selenium flame, is heavier than water and does not mix with it; when moist it turns yellow, probably from formation of $(CH_3)_2Se_2$; by boiling with water it is decomposed, selenium being set free. It mixes with alcohol and ether, is not affected by hydrochloric acid or sodic hydrate solution, but dissolves in strong nitric acid, forming

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[*] Ann. Chem. Pharm., xcvii, p. 1. [†] Ibid, clii, p. 208. [‡] Ibid, lxxxvi, p. 36.

[§] The atomic weight used for Se was 78, that given by Lothar Meyer in his Moderne Theorien der Chemie.

Nitrate of Methyl Selenide.—This substance can be purified by recrystallization from pure water or alcohol. Its formula is either $(CH_3)_2Se(NO_3)OH$ or $(CH_3)_2Se(NO_3).(CH_3)_2SeO$, but the percentages from these two formulæ are so nearly the same, that it was imposible to decide from the analysis which was the right one. It crystallizes in long white prisms with a slight odor, which melt at 90·5° (uncorr.), and are extremely soluble in water, less so in alcohol, but freely soluble in hot, insoluble in ether.

Chloride of Methyl Selenide $(CH_3)_2SeCl_2$ was formed by precipitating the nitrate with hydrochloric acid. It can be purified by crystallization from alcohol.

	Calculated for $(CH_3)_2SeCl_2$.	Observed.	
Chlorine	39·66	39·02	38·82

It forms large white very thin scales with a pearly luster and disagreeable odor. Melting point 59·5° (uncorr.). It is slightly soluble in water and ether, very soluble in alcohol and in hydrochloric acid.

Bromide of Methyl Selenide $(CH_3)_2SeBr_2$ was prepared like the chloride using hydrobromic instead of hydrochloric acid. The yellow precipitate was washed with cold water and recrystallized once out of hot alcohol.

	Calculated for $(CH_3)_2SeBr_2$.	Observed.
Bromine	59·70	59·95

It forms very thin sulphur-yellow crystalline plates, with a pearly luster and very disagreeable odor, melts under decomposition at 82° (uncorr.); is less soluble in water than the chloride, not very soluble in cold alcohol; when heated with alcohol a portion dissolves, the rest melts to a dark brown oil, which on cooling suddenly crystallizes in yellow scales, swelling very much and giving off methylselenide, which was recognized by its odor, the supernatant alcohol becomes brown. It would seem that the compound was disassociated according to the following reaction:

$$(CH_3)_2SeBr_2=(CH_3)_2Se+Br_2,$$

and that on cooling it was regenerated, while the heat given off by the combination volatilized a portion of the methylselenide, and the bromine thus left free colored the alcohol brown. This view is supported by the fact that $(CH_3)_2SeBr_2$ can be made by direct combination of $(CH_3)_2Se$ and bromine, much heat being evolved in the process. On account of this property, crystallization of the bromide of methylselenide from alcohol is always attended with loss. The substance is insoluble in ether.

Iodide of Methyl Selenide $(CH_3)_2SeI_2$ was formed when aqueous solutions of the nitrate and potassic iodide were mixed. The red precipitate was washed with water and alcohol. Only approximate results were obtained on analysis, as, owing to the great instability of the substance it was impossible to obtain it perfectly pure. It is a brick-red powder, which even in vacuo gradually decomposes, iodine being set free. It is insoluble in water, quite soluble in alcohol and ether, but its solutions are completely decomposed even by spontaneous evaporation, so that no crystals could be obtained. The substance breaks up, in the same way as the bromide, into methylselenide and iodine.

Various attempts to prepare the oxide $(CH_3)_2SeO$ by treating $(CH_3)_2SeCl_2$ with Ag_2O were unsuccessful, as the argentic chloride formed blackened almost immediately from formation of argentic selenide. The cyanide $(CH_3)_2Se(CN)_2$ seems to be extremely unstable. I did not succeed in preparing it in a form fit for examination, either by the action of KCN on the nitrate or of AgCN on the chloride. A sulphate, probably $(CH_3)_2SeSO_4$, was prepared by the action of argentic sulphate on the chloride, but was not investigated.

Methyl Selenide Platinic Chloride $[(CH_3)_2Se]_2PtCl_4$.—When methyl selenide is poured into an aqueous solution of platinic chloride a pale red precipitate is formed, which boiled with water becomes suddenly bright lemon yellow—the change of color is very striking—and can then be crystallized from a large quantity of hot alcohol by slow evaporation. The crystals were washed with cold alcohol, and the platinum determined by ignition; as with all other platinum compounds containing selenium, very long-continued ignition over the blast-lamp was necessary to drive off the last traces of selenium.

	Calculated for $[(CH_3)_2Se]_2PtCl_4$.	Observed.	
Platinum	35·54	35·48	....
Carbon	8·64	....	8·94
Hydrogen	2·16	....	2·42

It crystallizes in minute yellow plates made up of pennate groups of needles; when ignited below redness it gives off methylselenide and turns black; is very slightly soluble in water, sparingly soluble in hot alcohol, insoluble in ether.

BENZYL COMPOUNDS.

Benzyl Monoselenide $(C_6H_5CH_2)_2Se$.—Sodic monoselenide formed by the action of phosphoric pentaselenide on alcoholic soda was boiled with benzyl chloride in a flask with a return-cooler. The liquid gave on cooling, white needles of benzyl monoselenide, followed by yellow scales of benzyl diselenide, the two substances were separated by recrystallization from alcohol. It saves time to make a first rough separation with ether, in which the monoselenide is the most soluble.

	Calculated for $(CH_7)_7Se$.	Observed.	
Carbon	64·62	63·15	63·73
Hydrogen	5·38	5·54	5·56

If an excess of benzyl chloride is used, a yellow oily liquid is obtained, which after some time deposits the benzyl monoselenide in large well-formed flat prisms apparently belonging to the monoclinic system. These, as well as the white needles deposited by the alcoholic solution, melt at 45·°5 (uncorr.) and burn with a smoky flame, which shows the blue color characteristic of selenium. It has very little odor, is insoluble in water, freely soluble in alcohol and ether.

It seems to form salts analogous to those of the methyl selenide, but owing to their great instability and the difficulty of preparing benzyl monoselenide I was unable to get enough for analyses, and therefore cannot give their formulæ.

Nitrate of Benzyl Selenide.—Benzyl monoselenide was converted by heating gently with strong nitric acid into a white mass, which formed on crystallization from hot alcohol well-marked little rhombic crystals. Melting-point 88° (uncorr.). It is almost insoluble in water and ether, freely soluble in hot alcohol, less so in cold. Its solutions decompose very easily, so that the greater part of the substance disappears on recrystallization.

Chloride of Benzyl Selenide.—An alcoholic solution of the nitrate gave with hydrochloric acid a white precipitate which yielded yellow or brown needles on recrystallization from alcohol. It consists of branching needles either yellow or brown; the color, however, is probably due to partial decomposition, as it is extremely unstable, being decomposed even below the boiling point of alcohol into selenium and benzyl chloride. It is slightly soluble in hot alcohol, and its solution is even more unstable than that of the nitrate.

An alcoholic solution of nitrate of benzyl monoselenide treated with hydrobromic acid gave selenium, and vapors which attacked the eyes violently, and were probably benzyl bromide $C_6H_5CH_2Br$. It seems, therefore, that the chloride and bromide of benzylselenide are decomposed according to the following reaction:

$$(C_7H_7)_2SeBr_2=2C_7H_7Br+Se,$$

while the bromide and iodide of methylselenide undergo an entirely different decomposition:

$$(CH_3)_2SeBr_2=(CH_3)_2Se+Br_2.$$

It is interesting that the replacement of one atom of hydrogen in methyl by phenyl (which converts it into benzyl, HCH_2 into $C_6H_5CH_2$), should produce such a complete change in its attractions.

The nitrate of benzylselenide gave with potassic iodide a very unstable yellow precipitate.

Benzyl Selenide Platinic Chloride $[(C_7H_7)Se_2]_2PtCl_4$ was prepared by mixing alcoholic benzylmonochloride with aqueous platinic chloride. I could not obtain it crystallized, as its solutions decompose on evaporation. It was therefore purified by washing with alcohol

	Calculated for $[(C_7H_7)_2Se]_2PtCl_4$.	Observed.
Platinum........	22·97	22·32

It is a yellow powder insoluble in water, slightly soluble in alcohol; on heating it turned black and gave off a combustible gas.

Benzyl Diselenide $(C_7H_7)_2Se_2$, was prepared by treating a sodic selenide (from the reduction of Na_2SeO_3 with charcoal) with benzyl chloride, this being the method by which Wöhler and Dean obtained their so-called methyl monoselenide; but, as I expected from Rathke's researches in the ethyl series, I obtained principally diselenide with only a base of monoselenide. The crude sodic selenide was boiled in a flask, with a return-cooler with alcohol and benzyl chloride; the liquid thus obtained deposited light yellow scales which were purified by crystallization from boiling alcohol.

	Calculated for $(C_7H_7)_2Se_2$	Observed.
Carbon........	49·70	48·75
Hydrogen	4.14	4·48

It forms straw-yellow scales which are superficially decomposed by direct sunlight, burning brilliant red. It has very little odor, melts at 90° (uncorr.), and remains fluid a long time after it has been melted, burns with a blue selenium flame, and is insoluble in water, freely soluble in hot alcohol, less so in cold, soluble in ether, reacted on by hydrochloric acid, but oxidized by nitric acid.

Benzylselenious Acid $(C_7H_7)SeOOH$.—When benzyl diselenide was treated with strong nitric acid it dissolved with evolution of nitrous fumes to a colorless liquid, which on cooling crystallized in white needles. These were purified by recrystallization from alcohol or hot water.

	Calculated for $(C_7H_7)SeOOH$	Observed.
Carbon........	41·58	41·61
Hydrogen	3·96	4·25

It occurs in radiated groups of white needles with a weak odor when pure; when slightly impure it smells like the

Mephitis americana. Melting-point 85° (uncorr.). But slightly soluble in cold water, indefinitely so in hot, quite freely soluble in cold alcohol, much more in hot, almost insoluble in ether. Its solutions have an acid reaction and set free carbonic anhydride from the carbonates.

Ammonic Benzylselenite was prepared by dissolving the acid in ammonia and driving off the excess of ammonia by evaporation on the water-bath. It is extremely soluble in water and can be obtained with some difficulty in wart-like aggregations of crystals.

Argentic Benzylselenite $(C_7H_7)SeOOAg$ was prepared by mixing aqueous solutions of ammonic benzylselenite and argentic nitrate. The white curdy precipitate was purified by crystallization from a very large quantity of boiling water.

	Calculated for $(C_7H_7)SeOOAg$	Observed.
Silver	34·95	34·33

It forms long hair-like crystals which mat together into a felt-like mass. When heated in the water-bath it turns black, but is not altered in direct sunlight. Slightly soluble in a large quantity of boiling water. Freely soluble in nitric acid.

Sodic Benzylselenite, prepared by neutralizing the aqueous solution of the acid with sodic carbonate can be obtained on evaporation as an imperfectly crystalline white mass very soluble in water.

Baric Benzylselenite, prepared by dissolving baric carbonate in an aqueous solution of the acid is white and very soluble.

Plumbic Benzylselenite, prepared by precipitating ammonic benzylselenite with plumbic nitrate is white and imperfectly crystalline, insoluble in cold water, and even less soluble in boiling water than the silver salt.

Benzyl Selenocyanate $(C_7H_7)SeCN$.—Alcoholic potassic selenocyanate, prepared according to the method of Crookes* was treated with benzyl chloride till its smell remained after shaking. The liquid poured off from the potassic chloride formed, deposited white needles which were purified by recrystallization from alcohol or ether.

	Calculated for $(C_7H_7)SeCN$.	Observed.	
Carbon	94·23	48·99	....
Hydrogen	3·59	3·92	....
Nitrogen	7·23	....	7·39

It crystallizes with the utmost ease in long white needles or prisms, with a most disagreeable smell like that of the *Symplocarpus fœtidus.* Melting-point 71°·5 (uncorr.). It is insoluble in water, soluble in ether and alcohol unattacked by hydrochloric acid, oxidized by fuming nitric acid.

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* Annalen der Chemie und Phamacie, lxxviii, p. 177.

Nitrobenzyl Selenocyanate, $C_7H_6(NO_2)SeCN$, can be prepared by nitrizing benzyl-selenocyanate with fuming nitric acid at —4°; on dilution, white crystals mixed with an oil are deposited; these were purified by pressing between filter paper and crystallizing from boiling alcohol. If nitric acid much above 0° was used, the amount of product was much smaller, and was mixed with an acid of the formula $C_6H_4(NO_2)COOH$, probably a mixture of nitrodracylic acid and one of the other isomeres. A more convenient way of preparing the substance consists in treating potassic selenocyanate with nitrobenzyl chloride, $C_6H_4(NO_2)CH_2Cl$, prepared according to Strakosch's method,* decolorizing with bone-black, and recrystallizing from boiling alcohol. The substances prepared by these two methods seem to be identical and not isomeric, as has been already observed in the case of nitrobenzyl sulphocyanate by Henry.†

	Calculated for $C_7H_6(NO_2)SeCN$.	Observed.
Carbon	40·00	40·87
Hydrogen	2·50	8·12

It forms globular masses of radiating needles resembling wavellite, has but little odor, melts at 122°·5 (uncorr.), is insoluble in cold water and alcohol, but soluble in both when boiling, insoluble in ether. It dissolves in ammonia, but is obtained unaltered from this solution by acidifying, or by evaporating to dryness and recrystallizing. It is possible that the substance prepared from benzylselenocyanate is a little more easily soluble in ammonia than that prepared from nitrobenzylchloride.

SELENINES.

These compounds correspond to the sulphines discovered by Oefele. They should be called selenoniums and sulphoniums instead of selenines and sulphines, as they correspond to the salts of the compound ammoniums, and not to the tertiary amines, as is evident from the annexed comparison:

H_3N	H^2S	H_2Se
$(CH_3)_3N$	$(CH_3)_2S$	$(CH_3)_2Se$
$(CH_3)_4NI$	$(CH_3)_3SI$	$(CH_3)_3SeI$

Further, they resemble the compound ammoniums in all their properties, especially in their tending to form iodine addition-products and alkaline hydrates.

Benzyl diselenide was moistened with an excess of methyl iodide and allowed to stand for several days; it turned black and became pasty. On washing the mass with water and evaporating the wash water, white prisms were obtained consisting of trimethylselenonium iodide $(CH_3)_3SeI$, already described by Cahours,‡ while the black residue consists of benzyldimethylselenonium-triiodide and benzyl iodide.

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* **Berichte deutschen chemischen Gesellschaft, vi, 1056.** † **Ibid., ii, 637.**

‡ **Ann. Chem. Pharm., cxxxv, 352.**

$$(C_7H_7)_2Se_2+5CH_3I=(CH_3)_3SeI+(C_7H_7)(CH_3)SeI_3+C_7H_7I$$

The presence of C_7H_7I was proved by obtaining tribenzylamine, $(C_7H_7)_3N$, by heating with alcoholic ammonia. The white crystals were converted into the chloride by treatment with argentic chloride, and from this the platinum salt was made and analyzed.

Calculated for $[(CH_3)_3SeCl]_2PtCl_4$.		Observed.	
Platinum	30·07	29·27	----
Carbon	10·96	----	11·42
Hydrogen	2·74	----	2·79

Benzyldimethylselenonium Triiodide $(C_7H_7)(CH_3)_2SeI_3$.—The black residue already mentioned was washed very thoroughly with alcohol to free it from benzyl iodide and then recrystallized from boiling alcohol.

Calculated for $(C_7H_7)(CH_3)_2SeI_3$.		Observed.		
Carbon	18·62	19·34	18·92	----
Hydrogen	2·24	2·66	2·87	----
Iodine	65·69	65·69	----	65·17

It forms very heavy black needles, with a metallic luster like that of iodine, and a most offensive odor. It melts at 65°, but softens a few degrees below its melting-point. Heated somewhat more it gives off vapors which attack the eyes violently, is insoluble in water, slightly soluble in cold alcohol, quite so in hot, slightly in ether. Its solutions are red, and are decolorized by gentle heating with metallic mercury. Rathke observed a similar action with a black oil which he obtained by acting on triethylselenonium iodide with iodine. This was undoubtedly the triiodide, but he gives no analysis of it. I have also obtained a black oil by treating trimethylselenonium iodide with iodine, which must be $(CH_3)_3SeI_3$.

Platino-benzyldimethylselenonium Chloride $[(C_7H_7)(CH_3)_2SeCl]_2PtCl_4$, was prepared by precipitating the iodine out of the benzyldimethylselenonium triiodide with argentic nitrate, treating the filtrate with hydrochloric acid, and after filtering from argentic chloride adding platinic chloride. A yellow crystalline precipitate was gradually formed which was washed with alcohol.

Calculated for $[C_7H_7)(CH_3)_2SeCl]_2PtCl_4$.		Observed.
Platinum	24·42	24·03

It forms corn-colored crystalline scales, which appear square with obtuse re-entering angles under the microscope, and is insoluble in water and alcohol. On heating it turns brown below 100°, and at a somewhat higher temperature becomes black and gives off a combustible vapor.

The details of the processes will be found in an article soon to appear in Liebig's Annalen der Chemie und Pharmacie.

Art. XXI.—*Description of the Nash County Meteorite, which fell in May*, 1874; by J. Lawrence Smith, of Louisville, Kentucky.

The meteorite of Nash County, North Carolina, fell May 14th, 1874, at 2½ o'clock P. M., near Castalia, in lat. 36° 11′, long. 77° 50′. Its fall was accompanied with the successive explosions common in such cases, and with rumbling noises that lasted about four minutes, not unlike the discharge of firearms in a battle a few miles off.

The stones that fell must have exceeded a dozen or more; three only have been found, and they give evidence that the territory over which the fragments fell was ten miles long by over three miles wide. Although occurring in the day, the body appeared luminous to some observers. The three stones found weighed respectively one kilogram, 800 grams, and 5½ kilograms. The second was the fragment of one broken by the fall. Several fragments have come under my observation, from which I am enabled to give the following description.

They are of the more common aspect. They have a dull exterior coating, which in some places does not entirely cover the stones, there being a few spots of the fractured surface, less than a centimeter in diameter, over which the fused matter forming the coating is scattered in the form of pear-shaped beads. In one or two crevices, below the surface, some of the fused matter of the coating has penetrated five millimeters below the surface, and here it is more brilliant than on the surface.

The interior in many parts is of a dark gray color, and in other parts quite light; the principal cause of the dark color is doubtless owing to the larger amount of nickeliferous iron in that part, and in the lighter portion there are some white spots of a mineral that is doubtless enstatite.

The specific gravity of the stone is =2·601. Its composition is

Nickeliferous iron	15·21 p. c.
Stony minerals	84·79

The nickeliferous iron consists of—

Iron	92·12
Nickel	6·20
Cobalt	·41
	98·73

Copper and phosphorus not estimated.

The stony part, when treated with a mixture of chlorhydric and nitric acid, gave—insoluble part, 47·02; soluble part, 52·98. The former was found to be composed as follows:

Silica	52·61
Alumina	4·80
Protoxide of iron	13·21
Magnesia	27·31
Alkalies (soda with traces potash and lithia)	1·38
	99·31

and is essentially bronzite. The soluble portion gave—

Silica	38·01
Protoxide of iron	17·51
Magnesia	41·27
Alumina	·46
Sulphur	1·01
	98·26

This is evidently olivine, with a small amount of sulphide of iron, which is so disseminated through the stone that it is not easily separated completely by mechanical means. From the mineralogical examination and the chemical results detailed above, this meteoric stone consists essentially of nickeliferous iron, bronzite and olivine, with small particles of anorthite and enstatite. Its composition is, therefore, a usual one.

For particulars in regard to the fall of this meteorite I am indebted to Prof. Kerr, State Geologist of North Carolina.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Commercial Quinidine Sulphate.*—By this name various mixtures are known in commerce, such as of quinine and chinchonine sulphate, and of quinidine, cinchonine, and cinchonidine sulphates, beside the pure salt itself. Hesse recommends the following method for the examination of such mixtures: (1) One gram of the sulphate is dissolved in seven c. c. of a mixture of two volumes chloroform and one of 97 per cent alcohol. The pure salt dissolves, leaving behind any inorganic salts present. (2) Half a gram of the sulphate is digested at 60° C. with 20 c. c. of water, and 1·5 grams Rochelle salt is added, producing a crystalline precipitate. After standing an hour, the precipitate is filtered off; the filtrate should not show the slightest turbidity on adding a drop of ammonia. The production of a precipitate indicates the presence of cinchonine or quinidine. These may be distinguished by repeating the test with the addition of potassium iodide, as already detailed above. Or the cinchonine may be directly tested for by adding half a gram potassium iodide to the Rochelle salt solution; after an hour, the whole is filtered and a drop of ammonia is added. No precipitate is produced if cinchonine be absent.—*Liebig's Annalen*, clxxvi, 325, May, 1875. G. F. B.

2. *On Chrysezarin, the Dioxyquinone of Chrysene.*—In the course of an examination of certain specimens of commercial alizarin, Claus noticed in one of these a special substance, the solution of which in potassa gave a blood-red color, instead of the violet tint of potassium alizarate. It is separated from the alizarin by extracting the dry potassium salt with boiling alcohol. A dark brown residue is left on evaporation, which dissolves in water with a yellow-red color, acids throwing down a lemon-yellow precipitate. Crystallized from acetic acid, it forms dark brown needles, with a bronze reflection. It melts above 300° and sublimes at 305°–310°, condensing again in brilliant orange needles. Analysis gives as its formula $C_{18}H_{10}O_4$, and that of its potassium salt $C_{18}(K_2H_8)O_4$. The author, hence, considers it the dioxyquinone of chrysene, and gives it the name chrysezarin. One kilogram of alizarin paste contains four or five grams of chrysezarin.—*Moniteur Scientifique*, III, v, 396, May, 1875. G. F. B.

3. *Production of Albumin from Fibrin.*—Gautier shows that on dialysing a solution of fresh blood fibrin in sodium chloride, the inorganic matters are separated, and there remains a liquid coagulated by heat, by mineral acids and by mercuric chloride, the precipitates having the composition of those from pure albumin.—*Comptes Rendus*, lxxix, 227. G. F. B.

4. *On the conversion of Brucine into Strychnine.*—The close chemical relation between the different alkaloids of the same plant-species is a well observed fact. The two bases of the *Strychnos* family, strychnine, $C_{21}H_{22}N_2O_2$, and brucine, $C_{23}H_{26}N_2O_4$, form no exception to this rule, and Sonnenschein has recently succeeded in showing that the former may be produced from the latter at will. For this purpose the brucine is gently warmed with four or five times its weight of dilute nitric acid. The liquid becomes red and evolves carbonic acid. It is treated with potassium hydrate in excess, and agitated with ether. This on evaporation leaves a red mass, from which, on solution and recrystallization, a crystallized bitter substance was obtained, having all the properties of strychnine. This result is of the greatest practical importance. A student in the author's laboratory having been given a mixture containing lead nitrate and brucine, used the Stas-Otto method for detecting alkaloids, and found a substance giving the reactions of strychnine. Hence the use of nitric acid in examining for alkaloid poisons is to be avoided, since strychnine may be thus formed.—*Ber. Berl. Chem. Ges.*, viii, 212, March, 1875. G. F. B.

5. *Formation of Indol from Egg-albumin.*—Nencki, in an earlier paper, having ascertained the fact that indol injected into the body produces indigo-blue in the urine, made experiments to ascertain if indol was a normal product of pancreatic digestion, as had been asserted; and though the results were suggestive, they were not final. Kühne having given it as his opinion that the body thus obtained was not really indol, but some other body closely resembling it, Nencki returns to the charge and produces

indol actually obtained from albumin in this way, remarking that it may be much more readily prepared from this substance than from indigo-blue. For this purpose 300 grams commercial albumin was mixed with 4½ liters spring water, and an ox-pancreas, carefully freed from blood and fat, and finely divided, was added. The beaker, covered with a glass plate, was kept at 40°–50° for sixty or seventy hours. The liquid was then cooled, strained through cloth, acidulated with acetic acid to retain the excess of albumin in solution, and distilled on a water bath to one-fourth of its volume. The filtered and acid distillate was rendered alkaline with dry slacked lime and extracted with an equal volume of ether; the ether, on distillation, left having the characteristic odor of indol. Mixed with water it became crystalline, and recrystallized from water, it fused at 52°. Elementary analysis confirmed it as indol. The amount obtained was about 0·3 per cent of the albumin employed.—*Ber. Berl. Chem. Ges.*, vii, 1593; viii, 206, 336, March, 1875. G. F. B.

6. *Collodion Films.*—M. E. Gripon states that if collodion is poured on a very clean plate of glass, the film may be separated when dry and stretched on a frame. Its surface is polished, and it reflects light like glass; it polarizes by reflection and by refraction, the angle of maximum polarization being 56° 25′, corresponding to an index of refraction of 1·5108, or a little less than crown glass. From this index we can, by observing the interference fringes, calculate the thickness, with a result varying from ·0081 to ·0088 mms. So thin a film cuts off but little radiant heat. With the flame of a lamp as a source, ·91 is transmitted. With a blackened vessel containing boiling water ·70, with water at 50° but ·50, a proportion but little altered when the water is cooled to 20°. With two films the transmitted heat is ·583, the first one transmitting ·70, the second ·83.

A series of these films may be used to polarize light and heat by refraction; yet they are liable to break by the increase of tension when exposed directly to sunlight. Their diathermancy is much greater than that of piles of mica, and though more fragile, they are easily repaired. Two sets, each consisting of six collodion films, transmitted when crossed only ·66 as much heat as when placed parallel. A pile of nine films being placed before a Nicol's prism was found to polarize ·6 to ·7 of the heat.—*Comptes Rendus*, lxxx, 882. E. C. P.

7. *Combustion of Explosive Mixtures.*—M. Neyreneuf has described two methods of showing the vibratory motion attending the explosion of a mixture of oxygen and hydrogen in a cylindrical tube. First, by using tubes carefully dried, and secondly by covering the interior with paraffine. In the first case the steam condensing on the colder portions leaves transparent the parts which the flame in vibrating has more strongly heated. These latter portions are, on the other hand, shown in the second case by the melting of the paraffine. It is indispensable that the combustion shall not be too rapid; and further, to obtain the best effects

the proportions of the mixture should be varied with the dimensions of the tube. With a tube 3 cms. in diameter and 20 cms. long, containing one part of hydrogen and one of air, lines appear, resembling fern-leaves. With tubes of less diameter the effects are more regular, especially if a musical sound is produced by the explosion. Fine striæ are then observed perpendicular to the axis of the tube. When the tube is very long, there is only a confused sound, but the striæ are well separated and sharply defined. If the mixture is ignited by the eudiometric method, these appearances are no longer produced. If the tubes are open at both ends, the effect is the same as if one end is closed. With a long tube the appearance in the dark is the same as that of a Geissler tube illuminated by a single motion of the breakpiece.—*Journal de Physique*, iv, 138. E. C. P.

8. *Changes in Light due to the motion of the luminous source or of the Observer.*—M. MASCART has examined this question both experimentally and theoretically. Arago announced that the refraction of the light of two stars, toward one of which the earth is approaching and from the other receding, was the same, and Fresnel showed that this could be accounted for if part of the ether was transported with the refracting medium, the change in refraction in fact being compensated by the motion of the telescope. Many efforts have been made to show optically the motion of the earth. Babinet thought he found a solution in the diffraction of gratings, and showed that in this case the compensation does not take place. This experiment, repeated under the most favorable conditions, both with solar and with artificial light, yields results wholly negative. And the delicacy of the method is such that the change, if any, must be very small compared with that given by Babinet's formula. Examining the theory further reveals a source of error in the assumption that solar light reflected from a mirror acts exactly as if it came from a source on the prolongation of the ray. But this is not correct if the mirror moves, as is the case on the earth, but the conditions are the same as if the mirror was itself luminous, or as if a terrestrial source of light was employed. Accordingly, as experiment shows, negative results only are obtained.

Suppose that we have two sources of light, one the soda flame, the second a star containing soda, and that both emit rays in the opposite direction from the motion of the earth. The absolute time of vibration of the two sources are identical, but the wavelengths of the light emitted are different, owing to the motion of the first. The deviations of the two rays ought, therefore, to be different. To test this experimentally, two large instruments were constructed, one fixed with the collimator turned to the west, and inclosed in a cave where the daily changes of temperature were extremely small. The other was placed on a movable plate so that it could be pointed either east or west. Observations showed that the deviation was wholly inappreciable, although a displacement of a twentieth of that given by Fresnel's formula would have been perceptible. But this formula contains two terms, of which the first is proportional to the velocity of the medium, but is very small, and slight changes in it are quite inappreciable. The principal term, on the other hand, seems to depend only on the apparent period of the incident light, and is therefore independent of the motion. By applying a modification of the method of the interference of plates it appears that there is not a difference of ·000005, or of ·000002, using the method of Newton's rings. The negative results of M. Hock on the interference of light passing through a refracting medium with or against the motion of the earth is similarly explained. Again, by the double refraction of Iceland spar, fringes were obtained differing by 50,000 or even 100,000 wave lengths, without a variation of ·000001 in their apparent position. With the rotary polarization of quartz with an instrument capable of detecting a change of plane of quarter of a degree and a rotation of fifteen circumferences, not the least change was preceptible, due to the motion of the earth.—*Jour. de Phys.*, iv, 129. E. C. P.

9. *New source of Magnetism.*—M. DONATO TOMMASI states that when a current of steam under a pressure of five or six atmospheres is passed through a tube of copper two or three millimeters in diameter, and rolled in a helix around a cylinder of iron, the latter is so strongly magnetized that an iron needle placed some centimeters from the *steam-magnet* is strongly attracted and remains magnetized as long as the steam passes through the tube.—*Comptes Rendus*, lxxx, 1007. E. C. P.

II. GEOLOGY AND NATURAL HISTORY.

1. *The Geology of New Mexico.*—Prof. COPE stated that the ground covered by the geological investigation conducted mainly in New Mexico during 1874 in connection with the Wheeler U. S. topographical and geological survey, embraced the eastern slope of the Rocky Mountains from Pueblo to the Sangre de Christo Pass; both sides of the Rio Grande Valley from that point to Algodones, N. M.; the western or Sierra Madre range; and the country for forty miles to the westward of it, from the latitude of Sierra Amarilla as far south as the road from Santa Fé to Fort Wingate.

Little of novelty has been added from the first two named regions, as they have been previously traversed by competent geologists; but the last named has remained up to the present time almost unknown. The analysis of the structure of the Sierra Madre range is believed to indicate that its elevation took place near the close of the period known as Cretaceous No. 4, and that the elevating force was, in New Mexico, more powerful at its southern extremity than along the middle portions of its line. Another important discovery is the lacustrine character of the Triassic beds which form a part of the axis of the range; indicating the existence of extensive areas of dry land at that period, of which no portion is remaining in the region examined by him, but which may be supposed to be represented by the palæozoic beds farther south and west. A third important point is the determination that the plateau drained by the eastern tributaries of the San Juan River is composed of the sediment of an extensive lake of Eocene age, which was probably at one time of great extent, but whose deposits have been greatly reduced in extent through erosion. The boundaries of this lake to the east and south were determined.

It is believed that additional light has been thrown on the question of the age of the Galisteo sandstone; and that its paleontology has decided definitely that of the Sante Fé marls. The first fossils discovered in the "Trias" of the Rocky Mountains, have enabled me to reach more definite conclusions as to its position in the scale of periods. The remains of vertebrata obtained from the latter formation are those of fishes and reptiles. The former are rhomboganoid scales of small species which are numerous in the coprolites of the reptiles; the latter represent the three orders of *Crocodiles*, *Dinosauria*, and apparently of *Sauropterygia.* The dinosaurian order is represented by a part of the crown of a tooth of a species of large size, of the general character of *Laelaps.*

Prof. Cope describes the suspected Sauropterygian from a single vertebra, and names it *Typothorax coccinarum.* The flat and regularly fitted dermal bones distinguish this genus from *Belodon.* He remarks that the evidence from this species is favorable to the identification of this horizon with that of the Trias, although it cannot of course be regarded as conclusive, until more perfect specimens are obtained.

The thickness of the Eocene of the region is estimated at 3,000 feet, of the Cretaceous at about 5,000 feet, the Jurassic at 600 feet and the Trias at 1,000 feet or more.

2. *Fossil Ungulates from New Mexico.*—Professor COPE (Proc. Philad. Acad. Sci., 1875) has described a species of camel, about as large as the dromedary, from near Pojuaque, under the name *Pliauchenia vulcanorum.* The dental formula is, molars 4–3; also the *Hippotherium calamarium* Cope, from near San Ildefonso, and *Aphelops Jemezanus* Cope, a rhinoceros, from near the town of Santa Clara.

3. *Coal beds in the Subcarboniferous of Pennsylvania.*—Letter of J. P. LESLEY, Director of the Geological Survey of Pennsylvania to J. D. Dana, dated June 26.—I have just learned the important fact that Mr. Ashburner of this survey has discovered a considerable number of what he calls "baby coal beds" in No. X, Upper or White Catskill, Rogers' Vespertine. They are exposed by the tunnel of the East Broad Top Railroad through Sideling Hill in Huntington County. Mr. Billin and Mr. Ashburner are constructing a section of all the measures from the Trenton up to the coal of Broad Top, across the outcrops of the Clinton fossil ore-beds, the brown hematites of the Hamilton, and the ores of IX and XI, Catskill and Umbral (or Cuyahoga of Newberry), with

heavy beds of Chemung fossils, close under the Catskill, and coal-beds in X, (Berea Grit of Newberry). This last discovery is of the greatest importance to American geology. It explains the presence of the two coal beds on the face of the Alleghany Mountains, and the fourteen small coal beds which I counted years ago behind (west of) the Peak Mountain in Wythe County, Southern Virginia.

4. *On the Devonian Trilobites and Mollusks of Ererê, Province of Pará, Brazil;* by Prof. C. F. HARTT and R. RATHBUN. 28 pp. 8vo. From the Ann. Lyc. Nat. Hist. N. Y., vol. xi, May, 1875.—The collections here described were made in the course of the Morgan Expeditions, 1870–'71. The paper contains descriptions of two new species of trilobites, *Dalmania Paituna* and *Homalonotus Oiara*, six of Gasteropods, of the genera *Pleurotomaria*, *Holopea*, *Platyceras* and *Bellerophon*, eight of Lamellibranchs, and one Tentaculite. The Brachiopods are described by Mr. Rathbun in an earlier paper (see this Journal, vol. vii, p. 607).

5. *Practical Guide to the Determination of Minerals by the Blowpipe;* by Dr. C. W. C. FUCHS, Prof. Univ. Heidelberg. Translated and edited by T. W. DANBY, M.A., F.G.S., Fellow of Downing College, Cambridge, etc. 88 pp. 8vo. London, 1875. (London, Field and Tuer; Philadelphia, Claxton, Remsen & Haffelfinger).—Dr. Fuchs's work on the Determination of Minerals, which is well known abroad, commences with a brief account of blowpipe reagents and reactions. A "General table" contains a synopsis of the subdivisions of mineral species based on blowpipe characters arranged for convenient use; and following this table, the characters of the species in these several subdivisions are mentioned in detail. A large proportion of the mineral species are included in this part of the work. The volume closes with an alphabetical table of the mineral species, giving for each the hardness, specific gravity, and crystallographic system.

6. *Mineralogisches Lexicon für das Königreich Sachsen*, von AUGUST FRENZEL. 380 pp. 12mo. Leipzig, 1874. (W. Engelmann.)—This work is an excellent treatise on Saxon Mineralogy. The descriptions are drawn up with accuracy and precision, and end with a full list of Saxon localities. Probably no region so small embraces within its bounds a larger number of mineral species—about 300.

7. *Commelynaceæ et Cyrtandraceæ Bengalenses*; by C. B. CLARKE, late acting Superintendent of the Calcutta Botanic Garden. Calcutta: Thacher, Spink & Co. 1874, folio.—This is an imperial folio volume, of 135 printed pages and 93 lithographic plates, illustrating the Bengalese species of the two orders above-mentioned. Of the *Commelynaceæ* Mr. Clarke had made a previous study, and published a good paper in the Linnean Society's Journal, in 1870, a little before Dr. Husskarl's elaborate *Commelynaceæ Indicæ* appeared. Mr. Clarke recognizes much fewer genera than the Dutch botanist, and probably takes a sounder view of generic limitation. The outline figures now given, so far as orig-

inal, are from drawings by native artists, who were ignorant of botany. They are neat and clear, and evidently give the facies with accuracy. The dissections, which are not very many, are from the author's own sketches, and are doubtless "tolerably correct," although he is "totally uninstructed in drawing." That so sumptuous a work of the kind can be published in Calcutta argues well for the interest there taken in the indigenous botany, and the price (ten rupees) is most moderate. If the work were to be continued, a smaller form, like that of Wight's Icones would be more convenient and more practicable. A. G.

8. *The Box-Huckleberry* (*Gaylussacia brachycera* Gray) is one of the rarest of North American plants. The elder Michaux's habitat is in Virginia, near Winchester, but the specimen in his herbarium at the *Jardin des Plantes*, is ticketed "Warm Springs." Pursh's is "Western parts of Virginia, near Winchester and the Sweet Springs"; and, if I rightly remember, some specimen of his collecting was ticketed "Cacapon Mountains." Muhlenberg's specimen, from Matthew Kin, is ticketed "*Krien Preyer*": Anglice Green-brier. All this relates to the Alleghany Mountain region, west of the Blue Ridge. I am not aware that any of these stations have been rediscovered, or that any living botanist has seen this little evergreen shrub in Virginia. The only available habitat known in our time, is the one which Prof. Baird discovered, fully thirty years ago, in Perry County, near Bloomfield, Pennsylvania; also in the Alleghany region, but more northward. From this station the Botanic Garden of Harvard University fortunately still possesses one or two thriving living plants The locality was restricted, and, Prof. Baird informs me, is now probably extinct through the bringing of the ground into cultivation. It is pleasant to be able to announce that Mr. A. Commons, of Wilmington, has just discovered a new locality, but in a wholly unexpected district, namely, in Susex Co., in the southern part of Delaware. He found it "while walking along the banks of Indian River, on the edge of a pine forest which here skirts the shore, growing under the shade of the Laurel (*Kalmia latifolia*) on a dry sandy bank." The plants were very sparingly in fruit. We hope that this diminutive shrub will now become more familiar to botanists, and that it may be brought anew into cultivation. It resembles a dwarf Box. The flowers are rather pretty, but inconspicuous. A. G.

9. *On the preservation of Anatomical Preparations.** — Dr. Sesemann of St. Petersburg, gives an account in the last number of Reichert and DuBois Reymond's "Archiv fur Anatomie, Physiologie," etc., of his experience in the use of preserving solutions for anatomical preparations, which may be of some interest to zoologists, as well as anatomists.

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* Abstract of an article entitled "Ein Beitrag zur Conservirung anatomischen Präparate," von Dr. E. Sesemann in St. Petersburg.

Archiv. f. Anat. Phys etc., Reichert u. Dubois Reymond. 1874. No. 6 (published April, 1875), p. 679.

The first solution mentioned is composed of 6 *parts carbolic acid*, and 100 *parts olive oil* (parts, "*theilen*," refers probably to measures by bulk and not by weight). This solution he injected into the main arteries of a human hand. After exposure to the air for a few weeks the preparation became completely shriveled. A second solution of 3 *parts carbolic acid* and 10 *parts glycerine*, was used in the same manner with a similar result.

Prof. Laskowsky's method is then given. The solution used by him, is composed as follows:

100 parts glycerine, pure ("*rohen*"), 2 of carbolic acid, and 2 of acetate of soda.

The part of the subject experimented upon is left to soak in this solution for five to twenty days, according to its size. When taken out the specimen is quite hard, but after hanging sometime in the open air, it becomes fresh ("*rein*") again and remains so a long time unchanged.

Dr. Sesemann tried this process with a prepared hand of a human subject. He left the specimen in the solution 6 days; upon taking it out, it was completely hard, but became softer after a few days' exposure to the air. The muscles, however, had become of a dull brown color, and grew darker with longer exposure. The "*Van Vetter*" process is next described. In the place of the last solution, the following was used:

7 parts glycerine (22°), 1 part sugar, ½ part saltpetre.

This process was not found satisfactory, as the sugar crystallized out upon exposure and the muscles became brown, as in Prof. Laskowsky's process.

After various experiments, Dr. Sesemann hit upon the following method as in all respects the best.

Sesemann's method.—The blood is first pressed out of the larger vessels as completely as possible. Then solution (No. 1) is injected.

$$\text{Sol. No. 1.}\left\{\begin{array}{ll} 100 & \text{parts water.} \\ 50 & \text{" glycerine.} \\ 10 & \text{" arsenate of soda.} \end{array}\right.$$

(The *arsenate of soda* is prepared by adding arsenic to a hot concentrated solution of soda until no more of the former dissolves). The specimen is left then for 24 hours, when it is again injected with solution, No. 2.

No. 2.—Glycerine and water in equal parts.

After waiting another 24 hours, it is immersed in water heated to 70 or 88 degrees Cent. (about 160°–175° Fahr.) and left there 10–12 minutes. When taken out, and while still warm the vessels may be injected with wax, if it be desired. This being done, the epidermis is rubbed off with a rough cloth. A dull knife may be used for this purpose, though liable to injure the skin. It is well to moisten the specimen occasionally. If wrapped in a cloth moistened with water, glycerine and carbolic acid the specimen will keep a long time, and the anatomical structures may be studied and exposed at the student's leisure. After such preparation is completed the skin should be put back in its place and fastened together with pins. It is then to be immersed in solution No. 3, consisting of

100 parts pure glycerine, 20 of water, 4 of arsenate soda, and 2 of carbolic acid,

and left in this solution 5–30 days according to the size of the specimen. When taken out it is ready to expose to the air indefinitely. The skin may turn a little brown after some time in the atmosphere, but this may be remedied by covering it for a few hours with a cloth which has been moistened with a concentrated solution of corrosive sublimate in water.

The author says further that though a rather long process, it is recommended by the beauty and durability of the preparation. He has specimens thus prepared, which after several months' exposure to the air, have the appearance of having been just cut from the subject. He also suggests that it is further recommended by the smallness of expense involved.

Solution No. 2, after becoming weak from repeated use, may be purified and brought back to normal strength by putting in a tin pail and this in a large wash kettle and heating it for some time over a slow fire, then filtering the solution through a linen cloth. The filtration will also remove albuminous substances, which the heat will tend to coagulate. H. S. W.

III. ASTRONOMY.

1. *Diameters of the Planets.*—We give the following values of the apparent diameters of the planets reduced to the mean distance of the earth from the sun and of their true diameters in English miles, as being perhaps as reliable as any that can be assigned from existing data. They are founded in every case upon the measures which from observational circumstances appear to deserve the greatest weight, and in the reduction to true values the solar parallax is taken 8″·875, and Clarke's diameter of the earth's equator is adopted. It would of course be idle to attempt to offer final numbers, where the difficulties attending observations and the differences between the results of the most experienced and favorably-circumstanced observers are so considerable.

	″	Miles.	
Mercury	6·35	2,850	
Venus	16·95	7,550	
Mars	9·305	4,150	
Jupiter, Equat.	197·47	88,200	Compression $\frac{1}{15\cdot54}$
" Polar	184·76	82,500	
Saturn, Equat.	166·82	74,500	Compression $\frac{1}{9\cdot10}$
" Polar	148·50	66,300	
Uranus	68·57	30,600	
Neptune	67·26	30,050	

In fixing upon the apparent diameters of the bright planets it has been desired to adopt values which shall represent the actual arc values that are presented by the true diameters at the earth's mean distance. Many observations would assign larger values, but undoubtedly less trustworthy for computing real dimensions. As is well known, preference in such case is to be given to double-image over wire-micrometer measures, yet even if we confine ourselves to the former mode of observation we by no means secure great consistency of results.—*Nature.*

2. *Small Planets recently discovered.*—Upon p. 474, vol. i, (III) of this Journal, was given a table of the elements of the small planets recently discovered. We continue that table below, down to No. 143. A few of the later planets of the previous table are repeated. The elements as far as No. 136 are from the *Berliner Astr. Juhrbuch* for 1877.

No.	Name.	Time of discovery.	Discoverer.	Mean dist.	Angle of Eccent.		Incl.		Long. of node.		Long. per.	
					°	′	°	′	°	′	°	′
107	Camilla,	Nov. 17, 1868.	Pogson.	3·5602	7	3	9	48	175	41	112	50
108	Hecuba,	Apr. 2, 1869.	Luther.	3·2113	5	46	4	24	352	17	173	49
109	Felicitas,	Oct. 9, "	Peters.	2·6950	17	28	8	3	4	56	56	1
110	Lydia,	Apr. 19, 1870.	Borelli.	2·7257	3	51	5	58	57	19	329	28
111	Ate,	Aug. 14, "	Peters.	2·5927	6	3	4	57	306	13	108	42
112	Iphigenia,	Sept. 19, "	Peters.	2·4335	7	23	2	37	324	3	338	9
113	Amalthea.	Mar. 12, 1871.	Luther.	2·3767	5	2	5	2	123	12	198	58
114	Cassandra,	July 23, "	Peters.	2·6758	8	3	4	55	164	24	153	6
115	Thyra,	Aug. 6, "	Watson.	2·3795	11	11	11	35	309	5	43	7
116	Sirona.	Sept. 8, "	Peters.	2·7661	8	17	3	35	64	26	152	53
117	Lomia,	Sept. 12, "	Borelli.	2·9907	1	19	14	58	349	39	48	46
118	Peitho,	Mar. 15, 1872.	Luther.	2·4355	9	26	7	49	47	13	77	4
119	Althaea,	Apr. 3, "	Watson.	2·5804	4	48	5	47	204	0	12	27
120	Lachesis,	Apr. 10, "	Borelli.	3·1200	2	42	7	1	342	52	212	52
121	Hermione,	May 12, "	Watson.	3·4606	7	0	7	35	77	0	0	56
122	Gerda,	July 31, "	Peters.	3·2196	2	8	1	36	178	55	208	38
123	Brunhilda,	July 31, "	Peters.	2·6931	6	31	6	27	308	40	72	5
124	Alceste,	Aug. 23, "	Peters.	2·6297	4	30	2	56	188	26	245	42
125	Liberatrix,	Sept. 11, "	Pros. Henry.	3·0352	20	17	6	5	171	16	251	17
126	Velleda,	Nov. 5, "	Paul Henry.	2·4399	6	6	2	56	23	7	347	46
127	Johanna,	Nov. 5, "	Pros. Henry.	3·3211	11	46	8	33	31	23	101	24
128	Nemesis,	Nov. 25, "	Watson.	2·7500	7	13	6	15	76	45	12	21
129	Antigone,	Feb. 5, 1873.	Peters.	2·8758	11	57	12	11	138	1	240	57
130	Electra,	Feb. 17, "	Peters.	3·1298	11	46	22	56	146	8	20	18
131	Vala,	May 24, "	Peters.	2·4202	4	40	4	39	65	10	258	26
132	Aethra,	June 13, "	Watson.	2·6010	2	27	25	0	259	43	152	11
133	Cyrene,	Aug. 16, "	Watson.	3·0648	7	51	7	14	321	16	248	0
134	Sophrosyne	Sept. 27, "	Luther.	2·5673	6	44	11	36	346	30	66	52
135	Hertha,	Feb. 18, 1874.	Peters.	2·4315	11	45	2	18	343	59	318	45
136	Austria,	Mar. 18, "	Palisa.	2·3035	6	30	9	41	186	9	307	12
137	Meliboea,	Apr. 21, "	Palisa.									
138	Tolosa,	May 19, "	Perrotin.	2·4297	8	29	3	16	54	56	310	35
139		Oct. 10, "	Watson.	2·8141	2	57	8	19	348	37	115	32
140	Polana,	Oct. 13, "	Palisa.	2·7069	11	25	3	10	107	15		
141	Lumen,	Jan. 13, 1875.	Paul Henry.		12	54	11	33	318	59		
142	Siwa,	Jan. 28, "	Palisa.									
143	Adria,	Feb. 23, "	Palisa.	2·7525	3	49	11	32	333	41		
144	Vibilia,	June 3, "	Peters.									
145	Adeona,	June 3, "	Peters.									
146	Lucina,	June 8, "	Borelli.									

H. A. N.

3. *Diameter of the Sun.*—From a discussion of the Greenwich observations, 1836–1870, Dr. Fuhg obtains, *Astron. Nach.*, 2040, 32′ 2·″99 as the diameter of the sun deduced from 6827 measurements.

IV. OBITUARY.

SIR WILLIAM LOGAN, the Geologist, died at London, in June, in his 79th year, having been born in Montreal in April, 1798. Sir William Logan was head of the Geological Surveys of the Canadas from 1843 to 1871. After graduation at the University of Edinburgh, in 1818, he joined the mercantile house of his uncle in London, and later became manager, for the house, of coal mining and copper smelting operations in Swansea, where he studied so accurately the coal field of that region, that his maps and plans of it were later adopted by the Ordnance Geological Survey of Great Britain. In 1841 he visited the coal fields of Pennsylvania and Nova Scotia, and communicated several valuable papers to the Geological Society of London. The Geological Survey of the Canadian Provinces had its origin in his researches. He was remarkable for extreme care in investigation, and great caution in drawing conclusions—qualities which appear everywhere in his Geological Reports; and his labors have contributed vastly to our knowledge of North American Geology. He became a member of the Royal Society of London, and of many other learned societies. The Wollaston medal of the Geological Society was awarded to Mr. Logan in 1856, and he was knighted by Queen Victoria in the same year.

JOSEPH WINLOCK was born Feb. 6, 1826, in Shelby County, Kentucky. Graduating, in 1845, at Shelby College, he afterward held the professorship of Mathematics and Astronomy in that institution until 1852. The remainder of his life was passed chiefly at Cambridge, Mass.; but he spent some months at the U. S. Naval Observatory in Washington, and for more than a year was at the head of the mathematical department of the U. S. Naval Academy at Annapolis. He was twice made Superintendent of the American Ephemeris, finally quitting this office in 1866 to take the post of Phillips Professor of Astronomy at Harvard University, and in that capacity to serve as Director of the Observatory. He held this office at the time of his death, June 11, 1875. His last illness was short, and did not appear dangerous until a few hours before its termination.

Professor Winlock was an excellent mathematician and astronomer, and had a remarkably retentive memory not only for facts relating to his branch of science, but for the sources of information concerning those facts. The originality of his mind, however, was chiefly shown in his suggestions for the improvement of astronomical instruments. These inventions were singularly simple and effective. Four among them deserve special notice in this place.

(1.) The mounting of large meridian circles in such a manner as to allow the piers to be shortened, so that the graduated circles are wholly above the piers, and the steadiness of the whole instrument is increased. The theoretical advantage of this arrangement cannot here be discussed; it has been tested by five years' experience at Harvard College Observatory with very gratifying results; it has been adopted in other observatories, and will probably come into general use.

(2.) The application of a diagonal eye-piece, moved by a rack and pinion, to any large telescope, in such a manner as to dispense with the customary "finder," and to enable the principal object-glass to be used in finding faint objects which are to be examined with the spectroscope or otherwise. This invention has also been for some years in use at Harvard College Observatory.

(3.) A method of registering spectroscopic observations by marking lines upon a silver plate without requiring the removal of the eye from the spectroscope, or the use of artificial light. Professor Winlock registered in this manner his observations of the solar eclipse of December, 1870, which he observed in Spain.

(4.) The use of a lens of long focus and of a plane mirror in making photographs of the sun. Apparatus of this kind was brought into daily use in July, 1870, at Harvard College Observatory. Priority in this invention is claimed by some other astronomers; but it does not appear that any one actually used the combination of the mirror with the lens of long focus until some years after Professor Winlock. It should also be noticed that in 1869 Professor Winlock first photographed the solar corona without enlarging the image by an eye-piece.

During his connection with the Observatory, Professor Winlock greatly increased its instrumental equipment, and also its pecuniary resources, by the aid of contributions from neighboring friends of science. In particular, the system adopted for furnishing electric signals from one of the clocks at the Observatory to various points in Boston and elsewhere, has been profitable alike to the Observatory and to the public. It illustrates Professor Winlock's practical good sense, that instead of introducing new clocks, controlled by that at the Observatory, at the places where the signals are received, he provided simple telegraphic apparatus for the reception of the signals every two seconds; a method much cheaper than the other, and in practice equally satisfactory.

In private life, Professor Winlock's amiable, though reserved, character greatly endeared him to his friends. A. S.

Prof. HEINRICH d'ARREST, of the University of Copenhagen, died on the 14th of June, in his fifty-third year.

The most important of the labors of this distinguished astronomer were the construction of two catalogues, the one of nebulæ observed by him at Leipzig, the other of nearly 2,000 nebulæ observed by him at Copenhagen. For these observations the Royal Astronomical Society of London awarded to him this year their gold medal.

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[THIRD SERIES.]

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ART. XXII.—*On the formation of Hail in the Spray of the Yosemite Fall;* by WILLIAM H. BREWER.

[Substance of a paper read before the California Academy of Natural Science, April 19th, 1875.]

THE Yosemite Fall pours into the valley from the north. It is in an open niche or recess of so wide an angle that the whole sheet of the Upper Fall (in April) is in the full sunshine, from before 8 A. M. until after 2 P. M. The Upper Yosemite Fall is about 1,550 feet high. Below it, the stream descends, by a series of cascades and falls, about a thousand feet more to the bed of the valley.

On the 14th of last April, in company with Mr. Galen Clark, the official custodian of the Yosemite valley and well-known mountaineer, I visited the foot of this Upper Yosemite Fall and observed the phenomena to be described. During our visit of four days to the valley, the sky was nearly cloudless, each morning perfectly so, a few cumuli only appearing each day at about 10 A. M. and disappearing at or before sunset. The air was warm, the temperature rising to above 70° F. each day, and sinking at night to perhaps 50° or less. The streams were all very high from the melting snow, which was abundant on all the heights above the valley.

In the winter a great "ice-cone" forms at the foot of this fall, the accumulation of frozen spray. That formed last winter was much reduced in size by thawing at the time of our visit. I then thought it perhaps 100 feet thick. Messrs. Clark and

Conway, the two persons most familiar with it, gave their estimates, respectively, as "60 to 100 feet" and "nearer 200 feet." This ice-cone rises like a wall in front of the sheet, and then slopes from its apex down stream for some hundreds of feet. The water pours behind it, finds it way beneath and emerges from an ice arch below, strongly reminding one, both in shape and general appearance, of the ice-arch in the glacier at the source of the Arveiron at Mt. Blanc.

Later in the season, as the stream decreases in volume, it clings to the wall for some distance near the top, but at this time it left the rock at the very crest, shooting well out into the air, falling the whole immense distance in one grand leap. As a member of the State Geological Survey in former years I had visited the valley several times, always later in the season, and never before saw the volume of water half so large. From observations made by Professor Whitney at other seasons, it is probable that at this time the amount of water passing over the fall was 250 or 300 cubic feet per second. A heavy storm a week or so before had swollen the stream, and mud and sand had been carried out in the spray, tarnishing much of the surface of this ice which had before been pure white.

As we stood on the rocks near and above this ice—it was half an hour past noon—certain appearances suggested to me that the spray which drifted over it was, in part at least, snow. To reach the middle of the ice, without ropes to cling to, was impossible, for no man could withstand the fierce blast. We ventured, however, as far as we could go, and where, at times, it seemed as if we would be hurled into the chasm below.

Between the wall of granite behind and the wall of ice in front the stream fell with deafening sound. Great volumes of spray belched from the open throat of this abyss and swept furiously over the ice-cone toward the valley below. In this tempest, which stung our hands and faces like shot, we found abundant *hail* or ice-pellets. Their structure could not be studied in the blinding blast to which we were subjected, but like hail-stones, they were of hard ice, tolerably uniform in size, and I estimated their diameter at one-tenth of an inch. They accumulated on our clothes and on the windward side of rocks which came up through the ice-cone near its edge. They were found also (rapidly melting) on the rocks by the side of and near the ice. Farther down upon the cone many depressions in the dirty ice were filled with what looked like new white snow, but which we believed to be fresh accumulations of this hail; from their position it was impossible to reach and examine them.

We retreated from the ice and then pushed our way back to the granite wall over which the fall pours, and went as near the sheet as it was possible to stand or breathe. No fresh light on the matter was here gained. Having no protection for the eyes but our hats, nothing could be seen distinctly, and if any hail occurred there I could not feel it. At a little greater distance from the fall, and where from time to time by the swaying of the sheet we were left sufficiently outside the spray to look upward, the near views were indescribably grand. More than a quarter of a mile above us the clear stream leaped out into the air and was soon torn into spray. It seemed as mobile as smoke and assumed new varieties of outlines each instant, so light and airy that it seemed as easily swayed by wind as lace, yet it struck with deafening thunder; the concussion was perceptible through the granite for some distance and it was only by this that the vast forces involved were appreciated.

Although foreshortened from our position, by an illusion its height appeared greatly and abnormally increased. The mass of spray increases downward so that the base of the sheet is several times wider and thicker than the top, forming a sort of curving truncated cone. The actual height is so vast and so much beyond ordinary experience that it excites the imagination and deceives the judgment, and when thus seen from below in the intense illumination of the midday sun in that clear climate, looking up and along this white, airy, changeable cone, its tapering from the observer seems due mostly to its distance, and by this false or imaginary perspective it seems to stretch upward toward the intensely blue sky to an immense but vague height.

We had no thermometer with us to test temperatures at or near the fall. At Leidig's hotel in the valley, which is one and five-eighths miles distant in an air line and a thousand feet lower, my thermometer showed the following temperatures for that day. At 6 A. M., 52° F.; at 2.30 P. M., 78½°; at 3.15 P. M., 79°; at 9 P. M., 58°; and at 6 the next morning, 50°. These were probably about the temperatures of the other days of our visit. I had no wet-bulb to determine the dryness of the air, but that the air was very dry was shown by the rapidity with which our saturated clothes dried.

It will be noticed that at the time when this hail was observed, the sheet was in the full blaze of the sun from top to bottom and the heat further reflected toward it by the naked walls of rock sloping toward it on either side, and that the air near was of a temperature above 70°, perhaps, however, much less near the top of the fall. We were fully convinced while there that the hail was then actually forming, and not that it was merely portions of ice torn from the great ice-cone and hurled along with the spray by the blast.

When I first visited this fall in June, 1863, we had intended to have tested the temperature of the water at the top and the foot, to see if the entire fall of 2,550 feet sensibly heated the water. We became convinced that the rapid evaporation then taking place would vitiate any results obtained, so the experiment was not tried as it involved too much labor to be expended for so unsatisfactory a result. And on examining this hail, the cause of its formation, which immediately suggested itself, was evaporation.

The stream was then swollen by the melting snow which was still deep on the heights, and was abundant in niches to the very crest of the fall (which has an altitude of about 6,600 feet above the sea). The volume of water each day was least in the early morning and increased much during the day, thus showing its source. In the Fall it appears to be "atomized" for 1,200 or 1,400 feet of its descent. A great volume of air is drawn into this great falling mass along its whole course, the sheet spreading as it descends. The quantity of air is so great that it pours outward on the bottom of the valley and is very perceptible as a cool current more than a mile distant from the base of the upper Fall. The air as sucked into the Fall is immediately cooled to 32° by the ice-cold water. As it passes in, it is very dry, and the rapid saturation within the sheet is sufficient to freeze a portion of the drops.

Prof. John LeConte, of the University of California, who is familiar with the locality, has suggested that perhaps the cooled air within the sheet is somewhat compressed and condensed in the base of the fall, and when liberated just outside, by its expansion, freezes a part of the spray.

ART. XXIII.—*Walker's Statistical Atlas of the United States.* (Second paper.)

IN a former notice of this excellent work we sketched its plan and scope, reserving for another article some further notice of its first part. This relates to the "Physical Features of the United States," and is the part to which students in the physical sciences naturally turn with most interest.

The first map relates to the "*River Systems*," and the first memoir to the "*Physical Features.*" The map was prepared by Gen. A. von Steinwehr, and the memoir by Prof. J. D. Whitney, these authors having worked independently of each other. About seven-eighths of Professor Whitney's sketch is devoted to the mountain frame-work or skeleton of the country, and that of the *River Systems* supplements this. One may be said to describe the anatomy of the country, the other its physiology. This map is by far the best of its kind we have yet seen of the country. Twenty-one drainage areas are denoted (each accompanied with certain statistical information), the whole naturally thrown into groups or systems, only one of which we will here notice.

According to this map, the Mississippi basin embraces about 1,258,000 square miles of our domain. A small portion of the Missouri basin extends into British America, perhaps about 22,000 to 25,000 square miles. If this be added, the area of the whole basin will be about 1,270,000 to 1,273,000 square miles. This is somewhat larger than the area usually given by the authorities most often consulted. The figures from the Report upon the Physics and Hydraulics of the Mississippi River, by the U. S. Topographical Engineers some years ago, are 1,244.000 square miles, while the various works on geography and physical geography usually give it from 1,200,000 to 1,250,000.

This "Great Central Valley," as a geographical feature of the continent, is, however, much larger, including on its southern borders portions which drain directly into the Gulf, and northward passing insensibly into the great areas which drain into Hudson's Bay and even into the Arctic Ocean.

The Mississippi basin, according to this map, contained at the last census, a population of about 16,292,000, exclusive of "Indians not taxed." The whole basin is divided into six parts,—the Lower Mississippi, Upper Mississippi, Ohio, Missouri, Arkansas and Red River basins. The basin of the Ohio has naturally the greatest population. Somewhat almond-shaped in outline, or like a leaf, veined with large rivers, its point reaching to New York, it was the natural channel down which emigration flowed westward to the greater valleys beyond. Its genial climate, fertile soil, its prairies here, and wealth of timber-land there, have so attracted the settler that it will probably long remain the most densely populated basin of the system. The "center of population" of the nation passed into it about forty years ago. Another century will probably find it still there. The area is given as 207,000 square miles (the Report on Hydraulics, &c., already cited, stating it as 214,000), or about one-sixth of the whole Mississippi basin, and its population, 7,800,000, is nearly half of the population of the whole basin.

The Basin of the "Upper Mississippi" is credited with an area of 179,600 (169,000 according to the other authority quoted) and a population of about 4,000,000. The Missouri basin has an area of about 528,000 square miles, and a population of about 1,524,000. If we add to this last area the estimate already given for that part outside the United States, it would give the entire Missouri basin an area of about 550,000 to 553,000 square miles. The Engineers' report previously cited estimates it at 518,000.

The estimated average annual rain-fall is given for each of the basins of the map. If now we continue our comparisons between the basin of Missouri and Ohio, we find 18 inches for the former and 43 for the latter. The first is probably too high. Plate V. is a "Rain Chart of the United States," prepared by Chas. A. Schott of the U. S. Coast Survey, under the direction of Professor Joseph Henry, from materials in possession of the Smithsonian Institution. On this map we find that much of the Missouri basin (indeed it appears to be much the larger part) is there accredited with 12 inches or less. But on the estimate of the first figures, if we computed the amount of water falling in each entire basin, we find them very nearly equal,—that in the Missouri basin to that in the Ohio basin as about 1·1 to 1. Considering the vastly greater area for evaporation in the former, 2·65 to 1, and the dryer atmosphere, we are rather surprised that the comparative excess of water discharged by the Ohio is not greater than it is. According to the report cited, the mean discharge of the Missouri River is 120,000 cubic feet per second, of the Ohio 158.000. On the rain-chart, much or most of the basin lies inside of the line of 12 inches annual rain-fall, and only the very small part that lies below Atchison, Kansas, has 32 or more inches. On the same chart the lowest rain-fall of the Ohio Valley is 36 inches, reaching 62 in its extreme southern part.

Plate VII, "Temperature Chart of the United States" (of the same authority as Plate V.) shows that the mean annual temperature of the Missouri basin is about 10° F. less than that of the Ohio. The isothermal line of 48° F. at lon. 110° W. is in lat. 48°. It sweeps down near or a little above the center of the Missouri basin, crossing the river at Fort Randall, sinking to lat. 39° in southern Iowa (more than 600 miles south of our starting point), then it rises again eastward so as to keep entirely outside of the Ohio basin. Very nearly all the Ohio basin lies between the isothermals of 50° and 60°. The Missouri basin has a much wider range.

Even more suggestive is a comparison of these two basins with plate VIII, which is an "U. S. Signal Service Chart" showing the mean temperature at 4.35 P. M. of each day "of the hottest week of 1872" (by red lines), and of the 7.35 A. M. observations "of the coldest week" of the winter following (by blue lines). It will be seen that this chart does not show the actual range of temperature as indicated by the single extreme maximum and minimum observations of the year. It is rather a comparison of the coldest "spell" of the year with the hottest. On this, we find the greatest difference near the eastern base of the Rocky Mountains, or on the plains eastward. At Fort Benton this difference is upward of 102° Fahr. It diminishes eastward to the eastern base of the Apallachian system, amounting to 80° in northern New England, but south of New York it is usually less than 70°. The hot line of 85° in eastern Dakota is north of the cold line of −20, and passing eastward, successively cuts every cold line to +20, which it crosses in eastern Delaware. The hot line of 90°, which is first traced above Fort Benton, crosses the Ohio River near Cincinnati, and ultimately reaches the Atlantic near Cape Charles. The hot week was as hot in the Missouri basin as in that of the Ohio. During the cold week, nearly all the Ohio basin was between +5° and +20°, while in that of the Missouri the average was from zero to −20 and lower. Space forbids a further comparison of these two basins, or any notice of others we intended to have dwelt upon.

This last chart is of much interest to the student in biology. In Dakota (beyond which the lines are not traced) we have a difference of 105° F., and over large areas of the plains, a difference of 100° F. On the 91st meridian, the hot line of 90° is 330 miles north of the cold line of +10°; they cross each other near Cincinnati: and on the 83d meridian they are again 340 miles apart but in the reversed order. Again, the hot line of 85° and the line of +20° are together: in fact they cross in southeastern Delaware. They separate westward, the cold line crossing extreme southern Arkansas, the hot line running up to Dakota, more than 950 miles north. Again, in the longitude of Raleigh, N. C., the minimum line of +30° is but 130 or 135 miles from the maximum line of 90°. Passing westward, the former descends to lat. 27° in Texas, the latter rises to lat. 48° in Montana, equivalent to a distance of about 1450 miles on the meridian. These climatic peculiarities, taken in connection with the nature of the storms of winter and the sudden changes of temperature sometimes occurring there, must have much greater influence on the distribution of life than the annual means of temperature and rain-fall. It is, perhaps, practically there the controlling condition. That a dry and hot climate may have a flora and fauna rich in species is illustrated by South Africa. But this "middle region" is poor in species, and the whole of it is without forests as shown in the "Map of Woodlands."

The excellent geological map, compiled by Professors C. H. Hitchcock and W. P. Blake, we have already noticed. On it, the "Carboniferous and Permian" are shown as a single member. That portion lying east of the 100th meridian forms a broad doubly curved belt, like a huge inverted letter S, reaching from New York to Texas, with a few outlying patches, the largest of which is in Michigan. The upper division of the Carboniferous series, "the Coal Measures," is shown on a large (double plate) "Map of the Coal Fields of the United States," by Prof. C. H. Hitchcock. We have heard several persons speak of this as the most impressive map of the series; it may perhaps be so to many who love to dwell upon the future resources of our nation. The map is accompanied by an explanatory memoir. The following are the areas given of the groups specially treated.

New England basin	750	sq. miles.
Anthracite basins of Pennsylvania	472	"
Appalachian Coal Field	59,105	"
Michigan basin	6,700	"
Illinois basin	47,188	"
Missouri basin	84,343	"
Texas Coal Field	6,000	"
	203,808	

In addition to this, there are a few small areas of Triassic coal, amounting to a few hundred square miles at most. The various important deposits of coal west of the 100th meridian are not noted. Indeed the data does not exist to satisfactorily show the areas which they cover.

When we consider the educational and statistical value of the Atlas under consideration, it is greatly to be regretted that a large edition was not ordered by Congress, to be sold at the lowest cost of manufacture. As it is, the lithographer, Mr. Bien, is allowed to issue an edition at his own risk, and this will allow all who wish the work to purchase it at a rate which under the circumstances is very reasonable. W. H. B.

ART. XXIV.—*On Southern New England during the melting of the great Glacier*; by JAMES D. DANA. No. I.

GLACIAL scratches, southeastward in direction,* on the Taconic summit, Mt. Everett, in the southwest corner of Massachusetts, at a height of 2,600 feet above the sea, afford evidence that the ice which covered New England in the Glacial period overtopped this mountain, and had an elevation in that region not much under 3,000 feet. Similar facts in the White Mountains place the height there at not less than 5,800 feet.† Calculating the slope of the upper surface of the glacier over New England from these data, it follows that the height above the region of New Haven, in Southern Connecticut, may have exceeded 2000 feet, and could hardly have been less than 1500. With such facts in view, we may have some appreciation of the amount of material that was at hand, when the melting-time began, for making or deepening under-glacier streams and lakes, and, at last, swelling the waters to universal floods. The sinking of the land that took place after the ice had reached its height—placing the site of Montreal 500 feet below the sea level, making Lake Champlain an arm of the great St. Lawrence Gulf, and carrying other high-latitude lands much below their present level, a movement favoring greatly the wide extension of the floods—presents a reason for the continental change of climate which began the thinning of the glacier and finally hurried on its dissolution. The Champlain or Fluvial period—the period of this low level of the land, or the middle Quaternary—comprises, first, an era well called the *diluvial*, or that of the melting glacier and of the tumultuous floods thus occasioned, and, secondly, the *alluvial*, characterized by more quiet fluvial action. The following observations relate more especially to the earlier of these divisions of the period.

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* Hitchcock gives the direction S. 18° E. The author observed S. 27° E.

† Prof. C. H. Hitchcock has informed the writer in a letter dated Aug. 13, of the current year, that he has found true transported bowlders on the very summit of Mount Washington; and this may authorize a higher estimate.

Three prominent facts appear to be established by the Champlain deposits of Southern New England.

1. The occurrence of a vast flood during the closing part of the melting of the glacier, in which other parts of New England participated.
2. The absence of marine life from Long Island Sound through the Glacial period and the early part of the Champlain period.
3. A participation in the subsidence which affected the regions farther north.

I. THE FLOOD FROM THE MELTING GLACIER.

1. NEW HAVEN REGION.

Facts proving that a great flood closed the era of melting were brought out by me in my memoir on New Haven Geology, published, in 1870, in the Transactions of the Connecticut Academy of Sciences. Since that paper appeared I have made various additional observations which I think demonstrate its occurrence still more positively, and afford also, some idea of its extent and violence.

In order that the facts may be better appreciated, a map of the New Haven region is here introduced. The region properly includes the country about the New Haven harbor or bay having the eastern slope of the high land of Orange and Woodbridge (100 to 400 feet above the sea) on the *west*, the Mt. Carmel range, nine miles from the city of New Haven, on the *north*, and the hills of East Haven, east of the bay and of the valley of the Quinnipiac, on the *east*. The rocks of Orange and Woodbridge, adjoining the region, are, like those farther west, metamorphic, being chlorite slate and chloritic hydromica slate; while those underlying the rest of the surface, beneath the Quaternary, are Triassic (or Triassico-Jurassic) red sandstone and conglomerate, with intersecting ridges of trap. The trap ridges include the West Rock Ridge, commencing on the south at W, West Rock proper; P, or Pine Rock; M Wh, or Mill Rock ridge, having Whitney peak as its highest point; E, East Rock; Mt. Carmel to the north; Rt, Rabbit or Peter's Rock; and Saltonstall ridge, just west of Saltonstall Lake. The other hills to the eastward, with a few exceptions, are sandstone hills, or sandstone and trap, with a surface of drift; and between these hills south of Mill, Pine and West Rocks, extends the broad and nearly level New Haven plain, a region of stratified drift. The principal rivers, it will be observed, are three: (1) the Quinnipiac on the east, the largest; (2) Mill River, or the central; and (3) West River, on the west, with Wilmot Brook as a prominent tributary. The Quinnipiac has very broad flats on either side, as the map indicates; and, for the most part, they are not above high tide level all the way to North Haven, six miles north of New Haven. Mill River has a rapid descent from Mt. Carmel to Whitneyville (V, within a mile and a half of the city), and West River is of similar character down to Westville. The long lake above Whitneyville (V), has been made by a dam, 40 feet high, at V. Saltonstall Lake on the east, occupies a natural depression. Height above mean high tide of West Rock at W, 405 feet; of East Rock, 360; of Mt. Carmel, 736; of Rabbit Rock, 373.

The evidence afforded by the deposits of the New Haven region is of three kinds.

1. *Structural:* the flow, when it set in, having made its mark in some places on the structure of the beds it deposited.

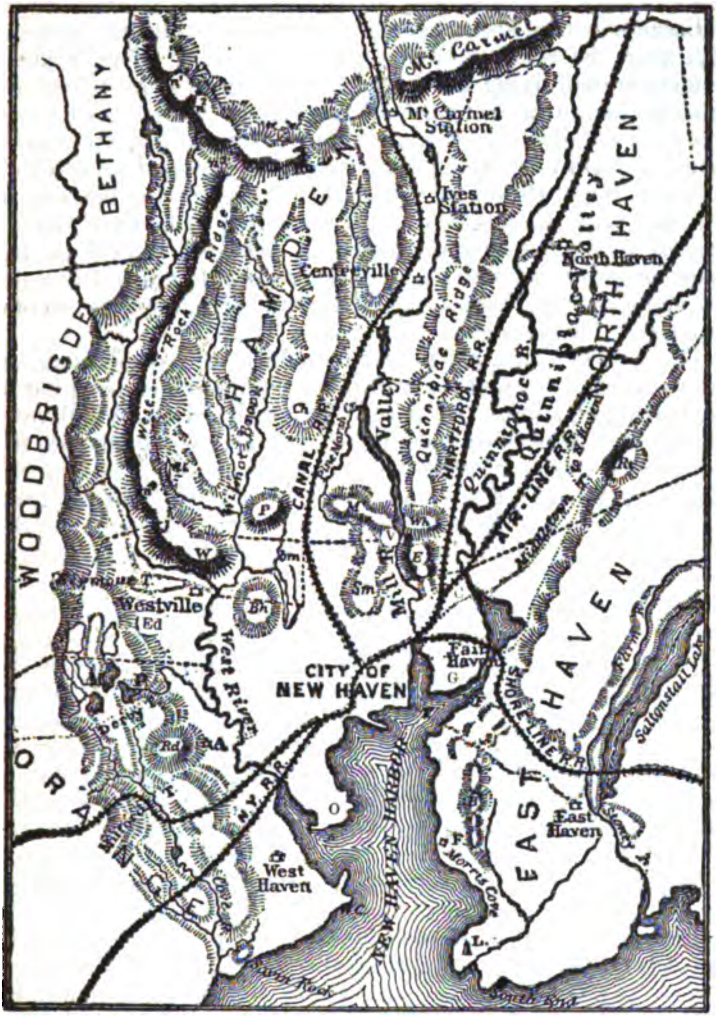
2. *Lithological:* the flood, where the current was strongest, having made gravel and cobble-stone deposits as a topping over the finer beds before laid down.

3. *Denudational:* the flood having produced a large amount of denudation throughout the drift formation.

1. *Structural evidence as to the flood.*—The Quaternary deposits about New Haven bay underlying the New Haven plain are estuary deposits of *stratified drift.* The average height of the upper half of the plain above mean high water level is about 42 feet; from this, there is a slope seaward of 8 to 10 feet a mile. The deposits consist of (1) sand; (2) sand and gravel; (3) rarely, of laminated clay; and in some large regions (4) layers of coarse pebbles and cobble stones.

Over the hills that rise above the level of the plain there is only *unstratified* drift, except along the small water courses.

MAP OF THE NEW HAVEN REGION.



Explanations of the Map.—A, Allingtown village. B Beacon Hill. Bh, Beaver hills. Ch, Cherry Hill. E, East Rock ridge, consisting of East Rock proper to the northwest, Indian Head next to the south, and then Snake Rock. Ed, Edgewood, the estate of Donald G. Mitchell. F, Fort Hale. *F*, Ferry Point, or Red Rock, on the Quinnipiac. J, Judges' Cave, on the West Rock ridge. L, Light House. M, Mill Rock. M P, Maltby Park, three of the proposed lakes of which are constructed. O, Oyster Point. P, Pine Rock. Rd, Round Hill. Rt, Rabbit or Peter's Rock. Sm, Sachem's ridge. T, Turnpike; also Tomlinson's bridge, across the head of New Haven bay. V, Whitneyville. W, West Rock, the south end of West Rock ridge. W C, West Cape, or West Haven Point. Wh, Whitney Peak. W. L, Wintergreen Lake, just north of Wintergreen Falls. Wn, Warner's Rock. *bm*, Beaver Pond Meadows; *m*, Mineral Spring, southeast of North Haven; *n*1, *n*2, *n*3, *n*4, different notches in the West Rock ridge; *n*1, *n*2, the Upper and Lower Bethany Notches; *n*3, the Hamden Notch; *n*4, the Wintergreen Notch.

Scale 4-10ths of an inch to the mile.

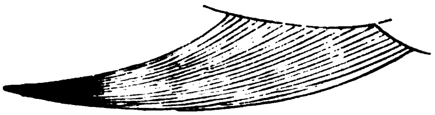
This unstratified drift consists of sand and gravel, with numerous scratched bowlders, without any underlying bowlder clay.

Large bowlders are rarely met with in the stratified drift except near the rocky bottom on which it rests. The Triassic sandstone underlying the New Haven plain in some places approaches within a few yards of the surface of the plain; and when so, the opening of trenches for sewers or other purposes brings to light a layer of gravel resting on the sandstone, which is full of large scratched bowlders, many over a cubic foot in size, and occasionally one over five hundred cubic feet. In the regions of unstratified drift, that is, above the level of the plain, bowlders are many and large, especially along the western margin of the region; and several of trap, are between 500 and 1000 tons in weight.

These facts teach, as I have elsewhere remarked, that, in the melting of the glacier and the accompanying dropping of the earth and stones, the part of the material which fell over the dry land went down *unstratified;* and that which fell into the waters was *stratified*, excepting a bottom portion made of the earth, gravel and great stones that were the first to drop from the ice. There is no evidence that the stratified drift of the plain is newer than the main part of the unstratified drift over the hills.

The stratification of the stratified drift is in almost all parts, except where too coarsely stony for it, of the *flow-and-plunge* style, as represented in the cuts on pages 173, 174. One of the parts of a layer having this structure is represented of the more common form, in the annexed cut, (fig. 1.) The oblique lamination indicates a violent onward movement in the waters; and the division of the layers into parts proves that there was a heavy plunging in connection with the rapid flow. A single obliquely laminated layer is often one to two feet in thickness, and in one case observed it was eight feet; and each must have been formed at a single onward movement of the plunging waters. This structure of the stratified drift hence shows that water and earth were let loose at the time in immense quantities; that there was a free flow of both which could have been well produced through the melting and discharge of a vast glacier; and that, consequently there was a very rapid piling up of the layers of gravel and sand.

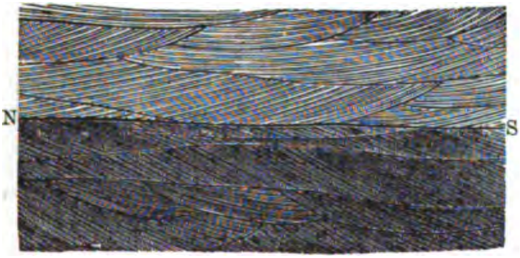
1.



In this oblique lamination which characterizes the deposits of the plain, the little laminæ rise, with only an occasional exception, *to the northward;* that is, dip to the southward, and hence prove that, throughout the New Haven estuary—then six miles in depth if we reckon only to Pine and Mill Rocks, and over half that in width and opening (as now) to the southward —the deposition took place under the pushing and plunging action of the *incoming* tide, the tidal waters having been forced into heavier plunging than usual owing to the violent resisting flood of fresh waters in front.

This rise to the northward in the oblique lamination (or dip to the southward) prevails except at the mouths of the valleys. The beds at the lower extremity of the Quinnipiac valley, just southeast of East Rock (E), afford one of these exceptions. At this place, along the cut of the Air-Line railroad between Mill River and the Quinnipiac, the stratified drift-deposits, which here have their full height or 42 feet above high tide level, are exposed to view to a depth of about 25 feet. Through the northeast half of the cut (near C) where it is within the range of the Quinnipiac valley, the beds of the upper 20 feet have the oblique lamination *rising to the southward*, that is, in a direction the opposite to that in the underlying beds, and the opposite to that which characterizes the whole thickness of the deposit over the New Haven region except at the mouths of the valleys. The following cut represents a part, six feet in height, of a vertical section, and shows the plane of junction NS between the upper and lower portions; above NS oblique lamination rises *to the southward*, and below NS *to the northward*. In addition to this difference of direction, the beds below are brownish-red, and those above rusty brownish-yellow.

2.

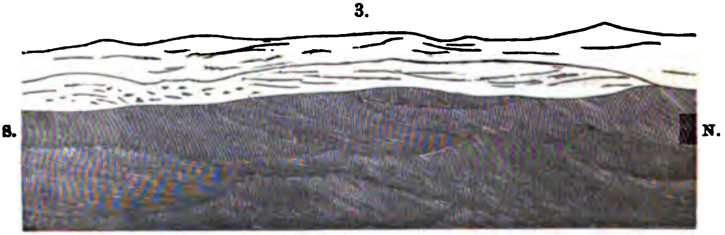


The deposits present this difference above and below for half a mile along the cut. But passing beyond this westward, or toward Mill River, out of the Quinnipiac Valley, the upper stratum gradually loses its distinctive character and becomes like that of the plain deposits elsewhere. This change on passing out of the range of the Quinnipiac proves that the existence of such an upper stratum was dependent on the flow of the river and not on any variation in level or depth, or on any subsequent action of currents. The drift deposits have a uniform level at top quite to the Mill River channel, and were all one in simultaneous origin.

Now the flow of the Quinnipiac waters must have undergone some change before the last twenty feet of sand and gravel were laid down, and it must have been a change in volume and violence. Owing to this change the current in the river became so powerful as to overcome the action of the incoming tide and take charge itself of the deposition of the earth and gravel. In other words, an extraordinary flood set in, extraordinary even at a time when a violent flood had been already long in progress; and the flood waters did the deposition even in the face of the tide.

Similar evidence of the flood should exist in the other valleys of the region. I have observed it in the valley of Wilmot Brook, near the southeast angle of West Rock (W on the map). The oblique lamination of the upper layer at the place rises to the northward, or dips southward, as at the termination of the Quinnipiac valley, and thus the place bears its testimony to the flood. The same evidence was found nearly a mile higher up the Wilmot Brook valley, where the road turns, at Selden's, to go to Wintergreen Falls and Lake, and the annexed cut represents a portion of the section.

3.



Section of stratified Drift up Wilmot Brook.

Owing to there being no good sections of the stratified drift at the extremities of the Mill River and West River valleys, I have not obtained any facts from these points.

2. *Evidence as to the flood in gravelly and stony formations.*—The material of the stratified drift-formation over a large part of the New Haven region is sand, with only small pebbles where any occur. But adjoining the river courses through the plain, especially Mill River and West River, it consists to a great extent of large pebbles or stones, and much of it of cobble-stones, many of which are six to eight inches in diameter.

The Quinnipiac Valley deposits are an exception, because the wide valley was at the time an interior harbor nearly four miles long and a mile in average width, with North Haven at its head, and much of it was over 50 feet deep. It was hence a level surface feeling the tides in their full force to its northern limit. Hence the flow of the waters through it would not have been turbulent like that along the streams of rapid descent. That this was the condition of the region is proved by the fact that the region is now one of wet peat meadows flooded at high tides. Further, the height of the stratified drift at North Haven, and at a point four miles below (near E) is alike, being not over 45 feet above the level of the flats.

The West River region shows its stony character strikingly at Westville where it leaves its narrow valley. The stony beds of the deposits follow the course of the stream southward through the New Haven plain either side of the river flats; but the stones diminish in size, at first rapidly, and then gradually, and two miles down the stream there are only pebbly beds with sand; thus showing the slower flow through the estuary of the era.

The stony beds along the course of Mill River are much more remarkable than those of West River, both in extent and coarseness. The area they occupy south of Whitneyville rapidly widens southward, becoming one-quarter to half a mile broad. The coarser part is situated to the east of the present channel of the river; and it extends southward, with a straight course to the head of the bay (south of VEN in the map), where it passes beneath the water to a distance yet undetermined. The beds consist partly of cobble stones 6 to 10 inches in diameter. The coarseness diminishes both to the east and west. On the east side of the river at the State street bridge (which on the map is near M in Mill River), where the stony stratum is 20 feet thick, the stones and pebbles are much smaller in average size than on the west side, and they diminish on going farther eastward.

Since such stony regions indicate where the waters run most violently along their valleys or through the estuary, it is evident that the main flow of Mill River from East Rock (E) southward was to the east of the course it now follows;* and up the same channel moved the great central tidal flow of the harbor. The rushing waters, combining the flow of the river with that of the ebbing tide, carried the finer material down stream, leaving the stones behind; and this finer material was made to contribute to the sandy stratified drift-deposits along the west side of the bay (where the inflowing tide now makes its chief depositions of sand), as well as to the Sound.

Now these stony beds testify to the flood not only by their presence, but more especially by their *belonging mainly to the*

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* Just south of the Shore-Line railroad, the coarse beds are wanting for nearly a third of a mile west of Mill River, or to Franklin st., as I learn from Mr. P. Chatfield.

upper fifteen or twenty feet of the formation, and therefore by their being the last work of the melting and discharging glacier. A coarse stony stratum above and fine below is found through all the Mill River stony region.* Along the sections made through the New Haven plain to the level of the river flats in grading the streets, the lower part of the exposed section is mainly of sand, while the upper is gravel and stones. The same has also been found to be true in the excavations for sewers in that part of the New Haven region. At one of these (at the corner of Orange and Laurel streets), the stony stratum was 12 to 15 feet thick, and below it the beds were mostly fine sand, the lower stony layer resting on a bed of fine quicksand. The vicinity of West River presents similar facts.

Such a transition in the drift deposits from the production of sand beds with but little fine gravel to that of beds of coarse gravel and large stones—partly cobble stones—proves that there was an equivalent change in the flow of the waters. These waters were in rapid plunging flow when the lower stratum was deposited; for the beds are characterized everywhere by the flow-and-plunge structure, and a foot is a common thickness for the bed made by a single plunge. But, however great the previous violence there was an increase afterward to a vaster flood.

The partial decline of the flood is also marked in some portions of the deposits by the return to sandy beds in the top portion attending a narrowing of the stony region. On the western margin of the Mill River stony area, in Grove street above its intersection with Church, the stony beds reach the surface for 200 feet above Church street; but from that point westward, the stony portion thins, and in place of its upper beds there are sandy and fine pebbly beds; in 200 feet this upper sandy part has become six feet thick: indicating thus the narrowing of the more violent part of the Mill River flood, as its season passed, or the melting of the glacier became completed.

In the region of the Quinnipiac valley, three to six miles north of New Haven, on both the east and west sides of the present lower flats, there are beds of fine clay, from a few feet to thirty-five or more in thickness. The upper surface is but little, if at all, above high tide level. As this Quinnipiac region was then (in the Champlain period) a great interior harbor or arm of the bay, over fifty feet in depth, still water would have existed on one or both sides of the main flow; and these clay beds, all finely laminated, indicate the positions of such still-water areas. The clay beds, when followed a few scores of yards to one side or the other, sometimes become sand beds—as at Quinnipiac, half way to North Haven; and this is demonstration that their existence depends not on the kind of deposits there let fall by the glacier, but on the quiet condition of the waters. There is also proof at Quinnipiac, as brought to my attention by Mr. S. P. Crafts, that the depositions took place before the ice had all melted; for a large bowlder—four feet in diameter—has been taken out of the clay from a depth of six feet, and some intercalations of gravel seem to have had a similar origin. At the same place, fifty feet to the west of the clay pit, a very coarse, stony stratum of stratified drift overlies the clayey stratum; the fact that the latter exists below the stony beds having been ascertained by Mr. Crafts by means of a drill, which descended through and brought up some of the underlying clay. This overlying stony bed was made by violent currents that succeeded to the still waters of the place, and these violently flowing waters were probably those of the great flood.

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* When my Memoir on the New Haven region was written I had not recognised the distinction between the upper stratum and the beds below.

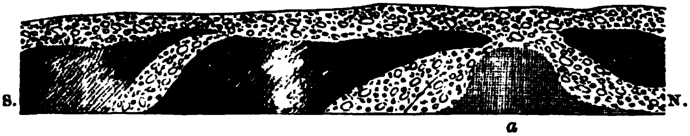
3. *Evidence as to the flood in its denudation of the stratified drift deposits.*—Along the *west* side of Mill River, where the beds are stony and of the coarsest kind, the plain is 10 feet below the usual level, the height not exceeding 30 feet above the river flats: while on the west side of the river near the State street bridge, where the stony beds are less coarse, the height is 40 to 42 feet—the full height of the New Haven plain in that latitude. The lower level on the west side continues south to the harbor, the surface sloping in that direction. The deposits of stones must have been made largely by the washing away of the finer material from the coarse—a denuding process; and it is reasonable to suppose that the lower level along the west side of the broad Mill River region was thus produced. But the river did not act alone in this; for the river-channel was also, as stated above, one of the sweeping grounds of the tidal waves and currents. Hence it is that this region of low level slopes and widens much southward, extending over half the width of Grape Vine Point (G, between the mouths of Mill River and the Quinnipiac), where the height is reduced to 10–20 feet.

The upper surface of the Terrace plain, near the junction of the Air Line and Hartford railroads, bears evidence of erosion to a yard or so in depth, over large areas of its surface, and of a subsequent deposition of fine sand.

At Quinnipiac the upper surface of the clay bed, alluded to above, has depressions that are results of erosion, and probably the eroding waters were those of the flood that deposited the overlying gravel bed.

A remarkable example of erosion of the drift deposit by the rushing waters of the flood was exposed to view in 1872 on Blake street, directly south of the extremity of Wilmot Brook valley, (on the map between Bl and West River). The following cut exhibits at *a* a portion of the previous deposit of fine sand, projecting up and enveloped in the coarse gravel that the tearing waters were bearing along down stream.

4.



Section of stratified Drift on Blake St.

Various depressions over the New Haven plain may be owing to the action of the flood, which cannot now be distinguished from those due to later denudation. But there are some isolated depressions 20 to 25 feet in depth, and one, Beaver Pond Meadows, having an area of an eighth of a square mile which have no such origin. These appear to be mainly due to the deeply excavating action of the ploughing glacier, they having been parts of underglacial water-courses in the Glacial period; they were excavated to so great a depth that the drift dumped into them by the melting glacier, or carried in by its waters, was not sufficient to fill them, while other shallower areas were built on up to the water's level of the era.

Thus the great flood left its mark not only in the structure and degree of coarseness of the deposits along the water-courses, but also in denudations of the drift-covered surface.

It is to be noted (1) that the upper stony stratum is not found over all the stratified drift of a valley; for through much the larger part of the New Haven region the upper stratum is sand. Such stony beds are to be looked for only where the flood waters ran most swiftly, while beds of fine pebbles, sand or earth exist where they were less rapid, and others of clay where the movement was slight or almost null.

It is to be noted, secondly, that the coarsest stony beds do not always or generally cap the highest terrace; for the fiercely-flowing waters often denuded the surface deeply and left the larger stones at one or more lower levels—levels that are now the upper portion of one or more of the lower terraces; but which, in the era of the flood, were either under-water flats, or else the broad bed of the flooded stream.

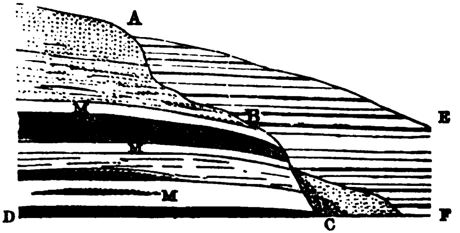
If the stony strata may have been merely the stony beds of streams, like the stony bed of any rapid stream in modern time, it may be questioned whether they afford evidence of a flood from the melting glacier. But such a stratum is common also at the top of the upper terraces, where it could have been made only by such a flood. Both Mill River and West River valleys afford examples of this. Again, since the Champlain deposits that lie *beneath the stony beds* are made of fine material, there is full evidence, whether the latter were formed over the bed of a stream or at higher levels, that an era of comparatively quiet work was in each case *followed* by one of coarse stony depositions, and hence that there must have been an increase in the flow at the last. The Glacial period scooped out the valleys and deepened them, and then came the filling from the unlading glacier; after the bottom had received part of the large bowlders that fell from the ice, there followed the deposition of sand and finer gravel, with clays in some places, and occasional bowlders; then, finally, the upper coarse beds were formed wherever the waters at the flood could carry off the finer material or carry in coarse.

The beds of gravel and cobble stones along Mill River extend for more than two miles through the New Haven plain, or what was then the great estuary, where there was no cause for rapid flow in the stream apart from a flood of enormous extent sweeping down the valleys.

Depositions subsequent to the flood.—It may be questioned whether the flood-made upper stratum in the vicinity of the river courses, which has been above described, may not have been deposited during some flood, or era of glacier melting, later than that of the early Champlain period closing the era of the great glacier. The evidence against such a supposition in the localities which I have examined is decisive. For, as I have shown, this upper stratum when followed laterally away from the river courses, changes into the ordinary stratified drift; or, in other words, the very same stratum bears the flood-marks near the river courses, and not so, or only sparingly, over other regions. The stratified drift of the New Haven region is evidently all one formation; it is one in structure, even to its top layer. All of later origin ordinarily overlying it is about a foot or less of blackish soil; and this is simply a portion of its surface sands modified by the growth of vegetation.

There are some portions where its upper surface was irregularly eroded, as has been explained, and there the surface was later built up to the old level by depositions of fine material, easily distinguished in kind from those below, indicative of changed conditions—but only a change from the flood condition to that of the more quiet subsequent part of the Champlain period. The terrace plain near the junction of the Air Line and Hartford railroads has been mentioned as one locality of this. The sands of the later deposit are here whitish, showing that they had been washed of their iron rust. A stratified bank of the same sands exists at the northeast end of the same railroad cut (that of the Air Line railroad). The stratified drift formation here drops suddenly in height, from the 42 feet above high tide level usual there, nearly to the low level of the broad lower flats of the Quinnipiac. The slope thus made was, in the Champlain period, the northward limit of the sand-flat which constituted the south boundary of the great inner harbor. Against this slope, spreading northward, there is situated a horizontally stratified deposit of fine white sand, which evidently owed its origin to the beating of the harbor waves against the sand-flat during the quiet half of the Champlain period, that is, after the ice and the flood had disappeared. Its stratification and its relation in position to the true stratified drift is shown in fig. 5, in which ACD is the stratified drift, made up of fine sand beds (M) and pebbly beds, some of them coarse pebbly drift, and AEF the subsequent deposit of white or sea-washed sand. The sands of the latter differ from those of the other beds not only in their white color, but also in their greater fineness, and in the absence of all pebbles. Through the washing of the waters against the shores, the sands were not only ground up, but they also lost almost entirely the oxyd of iron that tinges the quartz grains of the stratified drift. Near B, at the foot of the slope ABC, there is a heap of pebbles or stones and reddish sand, which evidently fell down the bank from the layers above when it existed as an exposed slope before the beds of whitish sands were deposited. These whitish sands, moreover, were laid down in even layers, free from the oblique lamination that occurs in the stratified drift. Thus we have evidence of some changes during the Champlain period after the drift was deposited; the wonder is that there is so little of such evidence.

5.



Section of stratified drift and later unconformable Champlain deposit, Air Line Railroad cut.

2. Outside of the New Haven region.

Evidence of the great flood produced by the melting of the glaciers is found along all of the rivers of Connecticut upon which I have had an opportunity for making observations.

Such opportunities are not often to be met with, since it requires an exposed section of the stratified drift of a valley—and one recently exposed, since the surface soon becomes concealed by slides down the slope. Moreover, in the narrower valleys of rather rapid descent, the waters were so tumultuous at all times that stony beds were thrown in at all levels. Ten miles west of New Haven, in the upper terrace of the Housatonic river, 90 feet in height above the flood level in the river, the upper stratum, for twenty to thirty feet in depth, is very coarse stony, and mostly sandy with finer stony beds below; and the top of a lower terrace is still coarser in its stones. I have observed similar facts in the Pequonnoc River valley, north of Bridgeport, Connecticut.

Again, to the east of New Haven, on the Thames at Norwich, the upper terrace, at the old cemetery, over one hundred feet above high tide, is exceedingly stony at surface. At the Navy Yard four miles above New London, on the east side of the Thames—where, as elsewhere on that side, there is only a narrow terrace on account of the crowding rocks—the following facts were presented in a cross section. The height of the terrace is about 30 feet. At a distance of 40 to 50 yards from the river there is at top a stony layer but two feet thick; and this rests on a horizontally stratified sand-bed.* Passing toward the river, the stony portion increases in extent, and also in coarseness, becoming a cobble-stone bed; and at the river, the whole of the formation, so far as in view, was of stones—that is the upper three-fourths, the lower part being concealed by the fallen gravel. Half way to the river along the section, irregular patches of sand beds, two or three yards in length were included among the irregular stony layers, after the style represented in fig. 4, (p. 178). Thus the flood-torn stony features of the formation increase toward the river; and some of the sands of the uprooted sand beds were left in patches among the stones.

To interpret the facts we have to note that the Thames is a wide and deep tidal estuary, or rather fiord, fifteen miles long; that, therefore, there is no seaward slope to make rapid currents. The earlier drift deposits, should hence have been of the finer kind, and in regular beds; only the extraordinary conditions of a glacier flood could have occasioned any other result. It must, moreover, have been a flood of enormous volume, with the violence of a cataract, to have produced, along such an estuary, so extensive erosions and cobble-stone deposits. Although I observed no other good section, I think it safe to say, judging from the stones which had fallen from some of the layers, that the formation, for at least half a dozen miles north of Groton (the part I examined), was very stony near the river, while mostly free from stones on the side toward the hills.

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* The section in this part showed only four feet of the underlying sand stratum. Fifteen yards nearer the river these four feet consisted of large stones and gravel instead of sand, being part of the stony stratum alluded to above.

On the opposite or western shore of the Thames, at Montville, six miles north of New London, where the terrace is one half higher, the facts are even better testimony to the same conclusions. The stony beds occupy a large part of the formation in a bluff close by the river between the shore and the railroad track, while forty yards farther from the river, to the west of the track, they are mostly confined to the upper third or less of the deposits. Further testimony with regard to the extent of the flood in the Thames will be given in another paper.

In a recent journey in Vermont, I found many examples of an upper stony stratum along the valleys. West of Ludlow, the upper stratum of the high terrace on the south side of the valley was coarse stony, while below the beds were mainly of sand. At Sutherland Falls, on the Otter Creek (or River, as it should be called), where fine sand was the main constituent of the formation—being fine enough for use in the marble mill—the upper portion for 8 to 20 feet was very pebbly, the pebbles mostly half to one inch in diameter. Just north of Pittsford, on the high terrace of a branch of Otter Creek, there is a coarse upper bed; and one still more coarsely stony on the terrace 50 feet or more below the highest, while the beds below were comparatively fine; also south of Rutland, in the upper part of the high eastern terrace of Otter Creek valley, especially near the junction with the valley of Cold River, and also with that of Mill River, west of the gap passed through by the railroad to Cuttingsville.

Professor Hitchcock, in his Report on the Geology of Massachusetts, makes the general statement that the material of the upper part of the terraced river formation generally differs from that below in being coarser. He refers on page 361 of his Geological Report of Massachusetts, to the fact, observed by him, that the clay stratum of stratified drift is almost always overlaid by sand, and then adds that, sometimes, toward the top, the clay is interstratified with beds of sand [evidence only that there were transitions from gentle to rapid movements in the waters] and then, above, the clay is absent, and "the sand becomes coarser, until, at the top, frequently small pebbles are found, and other evidences of agitation in the waters."

Conclusion.—If river valleys so widely apart over New England, and so many of them, bear evidence of a flood closing the existence of the glacier, we may reasonably conclude that all New England was, sooner or later as the melting progressed, swept by it. In fact, if the flood in any part was due to the rapid melting of the disappearing ice, it must have been thus universal; for the rapid melting would have been general.

The flood unquestionably preceded the ending of the ice; for, as already stated, only a melting glacier could have afforded the vast quantities of water required to fill and flood the wide valleys, and have set loose, simultaneously, the immense amount of sand, gravel, and stones, that were at the disposal of the waters.

The flood appears also to have risen rapidly to its height; for (1) the transition from the finer deposits to the upper coarser stratum is generally rather abrupt, and (2) at the lower extremity of the Quinnipiac valley, the change in the structure of the beds, from tide-made to flood-made, marked in the reversal of the oblique lamination, (page 173) took place along a well defined plane without any alternations, and was visible also in the color of the deposited sands.

Finally, the flood was a natural termination of that winter of winters: especially as the rigors of the Glacial climate had passed, and a lower level of the high-latitude lands was bringing on a time of warmer climate than the present—the era when even Britain was occupied by wild beasts of the warm temperate zone.

ART. XXV.—*On the Mechanical Work done by a Muscle before exhaustion, and on the "Law of Fatigue;"* by the Rev. SAMUEL HAUGHTON, M.D., Trinity College, Dublin.

IN the February number of this Journal, for the present year, a paper is published by Professor F. E. Nipher, containing experiments to illustrate the mechanical work done by a muscle (or a group of muscles) before exhaustion. In this paper Professor Nipher sets aside a former series of experiments of a similar kind made by him, without (as I believe) showing any satisfactory reason for depending more upon the latter experiments than upon the former. I am informed by a letter from Professor Hinrichs, of the Iowa State University, that both series of experiments were made in his Laboratory, and that, in his opinion, the experiments, which Prof. Nipher claims to have warned me against, were, in fact, as good as those he last made. I propose to establish the following propositions in the present paper:

1. That both series of experiments made by Professor Nipher are a valuable contribution to the facts of animal mechanics.
2. That they are not only consistent with the "Law of Fatigue" proposed by me, but illustrate both that law and my "Coefficient of Refreshment."
3. That Professor Nipher's discussion of his own valuable

experiments is worthless, as it is based on an empirical formula, which has no meaning, and leads to no further consequences.

4. That the "Law of Fatigue," which explains not only Professor Nipher's experiments, but so many other experiments also, is entitled to be received provisionally as a law of animal mechanics, and followed up by deduction to its legitimate conclusions.

I shall commence by describing some experiments* of my own, in which the muscles were kept in continued action, and no interval of rest was allowed. This condition is supposed in the "Law of Fatigue," and when it is departed from, a corresponding allowance must be made depending on the "Refreshment" afforded to the muscles during the interval of repose.

The approximate law of muscular action, which I have called the "Law of Fatigue," is thus expressed:

"*When the same muscle (or group of muscles) is kept in constant action until fatigue sets in, the total work done multiplied by the rate of work is constant.*"

I instructed a number of medical students, chosen at random, to raise dumb-bells of varying weight, one in each hand, in the transverse plane with hands supinated, and raising and lowering the weights in equal times regulated by the beat of a pendulum. This process was continued until the distress of the fatigue produced became intolerable, and the number of times each weight was lifted was noted. The students were required to count one, two, in time to the beat of the pendulum, so as to prevent them from counting the total number of lifts of the weight. Professor Macalister assisted me in these experiments, and one of us counted the number of lifts, while the other compelled the experimenter to observe the conditions of the experiment, which were—

1. To keep time with the pendulum.
2. To raise the weights in the transverse plane.
3. To supinate the hands.
4. To abstain from all bending† of the knees or spinal column.
5. To lower the weights so as to come down without velocity.

The 2nd, 3d, and 4th conditions are essential in order to confine the work done strictly to the same muscles of the shoulder, arm, and forearm.

The experimenter must be carefully watched in order to ensure the observance of these conditions; for he is impelled, instinctively and unconsciously, by pain, to bring in other muscular fibers to aid those which are suffering from the constrained and regulated motion. If this be permitted, the "Law of Fatigue" will appear to be violated, for of the muscular fibers actually employed in doing work some are quite tired out, and others not; whereas the "Law of Fatigue" applies only to groups of muscles, all of which are completely exhausted. For each experiment I chose twenty students at random, using altogether about fifty different students, and no individual was experimented on again until an interval of forty-eight hours had elapsed. The object of this arrangement was to avoid the effects of "training."

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* These experiments were communicated by me to the Royal Society on the 17th of June, 1875.

† To this defect we gave the name of "slinging."

In all cases there were three well marked stages:

1. The work done with ease.
2. Accompanied by respiratory distress.
3. Accompanied by pain in the muscles used.

During the last stage, great care must be taken to prevent changes in the posture and mode of motion, by which additional muscular fibers may come in aid of the fibers nearly fatigued.

Let W denote the total work done, and T the time of doing it; then, by the "Law of Fatigue,"

$$\frac{W^2}{T}=\text{constant.} \qquad (1)$$

If w be the weight held in the hand and α be half the weight of the arm, and n the number of times the weights are lifted: Since the time of raising and lowering the arms is constant, n is proportional to T, and the "Law of Fatigue" gives us the formula

$$(w+\alpha)^2 n=A, \qquad (2)$$

where A is an unknown constant.

In the following table I give the values of w and the mean value of n for 20 distinct persons.

The time of lift is in all cases *one second.*

TABLE I.—MEAN OF TWENTY EXPERIMENTERS.

No.	*w.*	*n* (obs.)	*n* (calc.)	Diff.
1	2·50 lbs.	131·80	128·0	+3·8
2	4·25 "	87·55	78·3	+9·2
3	5·87 "	47·35	53·5	−6·2
4	6·87 "	40·25	43·7	−3·5
5	7·75 "	34·60	37·1	−2·5
6	9·75 "	27·15	26·8	+0·3
7	14·00 "	17·20	15·4	+1·8

The column containing the calculated values of n was obtained from equation (2) by using the values

α=3·50 lbs.
A=4699 "

These values were obtained by finding the value of a, which renders A most nearly constant, or

$$\frac{\delta A}{A} = \text{minimum}.$$

This table gives 7 lbs. for the mean weight of the arm of all experimented on, a result which accords with known facts.

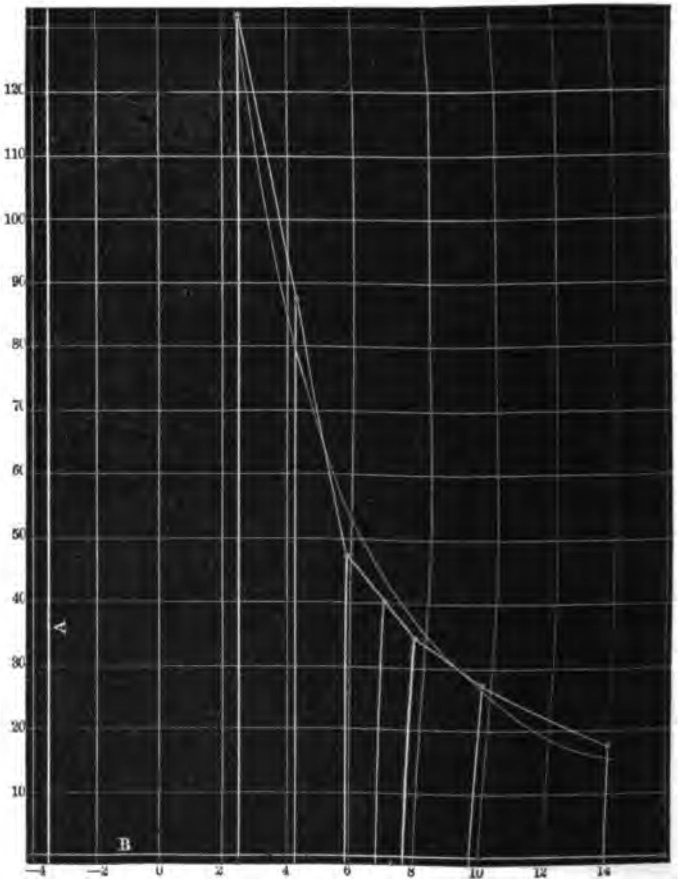

Diagram I.—Dr. Haughton. Mean of 20 Experimenters.
A, Asymptote $w+a'=0$; B, Asymptote $n=0$.
$(w+a)^2n=A$. $a=3\cdot5$ lbs. $A=4699$.

In the accompanying, diagram I, I have plotted the cubical hyperbola represented by equation (2); and also the several observations which lie sufficiently near the curve to justify me in considering the Law of Fatigue to be a first approximation to one of the fundamental laws of muscular action.

I have elsewhere* shown that the Law of Fatigue corresponds with other experiments based on different data.

If we consider the *useful* work only, we have from equation (2),

$$\text{Useful work} = wn = \frac{Aw}{(w+\alpha)^2}. \qquad (3)$$

This equation represents a cuspidal cubic whose ordinate has a maximum value, when $w=\alpha=$ half the weight of the arm.

The foregoing observations are in accordance with this deduction, as may be seen from table II.

TABLE II.—USEFUL WORK.

No.	w.	wn (20 experimenters).
1	2·50 lbs.	338
2	4·25 "	372
3	5·87 "	277
4	6·87 "	276
5	7·75 "	268
6	9·75 "	264
7	14·00 "	241

Professor Nipher has published two series of experiments, based on the principle of lifting different weights at a *constant* rate; and both series can be interpreted by means of equation (2), as I shall now show.

He has published other experiments based on the principle of raising the same weight at *varying* rates: these experiments are not only abandoned by himself, but contain internal evidence of error; for both these reasons I shall abandon them, but I feel bound to maintain Professor Nipher's first series at *constant* rates against his own repudiation, and as being, on Professor Hinrich's evidence, at least quite as good as those last made by him.

Professor Nipher's experiments differ in two respects from those made by me, tables I, II.

1. Professor Nipher allowed a *rest* equal to the time of *work*, whereas in my experiments, the work was incessant, as the arm came down without velocity.
2. From the mode of lifting the weights, there was a greater probability of other muscles assisting those intended to be fatigued.

The first of these causes would refresh the muscles and enable them to do more work than if not rested at all; and the second of these causes would bring in other muscles to their aid, and appear to make them do more than their proper work.

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* Principles of Animal Mechanics, London, 1873.

Let x represent the unknown weight held in the hand which would represent the total effect, both of *refreshment by rest*, and *aid from other muscles.*

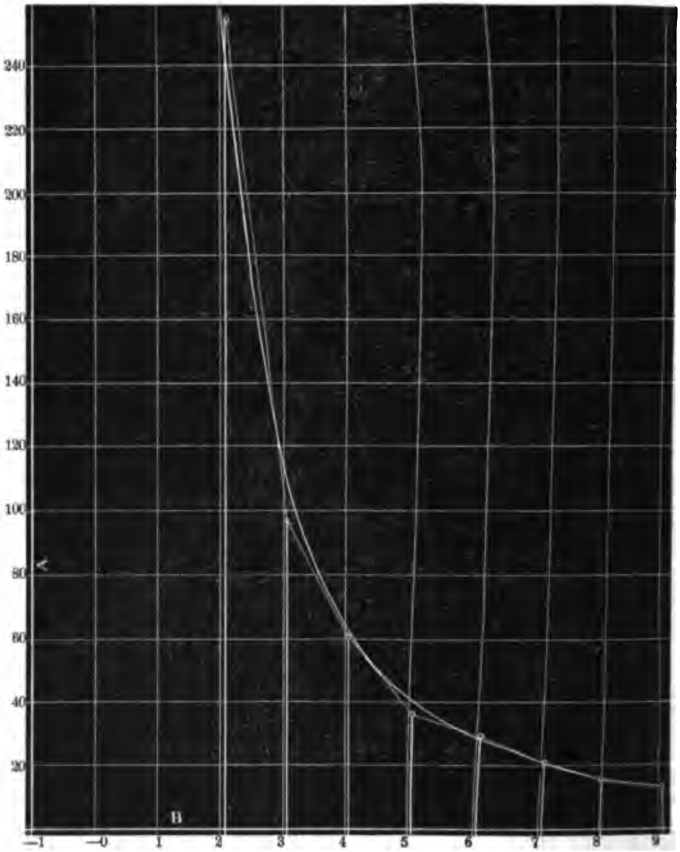

Diagram II.—Prof. Nipher. First Series.

A, Asymptote $w+a'=0$; B, asymptote $n=0$.

$(w+a^1)^2n=A$. $a^1=1{\cdot}05$ kil. A=1018.

Equation (2), which suited the conditions of my own experiments, must now be modified as follows:

$$(w+\alpha-x)^2n=A. \qquad (4)$$

or

$$(w+\alpha')^2n=A \qquad (5)$$

where

$$\alpha'=\alpha-x. \qquad (6)$$

α' is obviously less than α, and may even become negative, according to the quantity of *refreshment* and *aid* afforded to the laboring muscles.

Equation (5) accurately represents both Professor Nipher's series of experiments at constant rate, as is shown in the following tables:

TABLE III.—Prof. NIPHER'S FIRST SERIES.

No.	*w.*	*n* (obs.)	*n* (calc.)	Diff.
1	1 kil.	255	243	+12·0
2	2 "	97	109	−12·0
3	3 "	61	62	− 1·0
4	4 "	37·7	39·7	— 2·0
5	5 "	29·3	27·8	+ 1·5
6	6 "	21·5	20·5	+ 1·0
7	7 "	15·8	15·7	+ 0·1
8	8 "	12·8	12·5	+ 0·3

The calculated values of n were found from equation (5), the values of α' and A being found by the principle of least variation of A—

$$\alpha' = 1{\cdot}5 \text{ kil.}$$
$$A = 1018$$

In diagram II, I construct the cubical hyperbola, and show how closely the observations correspond with it.

As half the actual weight of Prof. Nipher's arm is 1·50 kil., it follows that in these experiments 0·49 kil. was lifted, by the *Refreshment* derived from rest, and from the occasional *aid* of other muscles.

TABLE IV.—Prof. NIPHER'S SECOND SERIES.

No.	*w.*	*n* (obs.)	*n* (calc.)	Diff.
1	3·0 kil.	152·5	145·5	+7·0
2	3·5 "	95·8	93·1	+2·7
3	4·0 "	67·2	64·7	+2·5
4	4·5 "	51·2	47·7	+3·5
5	5·0 "	36·9	36·4	+0·5
6	5·5 "	28·6	28·8	—0·2
7	6·0 "	22·7	23·3	—0·6
8	6·5 "	18·1	19·2	−1·1
9	7·0 "	14·5	16·1	−1·6

The calculated values of n were found from equation (5), the values of α' and A being found by the principle of least variation of A.

$$\alpha' = -1{\cdot}00 \text{ kil.}$$
$$A = \ 582$$

In diagram III, I construct the cubical hyperbola, and show how closely the observations correspond with it.

As half the actual weight of Prof. Nipher's arm is 1·50 kilos., it follows that in this series of experiments, 2·50 kilos. were lifted by the *Refreshment* derived from rest, and from the occasional *aid** of other muscles.

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* Including, possibly, those of the assistant employed in the experiments.

It is possible to calculate the coefficient of Refreshment from Professor Nipher's experiments by the difference between α and α'. Thus, in his second series of experiments

$$\alpha-\alpha'=x=2{\cdot}50 \text{ kil.}=5{\cdot}51 \text{ lbs.}$$

This weight was lifted through 0·70 m=2·29 ft. in 1·25 seconds —the muscles having previously rested for an equal time.

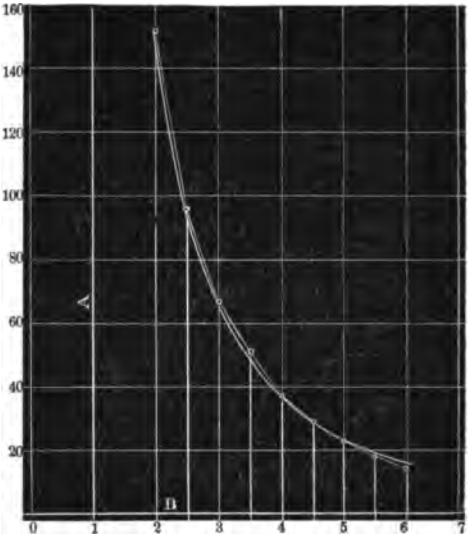

Diagram III.—Prof. Nipher. Second Series.

A, Asymptote $w+a'=0$; B, Asymptote $n=0$.

$(w+a'(^2n=A$. $a'=-1{\cdot}0$ kil. A=582.

Hence, in a cycle of rest and labor of 2·5 seconds duration, half of the time being devoted to rest, the work done, per second, is

$$\frac{5{\cdot}51\times2{\cdot}29}{2{\cdot}5}$$

and, assuming 34·5 oz.* of muscle employed, so as to reduce the coefficient to the units employed in my Animal Mechanics; we have finally

Coefficient of refreshment$=\dfrac{5{\cdot}51\times2{\cdot}29}{34{\cdot}5\times2{\cdot}5}=0{\cdot}142$ *ft. lbs. per oz. of muscle per second.*

The coefficient of Refreshment* given, from totally different experiments, by me, is

0·132 *ft. lb. per oz. per second.*

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* Principles of Animal Mechanics, pp. 482, 484.

ART. XXVI.—*Notes upon the Earthquake of December*, 1874;
by Professor DANIEL S. MARTIN.

ON the night of Thursday, the 10th of December, 1874, there occurred, in and around the city of New York, a slight, but very distinct, earthquake shock, which caused considerable excitement at the time, and furnished material for a brief sensation in the papers of the metropolis. On the succeeding Monday, the New York Lyceum of Natural History appointed a committee to collect information from every attainable source in regard to the shock and all attendant circumstances. Of this committee the writer was chairman, and in that capacity was engaged for the next three months in collecting, arranging and studying the information gained.

A brief circular was prepared, announcing the committee's appointment by the Lyceum, and requesting all persons who had knowledge of the facts to send information in regard to all or any of the following points, viz: 1, the place of observation, and its height above tide water, if known; 2, the time of the shock; 3, the duration of the shock; 4, the nature of the motion felt; 5, the direction of the motion felt; 6, any other facts observed.

This circular was first published in the principal daily papers of the city, with an added request that it be copied by local journals throughout the region affected. One thousand copies were then printed, and these were sent wherever there was any prospect of advantage.

About a hundred responses were received, varying of course greatly in their character: in the main, however, they furnished useful and excellent data, which were afterward carefully tabulated. Though almost all written by unscientific observers, yet the incidents and impressions were related in most cases with a care and clearness beyond what was anticipated, while many of the letters were exceedingly vivid and detailed.

Of course the main objects aimed at were, the determination of the geographical extent of the disturbance, of the rate and the direction of its motion, and if possible, through these, of its approximate depth.

The first end, as might have been expected, was pretty readily attained. The geographical range was determined with tolerable accuracy, and found far wider than at first supposed.

The second inquiry, however, was soon seen to be almost hopeless of result, from the uncertainty in regard to accurate time. Local time and railroad time, errors of ordinary timepieces and errors of observation, complicated also with a difference of longitude corresponding, between the extreme east and west points, to some four minutes of time, presented together a mass of uncertainties that might defy the powers of an abler committee to unravel. It was hoped that astronomical time might be obtained at some points, sufficient to fix a definite basis for averages and calculations, but such was not the case. The most that could be ascertained was that the shock occurred at about 10.25 P. M., throughout the entire district affected. The absence of any indication of a progress in time, together with other facts to be referred to directly, speedily led to the idea that the movement had its source at a very considerable depth, and was by no means local or superficial.

The shock was felt from a little beyond Fishkill Landing, Dutchess County, New York, southward to Sandy Hook lighthouse, a distance of some 80 miles. In an east and west direction, it extended from Morristown, N. J., at least to Stamford, Conn.; some of the newspaper accounts reported it as far east as Norwalk. This would give a total extent in this direction of from 54 to 62 miles. It traversed, in this area, portions of the gneiss of the Blue Ridge, the limestones of Westchester County, the "Atlantic" or "Montalban" gneiss of New York and Connecticut, the Cretaceous and drift of Long Island, and the Trias and trap of New Jersey. Its geological relations are in some respects curious and interesting. The fact is very noticeable, that the movement was felt far more strongly and frequently on rocky than on soft ground,—the latter seeming to produce a cushion-like effect, softening or modifying the shock. This circumstance is quite unusual, and is one of the indications pointing to a deep-seated source. In the San Francisco earthquake of some ten years ago, for instance, buildings situated on made ground were severely damaged, while those on the hills were uninjured. But the reverse condition obtained here, as is proved by many letters, and strikingly shown on the committee's map. The crowded observations in the metamorphic region of New York and Westchester, are in singular contrast to the few and scattered points laid down on the heavy drift of Long Island and the Trias of New Jersey. In the main, the shock seems to have been limited by the Highlands of New York and New Jersey,—the continuation of the Blue Ridge,—but four distinct observations, at as many separate points, near Fishkill Landing, show that in that vicinity it crossed the entire series of these lofty hills. Thence the line of observations follows through and along the Highlands by Garrison's (opposite West Point), Stony Point, and Ramapo, New York, to Morristown, New Jersey. Passing eastward across the thirty-mile belt of Trias and trap, scarcely an observation appears, until near the rear (or inner) edge of the great Palisade range of trap, when numerous reports began to come in, along the line of the Northern Railroad of New Jersey. Once over the Hudson, in the metamorphic region of New York and Westchester Counties, the shock was felt everywhere, especially in buildings founded on rock, as stated by many observers. On Long Island, the northwestern edge is gneiss, from Astoria to Greenpoint, and there the shock was felt; but over the great drift-covered portion to the east, underlaid probably by Cretaceous beds, the observations are few and far between. Four points are reported in the heart of the city of Brooklyn—all within a circle of half a mile across; and this fact would suggest that a ridge of rock perhaps approaches the surface at that point, though none is known to appear.

As regards the direction of the wave-movement distinctly reported by many observers, great difficulty was found in reaching any result. The accounts were most diverse and even contradictory, indicating a great amount of "personal equation" to be allowed for. From various sources, however, it was judged pretty clearly that the course must have been nearly from southwest to northeast, or upward *along the line of strike* of all the rocks of the region. The main sources were, (1) a small majority of the reports; (2) one very minute and careful observation in the upper part of New York City, in which the circumstances, detailed by Mr. J. W. Rhodes, seem not to admit of doubt. In a house fronting south on 113th street, "the sound approached from the south with a *crescendo* movement. As it struck the house the front windows rattled violently. About the middle of the room the sound, or shock, apparently reached its full volume, and then receded with a *diminuendo* movement. The rear windows rattled precisely as those of the front." (3) The direction of cracks in the ground. This effect was reported only at Closter, New Jersey, by Mr. J. L. Turnure, who kindly furnished a plan of the ground. Two narrow cracks appeared, of considerable depth, each on a distinct road, and both having a direction west-northwest and east-southeast, or transverse to the supposed line of movement.

The duration of the shock was very variously estimated by different observers. It is quite possible that it may have varied with the nature of the ground; but in general it would seem to have averaged about ten seconds.

The general phenomena presented nothing peculiar. A loud rumbling sound, a heavy jar, and in some cases a distinct wave-motion, were the chief features. In a few places bells were rung, clocks stopped, and cracks opened in the ground, as above. In only one case was the shock reported as felt on the water,—on a schooner in the harbor of New Rochelle. Very fortunately, the movement just stopped short of doing any serious injury to buildings or property, though it caused much alarm and excitement for a time in various places, both to animals and men.

The night was calm, mild, and somewhat cloudy, with no meteorological phenomena in any respect noteworthy. Several persons who had experienced earthquakes in South and Central America, referred to an intense and peculiar stillness of the atmosphere, just prior to the shock, which they had been wont to notice in a like connection in the tropics. This circumstance is one frequently reported, and it may be worth mentioning here.

It remains to speak briefly of certain other circumstances, which suggest some interesting conclusions.

From several observers, few indeed, but widely separated, and too many to admit of error, accounts were received of one or more later shocks, at about two o'clock the same night.

Two remarkable letters were sent to the committee, detailing a marked disturbance of very similar character in eastern Massachusetts, on the same afternoon between 5.30 and 6. This shock was felt at North Andover and at Salem, and in both cases by several members of a household. Inquiries from Professor Packard of Salem, and Professor Hitchcock of New Hampshire, and notices in the local papers, kindly inserted by the former, failed to elicit any further information on this interesting point.

In "Nature" (Dec. 31st, 1874), a brief account is given of an earthquake shock experienced by three travelers who were passing the night on the Pic du Midi, a lofty summit of the Pyrenees. It occurred at 4.45 on the morning of Dec. 11th; and "Nature" remarks its almost exact coincidence in time with the shock felt in North America.

These several reports, scanty though they are, made a strong impression on the writer's mind, which was referred to in his report to the New York Lyceum,—that the day was one of very wide and very deep-seated disturbance over a considerable part of the Northern hemisphere. At that time the news from Iceland had not arrived; but we have since learned that the tremendous and almost unprecedented series of volcanic outbreaks reported in that island, began about the middle of December last. May it not well be that the tremors felt on December 10th, at various points from the Pyrenees to the Blue Ridge, were connected with a wide and deep disturbance beneath the crust, which found an outlet ere long in the great Icelandic eruptions?

ART. XXVII.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXXVI. — *On some interesting Equine Calculi;* by R. H. CHITTENDEN, Ph.B., Assistant in Physiological Chemistry.

IN the latter part of April, 1875, a mare, the property of Mr. N. A. Baldwin of Milford, Conn., was taken sick apparently with colic. One peculiar feature of the case was that all passage of solid excrement ceased. After a few days the animal died. A post mortem examination was made and in the intestine, about one foot from the stomach, was found a calculus closing up the passage completely. In the stomach was found another calculus of the same appearance, but a third larger. A year previous to this the animal was taken sick in the same manner, and as a result of treatment passed a calculus differing from the others only in size, being somewhat smaller. Through the kindness of Mr. Baldwin, I was able to obtain these calculi for examination. The following is the result: The smallest calculus was perfectly smooth, nearly round and of light-brown color, its nucleus was a small pebble around which the material was arranged in concentric layers, preserving the form of the nucleus. A short distance from the center was a small, loose, irregular layer of organic matter, seemingly pieces of chaff, etc. The remaining portion was hard, compact and divided into a multitude of layers by slight shades of color. The weight of the two halves together was 213·22 grams. The calculus found in the stomach was of a yellowish brown color, its surface was covered with broad veins of a light yellow intermixed with narrow ones of a darker shade; it was nearly round, its circumference one way being 11½ inches, the other 11¼ inches, its weight was 679·6 grams. The calculus found in the intestine and which caused the death of the animal, weighed 441·57 grams; its nucleus was a thin and narrow piece of iron half an inch long. A transverse section revealed the same internal structure as the other, except that in this there was an extra spot of hair-like matter in the compact layer about the nucleus. The surface of the calculus, like that of the others, was perfectly smooth. On fracturing half of this calculus it separated readily into four distinct and regular layers, each of which was made up of smaller ones which could not be separated. On dissolving the substance in cold dilute nitric acid, a pale yellow fluid was obtained and a residue made up of organic matter, with a little silica. Not a trace of uric acid was found in any of the layers. The first or outer layer was nearly ⅛ of an inch thick; its specific gravity was 1·72. The second layer was $\frac{5}{16}$ of an inch thick, with a specific gravity of 1·69. The third layer was ¼ of an inch thick, specific gravity 1·66. The nucleus portion measured one way $1\frac{5}{16}$ inches, the other way $1\frac{7}{8}$ inches; specific gravity 1·71. While the three outer layers were yellowish-brown the nucleus portion was dark-brown, making a distinct contrast in color. The following are the analyses of the different layers;

	1st layer.	2d layer.	3d layer.	Nucleus portion.
P_2O_5	28·10	28·14	28·34	28·14
MgO	16·84	16·87	16·88	16·58
$(NH_4)OH$	12·57	12·59	12·61	12·61
H_2O	41·72	41·80	41·66	41·96
Residue insol. in HNO_3	·74	.58	·58	·60
	99·97	99·98	100·07	99·89

On igniting the substance at a red heat all the water and ammonia was driven off, thus giving the amount of these two substances. Then determining the ammonia directly by means of magnesia and deducting from the total volatile matter the amount of water was thus indirectly obtained.

These analyses show that this calculus is composed principally of ammonio-magnesian phosphate, and that the different layers are essentially the same. By making thin and polished sections of the different layers and examining them under the microscope with a half inch objective, they were found to be amorphous, but divided into layers by what seemed to be fine black lines, and on examination with a fifth of an inch objective these lines were resolved into fine black specks which may be looked upon as impurities in the phosphate, with regular arrangement, and which are insoluble in nitric acid. With polarized light a fine arrangement and display of colors was obtained.

The other two calculi were not at my disposal for analysis, but from their exact resemblance to this in external and internal structure and color, there is no doubt but that their composition is the same.

ART. XXVIII.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXXIV.—*Results of Dredging Expeditions off the New England Coast in* 1874; by A. E. VERRILL.

[Continued from p. 43.]

OUR investigations show conclusively that there is a very decided flow of cold currents through Fisher's Island Sound and Block Island Sound into Long Island Sound, and along the deeper parts of the latter for a great distance, especially toward the southern and deeper side. The influence of this

List of Dredging Localities where Temperatures were taken on the Steamer Bluelight, 1874.

Current No.	Date. 1874.	Hour.	Sky.	Locality.	Bearings.	Nature of bottom.	Depth in fathoms	Temperature. (F.)		
								Air.	Surface.	Bottom.
27	Jul. 22	--------	--------	Fisher's I. Sd.	Middle Clump, ½m. S.E. ----------------	Sand and shells,--	11-9	65°	64°	62°5
28		--------	--------	Fisher's I. Sd.	Middle Clump, ½m. S.S.E. ----------------	Sand and shells,--	11	65	64	62·5
29		--------	--------	Fisher's I. Sd.	West Clump, ¼m. S.S.W. ----------------	Sand and shells,--	8	65	64	63
30	23	2.53 P.M.	--------	Fisher's I. Sd.	Eel Grass Lt. Ship, ½m E. by W. ----------	Sand and gravel, -	7	66	64	62·5
31		3.22	--------	Fisher's I. Sd.	Latimer's Reef, 1m. N.N.E. ----------------	Sand, grav., shells,	10½	65·5	62·5	61·5
32		3.59	--------	Fisher's I. Sd.	Latimer's Reef, W.; Napatree Pt., 2m. E. ---	Sand and rock,---	11	65	62·5	61
33	24	10.34 A.M.	--------	Fisher's I. Sd.	Groton Long Pt., ⅛m. N.E. by N.; N. Hummock Lt., S.W. ----------------	Sand and shells,--	8	72	66	63
34		11.06	¾ Clear, -	Fisher's I. Sd.	Between Groton L. Pt. & Sea-flower Reef, ---	Sand and shells,--	7	71	65·5	62·5
35		12.14 P.M.	--------	Long I. Sd.--	Race Pt. Rock, 1¾m. E. ----------------	Rocks and shells,-	50	72	68	59
36		12.41	Clear,---	Long I. Sd.--	Race Pt. Rock, 2m. E. ----------------	Rocks and gravel,	50	68	68	58
37		2.33	--------	Nepeague B.-	Off Culloden Pt., L. I.----------------	Sand and mud,---	12½	74	66	61
39	27	2.40	Clear,---	Fisher's I. Sd.	E. part of Sweeper Ground,----------------	Sand and shells,--	4	70·5	66·5	65
40		3.10	Clear,---	Fisher's I. Sd.	Hotel on Ram I., ⅛m. N.E., ½ E.----------------	Sand,----------	4	68·5	66·5	65·5
41		3.31	Clear,---	Fisher's I. Sd.	Ram I., ½m. N.E.----------------	Sand,----------	3½	68·5	66·5	65
42		3.58	Clear,---	Fisher's I. Sd.	Off Middle Clump,----------------	Gravel and stones,	14	68·5	66·5	64
43		4.21	Clear,---	Fisher's I. Sd.	Off Middle Clump,----------------	Gravel and stones,	10½	67	66	64·5
44	29	11.00 A.M.	Cloudy, -	Fisher's I. Sd.	Eel Grass Light Ship, ½m. S.E ----------------	Sand, grav., shells,	7	----	----	----
45	30	11.10	Clear,---	Block I. Sd.--	Little Gull I. Lt., W.; Race Rock, N.W. ----	Sand,----------	45	76	62·5	57
48		1.15 P.M.	Clear,---	Block I. Sd.--	Gardiner's Point Light, ¾m. S. ----------------	Gravel, --------	14½	71	66	63·5
49		2.50	Clear,---	Gardiner's B.	In Gardiner's Bay, L. I.,----------------	Mud, ----------	6½	71·5	67·5	64·5
50		3.46	Clear,---	Gardiner's B.	In Gardiner's Bay, L. I.,----------------	Sand,----------	4½	72·5	66·5	65
51		4.10	Clear,---	Gardiner's B.	In Gardiner's Bay, L. I.,----------------	Gravel, --------	3	72	66·5	65
52		4.27	Clear,---	Gardiner's B.	In Gardiner's Bay, L. I.,----------------	Mud, ----------	6½	69·5	68·5	65
53	31	12.54	Clear,---	Block I. Sd.--	Watch Hill Light, 3m. N. by W. ----------	Sand,----------	18	68	66	56
55	Aug. 3	10.30 A.M.	Clear,---	Long I. Sd.--	Bartlett's Reef Light Boat, 1½m. E. ½ S. -----	Sand and mud,---	22	60·5	64·5	63·5
56		10.56	Clear,---	Long I. Sd --	Bartlett's Reef Light Boat, 2½m. E. ½ S. -----	Gravel and sand,-	14	59	64	63
57		11.33	Clear,---	Long I. Sd.--	Bartlett's Reef Light Boat, 3m E.----------	Sand, grav., shells,	15½	67	64·5	63·5
58		12.15 P.M.	Clear,---	Long I. Sd.--	Hatchet's Point, 2m. N.W.----------------	Gravel and shells,	19	61·5	64	63
59		1.00	Clear,---	Long I. Sd.--	Off Saybrook, Conn. ----------------	Sand,----------	4	67	64·5	63·5

Current No.	Date. 1874.	Hour.	Sky.	Locality.	Bearings.	Nature of bottom.	Depth in fathoms	Temperature. (F.)		
								Air.	Sur-face.	Bot-tom.
61	Aug. 4	12.19 P.M.	Clear,	L. Peconic B.	Little Peconic Bay, L. I.	Gravel and shells,	7½	66°5	74°	71°5
63		1.16	Clear,	L. Peconic B.	Little Peconic Bay, L. I.	Gravel,	7	----	----	72
64		2.02	Clear,	L. Peconic B.	Little Peconic Bay, L. I.	Sand and gravel,	10–13	67	72	71·5
66		3.29	Clear,	Gt. Peconic B.	In Great Peconic Bay, L. I.	Mud and gravel,	5½	67·5	74	72
67		3.50	Clear,	Gt. Peconic B.	In Great Peconic Bay, L. I.	Sand,	5½	68	73	72·5
68		4.09	Clear,	Gt. Peconic B.	In Great Peconic Bay, L. I.	Gravel,	4½	66·5	73	72·5
69		4.56	Clear,	L. Peconic B.	Little Peconic Bay, L. I.	Sand and shells,	9½	66	72·5	71
71	5	1.45	¾ Clouds,	Gardiner's B.	Gardiner's Bay, L. I.	Sand,	3½	70·5	----	68
73	6	9.35 A.M.	Clear,	Block I. Sd.	Watch Hill Lt. 3m N. ½ W.	Sand,	18	63	63	60
74		11.10	Clear,	Block I. Sd.	Montauk Pt., 6m. S.W. ½ S.	Sand,	17	66	63·2	60
75		12.06 P.M.	Clear,	Block I. Sd.	Block I. Lt., 3m. E.N.E.	Mud and stones,	19½	67	63·5	60
76		12.48	Clear,	Block I. Sd.	Block I. Lt., 4m. S.E. by E.	Sand and mud,	18½	68	64	60
77		1.22	Cloudy,	Block I. Sd.	Block I. Lt., 7m. E.S.E.	Mud,	19	68	64	59
78		2.50	Clear,	Block I. Sd.	Watch Hill, 4m. N.W. ½ N.	Sand,	23	70	64	58·5
79		--------	--------	Block I. Sd.	Watch Hill, 3m. N.W. ½ N.	Sand and shells,	22	----	----	----
80		3.20	Clear,	Fisher's I. Sd.	West Harbor, Fisher's I.	Sand and shells,	4	74	66·5	65·2
81	10	3.20	Clear,	Fisher's I. Sd.	West Harbor, Fisher's I.	Sand and shells,	3½	74	66·5	65·3
82		--------	Clear,	Fisher's I. Sd.	West Harbor, Fisher's I.	Sand and mud,	5½	74	66·5	----
83		--------	Clear,	Fisher's I. Sd.	West Harbor, Fisher's I.	Sand and gravel,	5½–2	74	----	----
84		5.13	Clear,	Fisher's I. Sd.	Off Middle Clump,	Mud and shells,	12½	73·5	69	64·7
85	11	--------	Clear,	Block I. Sd.	Fisher's I., N.	Sand,	15	75	66	61
86		10.35	Foggy,	Block I. Sd.	Fisher's I., N.	Sand,	8½	75	65·5	62·5
87		11.25	Clear,	Block I. Sd.	Fisher's I., N.	Stones,	8	72	65·5	63
88		11.52	Clear,	Block I. Sd.	Fisher's I., N.E.	Stones,	7½	76	66·5	63
89		12.24	Clear,	Block I. Sd.	Fisher's I., N.E.	Stones and gravel,	6	78	66·5	63·2
90		1.17	Clear,	Block I. Sd.	Fisher's I., N. W. by W.; Race Point, W.	Stones and gravel,	5½	76·5	66·5	63·2
91		2.40	Low fog,	Block I. Sd.	Fisher's I., N.	Sand and shells,	32½	75	66·5	58·5
93	12	2.57	Clear,	Long I. Sd.	Seaflower Reef, S.S.W.	Sand,	4¾	76	67·5	62·5
94		3.13	Clear,	Long I. Sd.	Seaflower Reef, S.S.W.	Sand and gravel,	4½	75	67	64·5
95		3.46	Clear,	Long I. Sd.	Seaflower Reef, E.	Fine sand and mud	6	----	67	64·5

Current No.	Date. 1874.	Hour.	Sky.	Locality.	Bearings.	Nature of bottom.	Depth in fathoms	Temperature. (F.)		
								Air.	Surface.	Bottom.
96	Au. 12	4.14 P.M.	Clear,	Long I. Sd.	Seaflower Reef, 1 m. E.	Mud and sand,	6	72°	67°	64°5
97	13	10.40 A.M.	Cloudy,	Block I. Sd.	Montauk Pt., 7m. S.S.E.; Middle Ground, 1½m. W. by N.	Sandy,	15½	74	65	64
98		11.20	Cloudy,	Block I. Sd.	Montauk Pt., 7½m. S.S.E.	Sand and gravel,	9	71	65	64
99		11.48	Cloudy,	Block I. Sd.	Montauk Pt., 7¾m. S.S.E.	Sand and rocks,	5½	72	65	64
100		12.45 P.M.	Cloudy,	Block I. Sd.	Montauk Pt., 4½m. S. by E.	Fine sand,	19	72	65	63·5
101		1.10	Cloudy,	Block I. Sd.	Montauk Pt., 3m. S. by W.	Sand and shells,	20	72	66	63·5
102		2.15	Cloudy,	Block I. Sd.	Montauk Pt., 2½m. S.S.W.	Stones,	8	72·5	65	65
103		2.45	Cloudy,	Block I. Sd.	Montauk Pt., 2m. W.S.W.	Rocks,	7½	72	65	64·5
104		3 10	Cloudy,	Block I. Sd.	Montauk Pt., 2m. W.	Rocks,	7½	72	65	64·5
105	14	2.43	Clo.&rain	Fisher's I. Sd.	Between Eelgrass Lt. Boat & White Rock,	Sand and gravel,	5¼	67	66	64·5
106		....	Clo.&rain	Fisher's I. Sd.	Bet. Eelgrass Lt. B. & White R., 1m. E. by N.	Sand and gravel,	6	67	----	----
107		....	Clo.&rain	Fisher's I. Sd.	Stonington Lt., 1m. N.E. ¼ E.	Sand and gravel,	5	67	----	----
108		....	Clo.&rain	Fisher's I. Sd.	Eelgrass Lt. B., ¾m. W.N.W.	Rocks,	5½	67	----	----
109	17	3.30	Clear,	Fisher's I. Sd.	Eelgrass Lt. B., ¾m. N.W. by W.	Stones,	7	69·5	67	63
110		3.43	Clear,	Fisher's I. Sd.	Eelgrass Lt. B., 1m. W.N.W.	Sand,	6½–3	69·5	67	63
111		3.49	Clear,	Fisher's I. Sd.	Eelgrass Lt. B., 1¼m. W. by N.	Rocks,	5¼	69	----	----
112		4.25	Clear,	Fisher's I. Sd.	Stonington Lt., 1¼m. E.N.E.	Sand,	4	69	66·5	----
113		5.10	Clear,	Fisher's I. Sd.	Eelgrass Lt. B., 1m. W. ¼ N.	Stones,	7	70	67	63
114		5.40	Clear,	Fisher's I. Sd.	Eelgrass Lt. B., 1m. E.	Sand,	7½	70	66·5	63
115	18	12.10	Clear,	Block I. Sd.	Mont. Pt. 9m. W.; S.E. end Block I., 6½m. N.E.	Sand,	20	71	66	47·5
116		2.43	Clear,	Off Block I.	Mont. Pt., 11m. N.W. by W.; S.E. end Block I., 9m. N.E. by N. ¼ N.	Sand and shells,	25	70	67·5	45·5
117		....	Clear,	Off Block I.	Same as last,	Sand and shells,	23¼	70	----	----
118		....	Clear,	Off Block I.	Same as last,	....	23¼	70	----	----
119		3.37	Clear,	Off Block I.	Old Harbor Point, 5m. N.	Sand and stones,	11	----	67	55
120		....	Clear,	Off Block I.	Old Harbor Point, 5m. N.	Sand and stones,	11	70	----	
121		4.38	Clear,	Off Block I.	Off New Shoreham,	Gravel and stones,	14	70	66	57·5
122		5.00	Clear,	Off Block I.	Off New Shoreham,	Sand and gravel,	18	70	66·5	52·5
123	19	9.00 A.M.	Clear,	Off Block I.	New Shoreham, 6m. N.W. by N.	Sand and gravel,	14	73	66·5	54

Current No.	Date. 1874.	Hour.	Sky.	Locality.	Bearings.	Nature of bottom.	Depth in fathoms	Temperature. (F.) Air.	Sur-face.	Bot-tom.
124	Au. 19	9.37 A.M.	Clear,	Off Block I.	New Shoreham, 6m. N.N.W.	Coarse sand,	14½	73°	66°	50°5
125		10.37	Clear,	Off Block I.	Block I., N.W.	Gravel,	14½	69·5	66·5	53
126		12.34 P.M.	Hazy,	Off Rhode I.	Point Judith, 4m. N.W.	Sand and gravel,	13½	75	67·5	54·5
127		1.10	Hazy,	Off Rhode I.	Point Judith Lt., 2½m. N.	Stones,	9	----	69·5	61
128		1.43	Hazy,	Off Rhode I.	Point Judith Lt., 3m. E.	Rocks and sand,	4	76	67·5	63
129		--------	Hazy,	Off Rhode I.	Off Narragansett Beach,	Sand and gravel,	8½	----	----	----
130		--------	Hazy,	Off Rhode I.	Off Narragansett Beach,	Gravel and stones,	10½	----	----	----
131	21	11.00 A.M.	Hazy,	Block I. Sd.	Watch Hill Lt., 3m N. ½ E.	Sand and gravel,	21	80	67·2	56·5
132		--------	Hazy,	Block I. Sd.	Watch Hill Lt., 3½m. N. by E. ½ E.	Sand,	20	80	67·2	----
133		1.25 P.M.	Hazy,	Block I. Sd.	Watch Hill Lt., 4m. N.E. by N.	Sand,	17½	79·2	67·2	----
134		1.25	Hazy,	Block I. Sd.	Off E. end Fisher's I.	Gravel,	9	78	66·5	63·5
135		2.00	Hazy,	Block I. Sd.	E. end Fisher's I., 2m. N.	Sand,	19½	78	67	57·5
136	24	11.45 A.M.	Cloudy,	Napeague B.	Fort Pond Bay,	Mud,	7½	76	73·2	65·5
142	25	3.00 P.M.	½ cloudy,	Long I. Sd.	Off Hay Harbor,	Sand,	4½	70	65·5	64·5
143		3.30	--------	Long I. Sd.	Race Point, ½m. S.	Mud and sand,	7½	70	65·5	----
144		3.40	--------	Long I. Sd.	Race Point, 1m. S.S.E.	Find sand,	8½	----	----	----
145		4.20	--------	Long I. Sd.	Off Race Point,	Rocks,	5½	----	----	----
146		5.30	--------	Fisher's I. Sd.	Between E. Clump & Ram I. Reef,	Hard,	7½	74·5	65·5	65
147		--------	--------	Fisher's I Sd.	Between E. Clump & Ram I. Reef,	Hard,	14	----	----	----
148		6.00 P.M.	--------	Fisher's I. Sd.	Ram I. hotel, W.N.W.	Hard,	7½	----	----	----
149	27	11.17 A.M.	Part clo.	Long I. Sd.	Off Niantic, Two-Tree I., E.	Sandy,	5	70·5	65	64
154		3.50 P.M.	Part clo.	Long I. Sd.	Plum I. Lt., 3m. S.E. by E.; Saybrook Lt., N.W. by W.	Gravel,	22	73·5	66	65
155		4.10	Clear,	Long I. Sd.	Same as last,	Gravel,	26	73·5	66	65
157	30	9.14 A.M.	Clear,	Off Block I.	Near Coxe's Ledge, Block I., 20m. W.N.W.	Sand, stones, rock,	21	67	62	51·5
158		--------	Clear,	Off Block I.	Near Coxe's Ledge,	Rocky,	21	----	----	----
159		9.00 A.M.	Clear,	Off Block I.	Near Coxe's Ledge, Block I., 20m. W. by N.	Rocks and sand,	21	----	----	----
161		1.40 P.M.	Clear,	Off Block I.	Old Harbor Point, 10m. N.W. by W., ½ W.	Mud,	34	70	64	52
162		2.00	Clear,	Off Block I.	10m. off Block I., near last,	Mud,	34	70	64	52
172	Sept. 2	2.26	Clear,	Off Block I.	Coxe's Ledge,	Hard,	21	72·5	62.5	50

cold current is very apparent as far west as New Haven, in the deeper parts of the sound.* This cold water is doubtless derived directly or indirectly from the arctic current that flows southward along our Atlantic coast; but its flowing into Long Island Sound may be due largely to the influence of the tidal currents, modified by the local wind currents. On the other hand, the much higher temperatures of such enclosed localities as the Peconic Bays may be safely attributed to the direct heat of the sun over a broad expanse of shallow water, from which the cold currents are excluded.

Improved methods of preserving specimens.—During the summer numerous experiments were made by members of the party, but more especially by Prof. W. N. Rice and the writer, to ascertain the effects of various chemical preparations upon marine invertebrates. The special objects were: 1st, to improve the methods of preserving specimens for museum purposes, or to devise new methods; 2d, to ascertain the best means of killing in an expanded state species that ordinarily contract badly when put directly into alcohol. Besides numerous negative results, several of value were obtained. Numerous very perfect and beautiful preparations of *Actiniæ* (chiefly *Metridium marginatum*), in a state of nearly complete expansion, were made by slowly adding a saturated solution of picric acid to a small quantity of sea-water in which they had been allowed to expand. When fairly dead they were transferred to a pure saturated solution of the acid and allowed to remain from one to three hours, according to size, &c. They were then placed in alcohol of about 60 to 70 per cent for permanent preservation. The alcohol should be renewed after a day or two, and this should be repeated until the water is all absorbed from the specimen. Hydroids (*Tubulariæ* thus preserved are especially beautiful) and most kinds of jelly-fishes can be easily and beautifully preserved in the same way, but with these the specimens may usually be placed alive directly into the acid, of full strength. Even delicate Ctenophoræ (*Mnemiopsis*, *Idyia*, &c.) can be thus preserved so as to make fair specimens. With osmic acid we did not succeed so well, for the specimens contracted more, and finally became so darkly stained as to render them useless.

Hydro-chloral was also experimented with. It proved to be useless as a permanent preservative of marine invertebrates, as it apparently had a caustic or solvent action, and all the soft parts gradually dissolved, but without putrefaction. It was, however, found very useful for killing certain kinds of animals in an extended condition. It succeeded well, in this respect, with many nemerteans and some annelids, but did not affect all the species alike. Our success with it, in this way, was so great as to encourage us to make many additional trials of this kind during the coming summer. Many experiments were made last season, as in previous years, to find some poison that will kill mollusks, especially Gastropods, in a fully extended state. Although numerous drugs have thus been tried, the results have hitherto been mostly negative. At least, no method has been discovered that is more generally successful than by allowing them to suffocate in stale sea-water, through excess of carbonic acid and deficiency of oxygen. Many excellent preparations of the larger species (*Fulgur*, *Buccinum*, *Natica*, &c.) were thus made last summer. In most cases when the animal was found to be well extended, and at the same time so stupified as to be nearly inactive, the soft parts were forcibly held out by the hand while it was killed by immersion in alcohol Sometimes it could be tied to the body of the shell so as to keep it from withdrawing when it was placed in alcohol. The accompanying figure was made from a specimen prepared in this way.*

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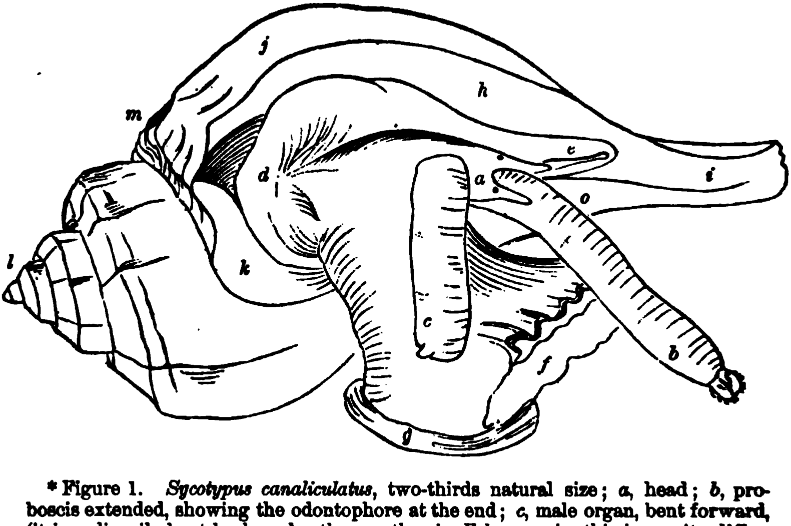
* The following temperatures were taken May 17th, a short distance off the outermost of the Thimble Islands, a few miles east of New Haven: surface 45½°, at 1 P. M., wind southerly, tide 4 hours ebb, sky clear; bottom 43½°, in 5 fathoms, rocks and mud; surface 48°, at 5.30 P. M., tide two hours flood.



* Figure 1. *Sycotypus canaliculatus*, two-thirds natural size; *a*, head; *b*, proboscis extended, showing the odontophore at the end; *c*, male organ, bent forward, (it is ordinarily bent back under the mantle; in *Fulgur carica* this is a quite different, thin, flat, tapering, tongue-shaped organ); *d*, mantle; *e*, siphon; *f*, lower side of foot; *g*, operculum; *h*, aperture of shell; *i*, canal; *j*, body whorl; *k*, inner lip; *l*, *m*, spire; *o*, columella.

ART. XXIX.—*On the Passage of two Bolides in* 1872 *and* 1874, *over Middle Kentucky;* by J. LAWRENCE SMITH, of Louisville, Kentucky.

THERE have not been, since 1871, any careful observations of the passage of solid meteoric bodies, except in the case of two which came under my immediate observation, both passing over the city in which I reside. The first was on the 12th of December, 1872, just after sunset, and the other on the evening of July 18th, 1874, about 9 o'clock P. M. Of the former I saw only the last remnant of its effects in the atmosphere; the latter I observed in part of its passage and saw it explode.

That of December, 1872, was certainly one of considerable importance, and I have succeeded in collecting not less than fifteen good observations, scattered over a space of one hundred miles east and west, and eighty miles north and south.

At Louisville (lat. 38° 20′ N., long. 85° 25′ W.), just after sundown on the 12th of December, a large red light suddenly appeared in the zenith, and for several seconds seemed to stand motionless, it evidently descending directly in a line with the eye of the observer. Then starting off with an uncertain, faltering motion, it moved slowly toward the horizon, in a southerly direction, gradually fading, in its flight, from a lurid red to a dark purplish hue, and leaving a dense stream of blue smoke behind, which remained for several minutes after the disappearance of the meteorite.

Not many miles from Louisville it is described as an electric flash in a clear sky (with the moon shining and the brighter stars visible), followed in two minutes, by a distant rolling noise like thunder, the reverberation of which lasted for over a minute; and near the zenith, two indistinct clouds were seen, resembling the smoke caused by the explosion of gunpowder.

Another observer, forty miles to the east of Louisville, followed its passage for about twenty degrees. It appeared to him to arise in the west, about ten degrees above the horizon, in the form of a ball of fire, one-fourth the size of the moon, followed by a trail of light which was visible for several seconds and gradually gave place to a well defined line of bluish vapor, which could be plainly seen for three or four minutes. Its passage was accompanied by a distinct noise.

At a place about eighty miles east of Louisville it was seen in a direction almost due west, about 30° above the horizon, resembling a large sky-rocket throwing off sparks, and moving rapidly southward inclined to the horizon; it disappeared about 20° above the horizon, leaving a bright track of smoke, which was at first very luminous, from its being yet in the light of the sun, that had just before sunk beneath the horizon; but its brilliancy faded as the sun descended lower, and then it behaved like a film of smoke, that was wafted into zigzags by a gentle breeze, curling up in folds and disappearing in about fifteen minutes, going toward the north. After the lapse of four and a half or five minutes, three or four loud detonations were heard in the direction in which the meteor disappeared—the sounds following each other in quick succession, and resembling very closely a rolling of artillery fired very rapidly. From the calculation of this observer, the explosion was located as far west as Louisville, and some thirty or forty miles to the south of this city.

All describe the cloud as remaining for several minutes, slowly breaking up and gradually fading away.

An observer at Danville, seventy miles southeast of Louisville, speaks of its motion as being very much slower than that of the ordinary shooting stars. It left a line of light which lasted but a short time, which had a beaded structure before it disappeared, after which the cloud already mentioned became visible. This observer did not hear any noise; to him it appeared as a viscid body leaving a portion of its mass adhering to the atmosphere as it passed through it, not unlike what we see when a rod is dipped into molten glass, and then withdrawn, leaving a thread attached to the original mass. The thread, however, left by the meteor, did not appear of a uniform size but in places was swollen into knots, and when it was consumed (the thinnest parts disappearing first) it left heavier masses of cloud or vapor at the points where it was thickest. Of these, two were particularly noticeable, as lasting for some minutes after the others had disappeared. They were two or three degrees apart and continued plainly visible for several minutes; and one was seen by this observer for at least ten minutes, and even then disappeared only from the failing light of the evening. The disappearance of the meteor was sudden, not gradual, and the heaviest part of the cloud was not at the point where it disappeared, and the light did not sensibly diminish up to the moment of its extinction.

An observer at Elizabethtown, forty miles south from Louisville, speaks of seeing the explosion, and hearing the report three and a half minutes afterward; by calculation, he located the explosion eight miles south or southwest of Louisville.

Although most observers locate its fall not very far from Louisville, still in this city no one heard any noise such as is produced by the explosion of these bodies. No discovery of any fragments of this meteorite have been made.

I have no comments to make in reference to the passage of this body through the atmosphere, except in connection with the blue or purplish cloud seen by all observers and lasting for no inconsiderable length of time. These clouds are not unfrequently connected with the passage of these bodies through our atmosphere, and are usually more striking in the day time, or, as in this instance, just after sunset, when the sun was well situated to light up the cloud and exhibit it to the observer who could no longer see the sun. What are these clouds? are they composed of impalpable matter abraded from the surface of these bodies in their passage, or are they true vapor clouds? From a close study of observations in connection with several well known falls of meteorites, I am more inclined to adopt the former view; but there is reason for believing that the violent disturbance of a portion of the atmosphere (much of it, in the rapid passage of the body, undergoing great condensation), added to an undoubted electric disturbance of the atmosphere, would tend to the deposition of moisture, upon the atmosphere being gradually restored to its former equilibrium. These, however, are but speculations advanced to draw to the subject the attention of other observers.

The bolide of July 8th, 1874, does not possess equal interest with that just described. Its central point of observation was the same, viz: Louisville. My own observation was made during the last twenty degrees of its course. It was seen by me at 10 o'clock P. M., as a brilliant pear-shaped body, one-third the diameter of the disk of the moon, with a stream of light in its rear, passing in a rapidly descending curve toward the south. When it arrived within almost twenty degrees of the horizon, it burst into three or four separate parts, flashing fourth red and blue lights and instantly disappeared, after the separation, in a southerly direction, the course of its passage being from N.N.W. to S.S.E. I did not hear any explosion at the time of its bursting.

At Franklin, 150 miles southwest of Louisville, it was observed to have a course from north to southwest, and described as being not less than a man's head in size, with a light bluish color, emitting sparks in its course, but no noise was heard until about three minutes after its explosion, when there was a noise like distant thunder. No fragments resulting from the explosion were ever found.

ART. XXX.—*Note on the Gases accompanying Meteorites;* by Prof. J. W. MALLET, University of Virginia.

IN the paper of Prof. A. W. Wright, in the American Journal of Science for July, on the gases obtained from the mixed iron and stony meteorite of Feb. 12, 1875, he remarks that his results "*warrant the following conclusions:* 1st. *The stony meteorites are distinguished from the iron ones by having the oxides of carbon, chiefly the dioxide, as their characteristic gases, instead of hydrogen.*"

The only specimens of meteoric iron from which, so far as I am aware, the occluded gases have previously been obtained and analyzed, are the Lenarto iron, originally examined by Prof. Graham, and that from Augusta County, Virginia, described by myself.

In the former, hydrogen was the predominant gas, amounting to 85·68 p. c. of the gaseous educt, with but 4·46 p. c. of carbon monoxide and no carbon dioxide; but from the latter I obtained the proportion:

Hydrogen	35·83	
Carbon monoxide	38·33	=48·08
Carbon dioxide	9·75	

So that the oxides of carbon stand to the hydrogen in round numbers in the the ratio 4 : 3,[*] and in the paper on the subject read before the Royal Society on May 30, 1872, I drew attention to the fact that this result did not agree with Graham's supposition as to hydrogen being *the* characteristic gaseous ingredient of meteoric iron.[†]

As to the relative amounts of the two oxides of carbon respectively, I remarked in the same paper:

"*Although it might be assumed, especially in view of the strong tendency of iron to take up and "occlude" carbonic oxide, that this gas had been the original form in which the gaseous car-*

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[*] In the preliminary trial made by Prof. Wright (this Journal, June, 1875, p. 459) the oxides of carbon were found to amount to 49 p. c. of the total gas, agreeing almost exactly with the above; in his account of the more complete investigation (this Journal, July, 1875, pp. 45 and 46) he does not clearly state the absolute volumes of mixed gases obtained at different temperatures, so that it is impossible to calculate the average percentage of carbon compounds for the whole. The arithmetic mean taken from his five analyses is 55·77, but as the larger part of the gas was obviously obtained in the later stages of the heating, and the last three analyses give far less of the oxides of carbon than the first two, this number is doubtless above the true mean.

[†] I may mention that at the British Association meeting at Brighton, in 1872, Mr. W. Chandler Roberts of the English Mint, formerly Prof. Graham's assistant, orally informed me that the examination of other specimens of meteoric iron, subsequently to the publication of Prof. Graham's well known paper, had yielded results similar to mine.

bon compounds obtained existed in the iron, and that it had in part broken up at the temperature of the experiment into carbon (remaining united with the iron) and carbonic anhydride (which escaped as gas), yet, in view of the steady decrease in the quantity of this latter gas collected as the experiment proceeded and the temperature became higher, and bearing in mind the ready decomposition it undergoes in contact with ignited iron, it seems more likely that a larger amount of carbon originally existed in the iron in this higher state of oxidation than appears from the figures of the analysis."

Prof. Wright's specimen of mixed iron and stony matter was in powder; my specimen was a single solid piece of iron. His was more gently heated at first than mine, and even at the end was not raised to as high a temperature. Both these circumstances would of course facilitate the escape of the carbon dioxide and diminish the chance of its undergoing partial reduction by prolonged contact with strongly heated iron.

On the whole I confess that I cannot look upon the above quoted conclusion reachéd by Prof. Wright as sustained by the scanty evidence as yet before us.

ART. XXXI.—*Contributions from the Physical Laboratory of the University of Pennsylvania.* No. I.—*A New Vertical-lantern Galvanometer;* by GEORGE F. BARKER, M.D., Professor of Physics.

[Read before the American Philosophical Society, May 7, 1875.]

DESIRING to show to a large audience some delicate experiments in magneto-electric induction, in a recent lecture upon the Gramme machine, a new form of demonstration galvanometer was devised for the purpose, which has answered the object so well that it seems desirable to make some permanent record of its construction.

Various plans have already been proposed for making visible to an audience the oscillations of a galvanometer needle; but they all seem to have certain inherent objections which have prevented them from coming into general use. Perhaps the most common of these devices is that first used by Gauss in 1827, and adopted subsequently by Poggendorff and by Weber, which consists in attaching a mirror to the needle. By this means, a beam of light may be reflected to the zero point of a distant scale, and any deflection of the needle made clearly evident. The advantages of this method are: 1st, the motion of the needle may be indefinitely magnified by increasing the distance of the scale, and this without impairing the delicacy of the instrument; and 2d, the angular deflection of the needle is doubled by the reflection. These unquestioned advantages have led to the adoption of this method of reading in the most excellent galvanometers of Sir William Thomson. While therefore, for purposes of research, this method seems to leave very little to be desired, yet for purposes of lecture demonstration it has never come into very great favor; perhaps because the adjustments are somewhat tedious to make, and because, when made, the motion to the right or left of a spot of light upon a screen fails of its full significance to an average audience.

Another plan is that used by Mr. Tyndall in the lectures which he gave in this country. In principle, it is identical with that employed in the megascope; i. e., a graduated circle over which the needle moves, is strongly illuminated with the electric light, and then by means of a lens a magnified image of both circle and needle is formed on the screen. The insufficient illumination given in this way, and the somewhat awkward arrangement of the apparatus required, have prevented its general adoption. A much more satisfactory arrangement was described by Professor Mayer in 1872,* in which he appears to have made use, for the first time, of the excellent so-called vertical lantern in galvanometry. Upon the horizontal plane face of the condensing lens of this vertical lantern, Mayer places a delicately balanced magnetic needle, and on each side of the lens, separated by a distance equal to its diameter, is a flat spiral of square copper wire, the axis of these spirals passing through the point of suspension of the needle. A graduated circle is drawn or photographed on the glass beneath the needle, and the image of this, together with that of the needle itself, is projected on the screen, enlarged to any desirable extent. The defect of this apparatus, so excellent in many respects, seems to have been its want of delicacy; for in the same paper the use of a flat narrow coil wound lengthwise about the needle, is recommended as better for thermal currents. Moreover, a year later, in 1873,† Mayer described another galvanometer improvement, entirely different in its character. In this latter instrument, the ordinary astatic galvanometer of Melloni was made use of, an inverted scale being drawn on the inside of the shade, in front of which traversed an index in the form of a small acute rhomb, attached to a balanced arm transverse to the axis of suspension of the needle, and moving with it. The scale and index were placed in front of the condensing lenses of an ordinary lantern, and their images were projected on the screen in the usual way by use of the objective. This instrument is essentially the same in principle as the mirror-galvanometer; but it cannot be as sensitive as the latter, while it is open to the same objection which we have brought against this—the objection of unintelligibility. In the hands of so skillful an experimenter as Mayer, it seems, however, to have worked admirably.

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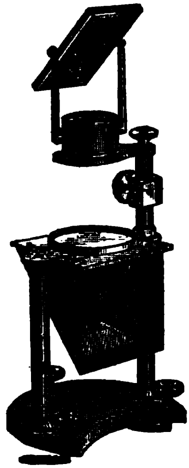
* This Journal, III, iii, 414, June, 1872; Jour. Frank. Inst., III, lxiii, 421, June, 1872.

† This Journal, III, v, 270, April, 1873.

It was a tacit conviction, that none of the forms of apparatus now described would satisfactorily answer all the requirements of the lecture above referred to, that led to the devising of the galvanometer now to be described, which was constructed in February of the present year. Like the first galvanometer of Mayer, the vertical lantern as improved by Morton,* forms the basis of the apparatus. This vertical lantern as constructed by George Wale & Co., at the Stevens Institute of Technology, as an attachment to the ordinary lantern, is shown in the annexed cut, fig. 1. Parallel rays of light, from the lantern in front of which it is placed, are received upon the mirror, which is inclined 45° to the horizon, and are thrown directly upward, upon the horizontal plano-convex lens just above. These rays, converged by the lens, enter the object glass, and are thrown on the screen by the smaller inclined mirror placed above it. The upper face of the lens forms thus a horizontal table, upon which water-tanks, etc., may be placed and many beautiful experiments shown. To adapt this vertical lantern to the purposes of a galvanometer, a graduated circle, photographed on glass, is placed upon the horizontal condensing lens. Above this, a magnetic needle, of the shape of a very acute rhomb, is suspended by a filament of silk, which passes up through a loop formed in a wire stretched close beneath the object glass, and thence down to the side pillar which supports this objective, where it is fastened by a bit of wax, to facilitate adjustment. The needle itself is fixed to an aluminum wire, which passes down through openings drilled in the scale glass, the horizontal lens, and the inclined mirror, and which carries a second needle near

1.



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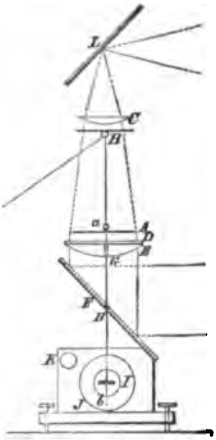
* This Journal, III, ii, 71, 153, July, Aug. 1871; Jour. Frank. Inst., III, lxi, 300, May, 1871; Quar. J. Sci., Oct. 1871. In Duboscq's vertical attachment, which was advertised in his catalogue in 1870, the arrangement is similar, except that the beam received upon the mirror is a diverging one, and consequently the horizontal lens is of shorter focus. A total reflection prism, placed above the object glass, throws the light to the screen. The instrument gives a uniformly illuminated but not very bright field.

its lower end.* Surrounding this lower needle is a circular coil of wire, having a cylindrical hollow core an inch in diameter, in which the needle swings, and a smaller opening transverse to this, through which the suspension wire passes. In the apparatus already constructed (in which the upper needle is five centimeters long) the coil is composed of 100 feet of No. 14 copper wire, and has a resistance of 0·235 ohm. The accompanying cross section (fig. 2) of the vertical-lantern galvanometer as at present arranged, drawn on a scale of $\frac{1}{12}$, will serve to make the above description more clear. A is the needle, suspended directly above the scale-glass D, by a silk filament passing through the loop B, close under the objective C. This needle is attached to the aluminum wire *ab*, which passes directly through the scale-glass D, the condensing E, and the inclined mirror F, at H, and carries, near its lower end, the second needle I. This needle is shorter (its length is 2·2 cm.) and heavier than the upper one, and moves in the core of the circular coil J, whose ends connect with the screw-cups at K. This coil rests on the base of the lantern, enclosed in a suitable frame. It is obvious that when the instrument is so placed that the coil is in the plane of the meridian, any current passing through this coil will act on the lower needle, and, since both needles are attached to the same wire, both will be simultaneously and equally deflected. Upon the screen is seen only the graduated circle and the upper needle: all the other parts of the apparatus are either out of the field or out of focus. Moreover, the hole in the lens is covered by the middle portion of the needle, and hence is not visible. The size of the image is of course determined by the distance of the galvanometer from the screen; in class experiments, a circle eight feet in diameter is sufficient; though in the lecture above referred to, the circle was sixteen feet across, and the needle was fourteen feet long, the field being brilliant.

2.



The method of construction which has now been described, is evidently capable of producing a galvanometer for demonstration, whose delicacy may be determined at will, depending only on the kind of work to be done with it. In the first place, the needles may be made more or less perfectly astatic and so freed more or less completely from the action of the earth's magnetism, and consequently more or less sensitive. Moreover, an astatic system seems to be preferable to one in which damping magnets are used, since it is freer from influence by local causes; though, if desirable for a coarser class of experiments, the considerable distance which separates the needles in this instrument, allows the use of a damping magnet with either of them. In the galvanometer now in use, the upper needle is the stronger, and gives sufficient directive tendency to the system, to bring the deflected needle back to zero quite promptly. In the experiments referred to below, the system made 25 oscillations per minute.

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* After the new galvanometer was completed and had been in use for several weeks, I observed, in re-reading Mayer's first paper, a note stating that the idea had occurred to him of using an astatic combination consisting of two needles, one above the lens and the other below the inclined mirror, the two being connected by a stiff wire passing through holes in the condenser and the mirror. The plan of placing the coil round the lower needle does not seem to have suggested itself to him. Indeed, it does not appear that the arrangement he mentions was ever carried into practical effect.

Secondly, the space beneath the mirror is sufficiently large to permit the use of a coil of any needed size. Since, therefore, the lower needle is entirely enclosed within the coil, the field of force within which it moves, may be made sensibly equal at all angles of deflection, as in the galvanometers of Sir William Thomson. Hence the indication of the instrument may be made quantitative, at least within certain limits. The circular coil too, has decided advantages over the flat coil, since the mass of wire being nearer to the needle, produces a more intense field. Were it desirable, a double coil, containing an astatic combination, could be placed below the mirror, the upper needle in that case serving only as an index. The instrument above described has a coil three inches in diameter and one inch thick; the diameter of the core being one inch. Since its resistance is only about a quarter of an ohm, it is intended for use with circuits of small resistance, such as thermo-currents and the like.

The results of a few experiments made with this new vertical-lantern galvanometer will illustrate the working of the instrument and will demonstrate its delicacy. The apparatus used was not constructed especially for the purpose, but was a part of the University collection.

Induction Currents.—1. The galvanometer was connected with a coil of covered copper wire, No. 11 of the American wire gauge, about ten centimeters long and six in diameter, having a resistance of 0·323 ohm. A small bar magnet 5 centimeters long and weighing six and a half grams, gave, when introduced into the coil, a deflection of 40°. On withdrawing the magnet, the needle moved 40° in the opposite direction.

2. A small coil, 20 centimeters long and 3·5 in diameter, made of No. 16 wire and having a resistance of 0·371 ohm,

through which the current of a Grenet battery exposing four square inches of zinc surface, was passing, was introduced into the center of a large wire coil, whose resistance was 0·295 ohm, connected with the galvanometer. The deflection produced was 20°. The same deflection was observed on making and breaking contact with the battery, the smaller coil remaining within the larger.

3. A coil of No. 14 copper wire, sixty centimeters in diameter, and containing about 40 turns, the resistance of which was 0·85 ohm, was connected with the galvanometer, and placed on the floor. Raising the south side six inches, caused a deflection of 4°. Placing the coil with its plane vertical, a movement of two centimeters to the right or left caused a deflection of 3°, and of twenty centimeters, of 10°. A rotation of 90° gave a deflection of 12° and one of 180°, of 24°. These deflections were of course due to currents generated by the earth's magnetism.

Thermo-currents.—4. Two pieces of No. 22 wire fifteen centimeters long were taken, the one of copper, the other of iron wire, and united at one end by silver solder. On connecting the other ends to the galvanometer, the heat of the hand caused a deflection of the needle of 20°.

5. A thermo-pile of 25 pairs, each of bismuth and antimony, was connected to the instrument. The heat from the hand placed at five centimeters distance caused a deflection of 3°.

6. Two cubes of boiling water acted differentially on the pile. At the distance of five centimeters the deflection was 20°; moving one to ten centimeters, the deflection was reduced to 5°.

Voltaic current.—7. A drop of water was placed on a zinc plate. While one of the connecting copper wires touched the zinc, the other was made to touch the water. The deflection was 16°.

The claim which is here made for the instrument, however, is rather for the general principle of its construction, than for the advantages possessed by the individual galvanometer above described, which was constructed at short notice, to meet an emergency. The comparatively small cost for which it may be fitted to the vertical lantern, the readiness with which it may be brought into use, the brilliantly illuminated circle of light which it gives upon the screen, with its graduated circle and needle, the great range of delicacy which may be given to the instrument by varying the coil and needles, so that all experimental requirements may be answered, and finally, the satisfactory character of its performance as a demonstration galvanometer, all combine to justify the record which is here made of it.

ART. XXXII.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXXV.—*Notice of the occurrence of another Gigantic Cephalopod* (*Architeuthis*) *on the coast of Newfoundland, in December,* 1874; by A. E. VERRILL.

IN an article published in this Journal, February and March, 1875,* I gave a summary of our information concerning twelve specimens of gigantic cephalopods that have been obtained in American waters during a few years past, together with a brief notice of the various specimens that have been described by European writers.†

I am now able to add some important information concerning an additional specimen which was cast ashore last winter at Grand Bank, Fortune Bay, Newfoundland. As in the case of several of the previous specimens, I am deeply indebted to the Rev. M. Harvey for information concerning this one, and also for the jaws and one of the large suckers of the tentacular arms, these being the only parts preserved. Although this specimen went ashore in December, Mr. Harvey did not hear of the event until March, owing to the unusual interruption of travel by the severity of the winter. He informs me that Mr. George Simms, Magistrate of Grand Bank, has stated in a letter to him that he examined the creature a few hours after it went ashore, but not before it had been mutilated by the removal of the tail by the fishermen, who finally cut it up as food for their numerous dogs; and that the long tentacular arms were 26 feet long and 16 inches in circumference (probably meaning at their broad terminal portion); the short arms were "one-third as long as the long ones, and about the same in circumference;" the back of the head or neck was 36 inches in circumference," (evidently meaning the head, behind the bases of the arms); the length of the body "from the junction to the tail" was 10 feet, (apparently meaning from the anterior edge of the mantle to the origin of the caudal fins). He thinks the tail, which had been removed, was about one-third as long as the body, but this is probably overestimated, judging from the Logie Bay specimen (No. 5 of my former papers), in which it was about one-fifth, but it may have been cut off above its proper base. Allowing one-fifth also for the length of the head, the total length would be about 40 feet, the head and body together being about 14. The large sucker, in my possession, is one inch in diameter, across the denticulated rim, and in form and structure agrees closely with those previously described and figured by me from the tentacular arms of Nos. 4 and 5 (vol. ix, Plate IV, figs. 11, 12, 13).

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* This Journal, vol. ix, pp. 123, 177, Plates II–V. See also the American Naturalist, vol. ix, pp. 21, 78, January and February, 1875.

† In the "Journal de Zoologie," vol. iv, No. 2, p. 88, 1875, M. Paul Gervais has also given a summary of the gigantic cephalopods previously known, and has mentioned an additional species (*Architeuthis Mouchezi* Vélain), of which portions were brought to Paris by M. Vélain, from the Island of Saint Paul, where it was cast ashore. He also quotes the brief notice of the animal by M. Vélain (in Comptes rendus, t. lxxx, p. 1002, Seance du April 19, 1875). It is stated that this example belongs to the same group with *Ommastrephes*, and if so it will probably prove to be generically distinct from both of the Newfoundland species. M. Gervais does not refer, in any way, to the several American specimens described by the writer and others.

The jaws are still attached together, in their natural position, by the cartilages.* They agree very closely in form with the large jaws of *Architeuthis princeps* V. (No. 10), figured on Plate V, vol. ix, but they are about one-tenth smaller. The upper jaw measures 111^{mm} in height (front to back); 88^{mm} from tip of beak to front edge of palatine laminæ; 20^{mm} from tip of beak to the base of the notch. The lower jaw measures 96^{mm} in total length; 80^{mm} from tip of beak to front edge of laminæ; 19^{mm} from tip to base of notch.

From the close agreement of these jaws with those of *A. princeps*, there can be very little doubt that they belong to that species; and if so the measurements given will be of great importance as affording additional knowledge of the approximate form and proportions of this, the largest known species.

NOTE.—In "The Zoologist," London, 2d Series, No. 118, p. 4526, July, 1875, there is an article entitled, "Notice of a gigantic Cephalopod (*Dinoteuthis proboscideus*), which was stranded at Dingle, in Kerry, two hundred years ago. By A. G. More, F.L.S." The article is chiefly a reprint of the rude popular accounts written at the time of the capture, and upon these alone Mr. More attempts to found a new genus and species. The one character which he relies upon as of generic value, is the power of projecting the beak in the form of a proboscis. But he apparently does not know that this is habitually done by the various common species of *Ommastrephes*, *Loligo*, etc., and perhaps by all ten-armed cephalopods. There is no reason to suppose, from the published accounts, that this specimen differed in any way from the *Architeuthis monachus*. It was described as 19 feet in total length, the long arms having been mutilated, the part remaining was 11 feet long, and as thick as a man's arm; the short arms varied from 6 to 8 feet in length, and were as thick as a man's leg, and had two rows of large serrated suckers; the proboscis (buccal mass with beak) was the "size of a man's fist;" the beak was "like an eagle's but broader." The whole animal was said to have been as large as a large horse. The measurements given indicate a specimen smaller than several of the American examples, and but little, if any, larger than our No. 5, from Logie Bay.

In the August number of the "Annals and Magazine of Natural History," vol. xvi, p. 123, the same writer has briefly described the beak, and portions of the tentacles and arms of another specimen taken off Boffin Island, on the west coast of Ireland, last April. The tentacular arms are said to have been 30 feet long; the expanded portion 2 feet 9 inches; the large central suckers nearly 1 inch in diameter; those of the outer rows ·5 of an inch; one short arm is said to have been 8 feet long, and 15 inches in circumference at the base, when fresh.

Mr. More believes this to be distinct from the Newfoundland species and refers it to *A. dux*, but his description agrees closely with the corresponding parts of *A. monachus* (No. 5) described by me. He appears to be ignorant of my articles on the subject, published in this Journal.

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* These will be figured in an article on the gigantic cephalopods, now in preparation for the Transactions of the Connecticut Academy of Sciences.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Hypochloric oxide and Euchlorin.*—PEBAL has made an extended investigation into the properties of hypochloric oxide and euchlorin. These substances were first investigated by Davy, the former in 1815, the latter in 1811; but owing to the difficulty and danger of the operation, their accurate composition has remained unknown. The substance experimented on was obtained either by the action of hydrochloric acid diluted with an equal volume of water upon potassium chlorate, or preferably by the action of dilute sulphuric acid upon a mixture of 20 parts potassium chlorate and 48 parts sodium chloride. The acid was contained in a flask with a lateral tubulure, connected with a similar flask containing the mixture. By raising or lowering the former flask, the access of the acid to the latter flask could be regulated. The gas as evolved was first washed, then dried by a calcium chloride tube, then passed into a flask immersed in a freezing mixture, connected with a tube closed by cocks at both ends (called an explosion tube). The composition of the gas in this tube was determined, either (1) by connecting the tube after explosion with a similar tube standing over a saturated salt solution, thus measuring the expansion, and then estimating the chlorine and oxygen by absorption of the former; (2) by passing the exploded gases through potassium iodide solution and titering the iodine set free; or (3) by calculating the composition from the weight of the known volume of the mixture. The author concludes: 1st, that the composition of hypochloric oxide, calculated by Davy and Gay Lussac from their experiments, is correct; his experiments affording for its molecular weight 67·29 and its formula ClO_2; 2d, that the boiling point of the liquid oxide is about 9° C., and not 20° nor 32° as has been asserted; 3d, that euchlorin is a mixture of hypochloric oxide and free chlorine, the proportions of the constituents varying with the mode of preparation; and 4th, that Millon's statement of the existence of a compound Cl_6O_{13} rests on mistaken conclusions.—*Liebig's Annalen*, clxxvii, 1, May, 1875. G. F. B.

2. *On the presence of Sulphuric oxide in the Gaseous products of combustion of Pyrite.*—SCHURER KESTNER finds that the abundant white fumes which accompany the sulphurous oxide which is evolved in the combustion of pyrite, are not sulphuric acid as was supposed, but consist of sulphuric oxide. To ascertain how this substance was produced, he made a series of experiments. In the first, he passed a slow current of sulphurous oxide through a platinum tube forty centimeters long, heated to a temperature but little inferior to that of the pyrites furnace; but with no result. The SO_3 does not therefore come from dissociation of SO_2. In a second experiment, the sulphurous oxide was mixed with twice its volume of air; but the issuing gases did not render turbid a

solution of barium chloride. Finally, the same mixture of gases was passed through the heated tube which was now filled with ferric oxide obtained as a residue in the combustion of the pyrite. Abundant white fumes were evolved which gave a precipitate of barium sulphate. The amount of the sulphuric oxide formed does not exceed two or three per cent of the sulphurous oxide. Its production accounts for the loss of oxygen observed in the sulphuric acid manufacture.—*Bull. Soc. Chim.*, II, xxiii, 437, May, 1875. G. F. B.

3. *On Calcium Hypochlorite from Bleaching Powder.*—**Kingzett** has thrown some light upon the chemical constitution of bleaching powder, a subject upon which very much discussion has recently been had, by obtaining from it calcium hypochlorite. The freezing of a saturated solution of bleaching powder, which yielded needle-shaped crystals called his attention to the matter; he repeated the experiment and isolated the crystals. They dissolved in water, depositing calcium carbonate, the solution evolved chlorine with acids and contained calcium. Another portion of bleaching powder solution evaporated over sulphuric acid and potash in a vacuum, gave a dense crystalline mass, insoluble in alcohol, and having oxidizing and bleaching properties. It smelled of hypochlorous acid, contained chlorine and calcium and evolved free chlorine with hydrochloric acid. A second solution similarly treated yielded 2 grams of a first crop of crystals which gave on analysis 18·54 per cent Ca and 30·20 Cl; the formula $CaCl_2O_2$, $(H_2O)_4$ requires 18·60 Ca and 33·02 Cl. The second crop of crystals, partially dried, as well as freshly prepared crystals gave calcium, chlorine and oxygen in very nearly the atomic ratios 1 : 2 : 2, which are those required by calcium hypochlorite. Hence Kingzett regards Odling's view of the constitution of bleaching powder $Ca\left\{\begin{matrix}Cl\\OCl\end{matrix}\right.$, as the most probable, but believes that it is decomposed by water into $CaCl_2O_2$ and $CaCl_2$.—*J. Chem. Soc.*, II, 404, May, 1875. G. F. B.

4. *Occurrence of Bromoform in Commercial Bromine.*—In titering, by means of potassium iodide, a solution of bromine in water, **Reymann** observed that the result obtained was too low and that the liquid possessed a peculiar odor recalling that of chloroform. Further investigation showed the bromine to be mixed with at least 10 per cent of a substance boiling between 80° and 165°, the principal part of which consists of bromoform. It is readily detected by the influence it has in lessening the solubility of bromine in water as well as by its odor, which is most readily perceived when the bromine is agitated with a solution of potassium iodide, and the whole decolorized by sodium thiosulphate.—*Ber. Berl. Chem. Ges.*, viii, 792, June, 1875. G. F. B.

5. *On the use of Spectrum Analysis in titrition.*—**Vierordt** has proposed to use his exceedingly ingenious quantitative method of spectrum analysis in titrition. In a word, this method measures the intensity of any colored light by the quantity of white lig

necessary to extinguish it. To apply it to volumetric analysis, as for example to the determination of dextrose by Fehling's solution, he would proceed as follows: Add to a measured volume of Fehling's copper solution a measured volume of the sugar solution to be tested, which must contain an amount of sugar less than is required to reduce the whole of the copper. After the reaction is over, therefore, the liquid will still be blue. By the spectroanalytic method, using that portion of the spectrum where the light is most strongly absorbed, the quantity of the still unreduced copper is determined; and of course, its sugar-equivalent. Subtracting from the sugar-equivalent of the copper solution originally used, that now found, the difference is that of the sugar in the solution. The advantages claimed for the method are: 1st, It does not depend for its accuracy upon a definite, quantitatively exact point in the reaction; hence the continual testing and the use of control-experiments to fix this point, are avoided. Moreover, the time required for its application is shorter. 2d, The necessity for great care in the constancy of the test-liquid is avoided. The extinction coefficient is readily and easily determined at the time of use. 3d, The small volume of the liquid (about two cubic centimeters) which is required. 4th, It admits perfectly of control-experiments, since the volume-relations of the liquids and the degree of their dilution may be varied within wide limits. 5th, The result is not affected by the presence of coloring matter. And 6th, It does not require, like the ordinary method, a long practice with it in order to obtain exact results. Results of the use of the method are given which are closely accordant and quite satisfactory.—*Liebig's Annalen*, clxxvii, 31, May, 1875. G. F. B.

6. *Fluorene and fluorene Alcohol.*—Barbier, having shown that fluorene—a hydrocarbon first obtained by Berthelot from coal tar—$C_{12}H_8$, CH_2, may be transformed by oxidation into diphenylene carbonyl $C_{12}H_8$, CO, has now succeeded in producing a series of compounds from it, by taking advantage of the peculiar properties of the carbonyls as first pointed out by Berthelot. When sodium amalgam reacts on diphenylene carbonyl in alcoholic solution, a substance is produced which, after crystallization from boiling benzol, appears as hard white hexagonal plates, having the composition $C_{12}H_8$, CHOH. It is fluorene alcohol. The reaction is

$$C_{12}H_8(CO)+H_2=C_{12}H_8(CH_2O)=C_{12}H_8(CHOH).$$

It melts at 153°, and is oxidized by chromic acid to diphenylene carbonyl again. When heated for some time above its fusing point, it loses water and yields fluorene ether, which melts at about 290°. It is formed also when the alcohol is heated to 150° or 160° with acetic oxide. Analysis gives its composition as $(C_{12}H_8, CH)_2O$. When the alcohol is heated with acetic oxide to 100° for eight hours, an aceto-fluorene ether $(C_{12}H_8, CH)(C_2H_3O)O$ is produced, in rhomboidal plates fusible at 75°. Fluorene alcohol is the first alcohol discovered which loses water by heat alone and forms an ether. It plays the part of an incomplete pseudo-alcohol.—*C. R.*, lxxx, 1396, June, 1875. G. F. B.

7. *On Pararabin, a new Carbohydrate.*—Reichardt has prepared both from the tissue of beets and of carrots, a new carbohydrate, which on account of its close resemblance to Scheibler's arabinic acid, he calls *pararabin.* The root is rasped, the pulp pressed out, treated with water and alcohol to remove everything soluble, digested for several hours with a one per cent solution of hydrochloric acid, heated to boiling and the liquid strained off. Alcohol throws down a gelatinous precipitate, which after washing with alcohol and drying, forms a friable white powder, swelling up in water and dissolving on the addition of an acid when heated. Alkalies precipitate it again, and it gives no sugar by the action of sulphuric acid. Its formula is $C_{12}H_{22}O_{11}$. It differs from arabinic acid by its neutral reaction and its chemical indifference; by its yielding no sugar; by its solubility in acids and precipitation by alkalies instead of the reverse. If, however, it is acted on for a long time by an alkali, or if it be warmed in contact with it, it is converted into arabinic acid. Quantitative experiments showed that 38·5 per cent of the beet pulp was arabinic acid, 54·0 per cent pararabin, and 7·5 per cent cellulose. —*Ber. Berl. Chem. Ges.*, viii, 807, June, 1875. G. F. B.

8. *Friction of Rarefied Gases.*—A. Kundt and E. Warburg have investigated some results of the kinetic theory of gases when the pressure is exceedingly small. This theory presupposes that the mean length of path of the molecules is a quantity that may be neglected in comparison with the linear dimensions of the space filled with the gas. But as this path is inversely as the density, this is equivalent to saying, that the density in a given space must not be too little. It appears that the sliding coefficient for a gas and a solid partition has sensibly a determined value dependent on the nature of the gas, so long as the latter is present in layers thicker than fourteen times the mean length of path; and it is inversely proportional to the pressure. The absolute value of the sliding coefficient is obtained by admitting that the gas molecules are reflected from the partition with its velocity of translation, to 1·4 ($\frac{1}{2}l$),—consequently for air, for which at 760 mms. pressure $l = \cdot000083$ mms., according to Stefan, to $\cdot000058\frac{760}{p}$, p being the pressure in mms. of mercury. The present experiment gives the coefficient about twice as great, or ·0001. From this we may conclude that on the collision of the molecules with the partition the velocities of translation of the two are not perfectly balanced. The present experiments are conducted according to Maxwell's method. The logarithmic decrement of the torsional vibrations executed by a glass disk between two fixed disks near it was measured. The diameter of the moving disk was 159 mms., its weight 61·9 grms., and it was held by a bifilar suspension of two fine silver wires ·063 mms. in diameter. This made the disturbing damping movements so slight that they might

be neglected; with the smallest decrements measured, they only amounted to a little over one per cent, and with nearly all the measurements employed for calculations much less than one per cent, of the total value observed. A substantial simplification of the apparatus employed was thus obtained, one vibrating disk being sufficient. With pressures of the air varying from 750 to 380 mms., the absolute coefficient of friction of air was found to be ·000189. Maxwell found the value ·000198, Meyer by Maxwell's method ·000197, Meyer from transpiration experiments found ·000182 and Puluj ·000185. Calling the coefficient of air 1, that of H was ·488 and of CO_2 ·806, while Graham's transpiration experiments gave ·4855 and ·807. Pure aqueous vapor at 21° C. and 16 mms. pressure gave ·526. To test the theory at low pressures the chief difficulty consisted in filling the space with pure gases. Nothing could be done with caoutchouc connections, which were, therefore, replaced by glass joints, while to secure flexibility and security against fracture by expansion, thick, wide, elastic glass tubes were inserted, bent in three directions at right angles. Exhaustion was produced by a Geissler mercury pump. With pressures from 380 mms. to 1·5 mms. the logarithmic decrement altered from ·0425 to ·0405 in the case of air, and from ·0341 to ·0331 in the case of hydrogen. In carrying the exhaustion still further, great difficulty was experienced in removing the last traces of aqueous vapor. In four cases the logarithmic decrement had the values, ·0180, ·0140, ·0119 and ·0220. In the first the vacuum was obtained directly with the pump, in the second the little gas bubble was allowed to pass from the receiver into a vacuum, and in the third this was continued until nothing passed out even then. In the last case the third vacuum was allowed to stand over night. With a pressure of 760 mms. the value of the decrement was ·0387.—*Phil. Mag.*, 1, 53; *Proc. Roy. Pruss. Acad.*, 1875, p. 160. E. C. P.

9. *Conductibility of Heat by Gases.*—A. KUNDT and E. WARBURG have studied also the flow of heat through gases at very low pressures. As in the case of friction, the law must change when the density becomes so small that the mean length of path becomes appreciable. The difference in temperature here corresponds to the difference in velocity in the case of friction. The experiment consisted in the measurement of the rapidity of cooling of thermometers of various forms in glass cases of different shapes at 0°. At high pressures the effects are masked by air currents, but with low pressures this disturbance disappears. Thus with a spherical thermometer the velocity was independent of the pressure when the latter was contained between 10 mms. and 1 mm., and affords an accurate means of calculating the coefficient of heat conduction. Calling that of hydrogen 1, that of air was found to be ·137, and that of CO^2, ·082 against ·141 and ·103 calculated by Maxwell. The time of cooling of a thermometer in a vacuum diminishes if left to itself, in one experiment changing in twelve hours from 351 to 307 seconds. This change is probably

due to exceedingly minute traces of aqueous vapor from the plug of the stop-cock. The quantity of ponderable matter is so small, and the ease of measuring the velocity of cooling such, that it seems to offer an extremely good test for the quality of a vacuum. In an experiment, under 760 mms. pressure, the time was 225 seconds, under 1·3 mms. 364, and continually increasing the exhaustion the times 664, 555, 602 and 712·5 seconds were obtained. The last number was the result of drying the apparatus in an oil bath at 200° C., and shutting it off at that temperature. To prove that we thus obtain an actual vacuum with regard to heat conduction, a thermometer was manufactured which by slips of glass could be put into two different envelopes. With medium pressures the times of cooling were nearly as one to two, but with the most perfect exhaustion the times became almost identical. Thus at 760 mms. the times were 171 and 114, at 168 mms. 234 and 114, at 9·5 mms. 270 and 116, at 0·5, 280 and 154, and with the last vacuum 576 and 576. Replacing the air by hydrogen, gave 588 and 578, and with carbonic acid 586 and 578. In the experiments of Dulong and Petit, their most perfect vacuum must still have possessed, according to these experiments, its full heat-conducting power. In one series of their experiments the ratio of the quantities of heat carried over by radiation and conduction was 6, and in the other 2·5. Their experiments, therefore, cannot be looked upon as rigorously demonstrating the law founded upon them, the significance of which is, moreover, according to the authors themselves, detracted from by the dependence of the specific heat of mercury on the temperature.—*Phil. Mag.*, 1, 58; *Proc. Roy. Pruss. Acad.*, 1875, p. 160. E. C. P.

10. *Emissive Power of Leaves.*—M. MAQUENNE on comparing the quantity of water evaporated by a cultivated soil during vegetation with that furnished by the rain, finds in general an excess in favor of the former. May not this excess be caused by the dew deposition at night on the plants? When the dew is measured by a pluviometer the results are much too small. The leaves condense far more than surrounding bodies, and their temperature may fall six or eight degrees below the air, showing that their emissive power is much greater than that of the metal surfaces of the pluviometer.

To determine the emissive power of leaves, a Leslie cube was employed; one of its faces was blackened, another covered with leaves, and the two surfaces turned successively to the pile. The temperature of the water did not exceed 40°, to avoid injuring the leaves. The deflections were measured by a mirror and scale, and a twentieth of a degree was easily observed. On trying several kinds of leaves it appeared that their emissive power did not differ greatly, was the same on both sides, and had an average value of 94, that of lampblack being 100.

To measure the absorbent power, a thermopile was formed of a thin sheet of copper riveted to a steel spring. One face was covered with lampblack, the other with the leaf to be examined.

Exposing the two surfaces in turn to the radiation of a metallic blackened box heated by steam, and waiting until the galvanometer needle came to rest, the ratio of the deviations gave the absorbent power of the leaf. From the results it appeared that the absorbent power was sensibly equal to the emissive power, and consequently the amount of dew deposited on plants should be determined by pluviometers painted black.—*Comptes Rendus*, lxxx, 1357. E. C. P.

11. *Velocity of Magnetization.*—M. Deprez, in pursuing his researches on electro-magnets and their application to the registration of very rapid phenomena, the first results of which have already been communicated to the Academy, has been led to investigate what was the effect of the nature of the iron of the electro-magnet upon the duration of the phases of magnetization and demagnetization. For this purpose he employed a register in which the pieces of iron constituting the electro-magnet are removable, the bobbins, armature, style, etc., remaining the same, so as to make evident the influence of the metal of the electro-magnet. To measure the duration of the phases he used the method indicated in his communication on electric chronographs. The metallic portion of the elecro-magnets consisted of cores two mms. in diameter and thirteen mms. in length. The coils contained fourteen meters of wire two mms. in diameter. The battery consisted of one Bunsen cell modified by Delaurier. The varieties of iron tried were the ordinary iron of commerce, the soft iron used specially for telegraphs, malleable cast iron, cast steel stretched and chilled, and gray cast iron. The results were quite unexpected, for all but the last kind of iron gave nearly the same period for magnetization and demagnetization. The first of these was about ·0015, and the second ·00025. The gray cast iron gave still better results, the time of magnetization was reduced to about $\frac{1}{1000}$ of a second. It would seem, therefore, to be the metal which would permit the greatest possible rapidity in the transmission of signals. With his registers, perfectly distinct signals can be obtained at intervals of $\frac{1}{350}$ of a second, or with gray cast iron at intervals of $\frac{1}{500}$ of a second. With a series of signals at regular intervals much greater rapidity is attainable. The superiority of cast iron appears to depend on its molecular texture and not on the quantity of carbon which it contains. It is probable that with soft iron, cast but not forged, still better results will be obtained. The above durations do not include the time employed by the style in traversing its trajectory; it is by adding this to the durations of magnetization and demagnetization the $\frac{1}{350}$ and $\frac{1}{300}$ of a second, according to the case, is found for the total duration of the signal. It therefore includes the demagnetization, the time of fall of the style, the magnetization, and the return of the style to its initial position. These are, moreover, the numbers when only one cell is employed; by increasing the current the rapidity of action is also increased.—*Comptes Rendus*, lxxx, 1353; *Phil. Mag.*, l, 79. E. C. P.

12. *The "Challenger's" Crucial Test of the Wind and Gravitation Theories of the Oceanic Circulation;* by JAMES CROLL. (From pp. 220—225 of Prof. Croll's work on "Climate and Time.")—It has been shown in former chapters that all the facts which have been adduced in support of the gravitation theory are equally well explained by the wind theory. We may now consider a class of facts which do not appear to harmonize with both theories. The recent investigations of the *Challenger* Expedition into the thermal state of the ocean reveal a condition of things which appears to me utterly irreconcilable with the gravitation theory.

It is a condition absolutely essential to the gravitation theory that the surface of the ocean should be highest in equatorial regions and slope downward to either pole. Were water absolutely frictionless, an incline, however small, would be sufficient to produce a surface-flow from the equator to the poles; but to induce such an effect some slope there must be, or gravitation could exercise no power in drawing the surface-water polewards.

The researches of the *Challenger* Expedition bring to light the striking and important fact that the general surface of the North Atlantic in order to produce equilibrium must stand at a higher level than at the equator. In other words, the surface of the Atlantic is lowest at the equator, and rises with a gentle slope to well nigh the latitude of England. If this be the case, then it is mechanically impossible that, as far as the North Atlantic is concerned, there can be any such general movement as Dr. Carpenter believes. Gravitation can no more cause the surface-water of the Atlantic to flow toward the arctic regions than it can compel the waters of the Gulf of Mexico up the Mississippi into the Missouri. The impossibility is equally great in both cases.

In order to prove what has been stated, let us take a section of the mid-Atlantic, north and south, across the equator; and, to give the gravitation theory every advantage, let us select that particular section adopted by Dr. Carpenter as the one of all others most favorable to his theory, viz: Section marked No. VIII. in his memoir lately read before the Royal Geographical Society.*

The fact that the polar cold water comes so near the surface at the equator is regarded by Dr. Carpenter as evidence in favor of the gravitation theory. On first looking at Dr. Carpenter's section it forcibly struck me that if it was accurately drawn, the ocean to be in equilibrium would require to stand at a higher level in the North Atlantic than at the equator. In order, therefore, to determine whether this is the case or not I asked the hydrographer of the Admiralty to favor me with the temperature soundings indicated in the section, a favor which was most obligingly granted. The following are the temperature soundings at the three stations

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* Proc. Roy. Geog. Soc., vol. xviii, p. 362. A more advantageous section might have been chosen, but this will suffice. The section referred to is shown in Plate III. The peculiarity of this section, as will be observed, is the thinness of the warm strata at the equator, as compared with that of the heated water in the North Atlantic.

A, B, and C. The temperature of C is the mean of six soundings taken along near the equator:—

Depth in Fathoms.	A	B	C Mean of six temperature soundings near equator.	
	Lat. 37° 54′ N. Long. 41° 44′ W. Temperature.	Lat. 23° 10′ N. Long. 38° 42′ W. Temperature.	Depth in fathoms.	Temperature.
Surface.	70°·0	72°·0	Surface.	77°·9
100	63·5	67·0	10	77·2
200	60·6	57·6	20	77·1
300	60·0	52·5	30	76·9
400	54·8	47·7	40	71·7
500	46·7	43·7	50	64·0
600	41·6	41·7	60	60·4
700	40·6	40·6	70	59·4
800	38·1	39·4	80	58·0
900	37·8	39·2	90	58·0
1000	37·9	38·3	100	55·6
1100	37·1	38·0	150	51·0
1200	37·1	37·6	200	46·6
1300	37·2	36·7	300	42·2
1400	37·1	36·9	400	40·3
1500	---	36·7	500	38·9
2700	35·2	---	600	39·2
2720	---	35·4	700	39·0
			800	39·1
			900	38·2
			1000	36·9
			1100	37·6
			1200	36·7
			1300	35·8
			1400	36·4
			1500	36·1
			Bottom.	34·7

On computing the extent to which the three columns A, B, and C, are each expanded by heat according to Muncke's table of the expansion of sea water for every degree Fahrenheit, I found that column B, in order to be in equilibrium with C (the equatorial column), would require to have its surface standing fully 2 feet 6 inches above the level of column C, and column A fully 3 feet 6 inches above that column. In short, it is evident that there must be a gradual rise from the equator to latitude 38° N. of 3½ feet. Any one can verify the accuracy of these results by making the necessary computations for himself.*

I may observe that, had column C extended to the same depth as columns A and B, the difference of level would be considerably greater, for column C requires to balance only that portion of columns A and B which lies above the level of its base. Suppose a depth of ocean equal to that of column C to extend to the north pole, and the polar water to have a uniform temperature of 32° from the surface to the bottom, then, in order to produce equilibrium, the surface of the ocean at the equator would require to be 4 feet 6 inches above that at the pole. But the surface of the ocean at B would be 7 feet, and at A 8 feet, above the poles. Gravitation never could have caused the ocean to assume this form. It is impossible that this immense mass of warm water, extending to such a depth in the North Atlantic, could have been brought from equatorial regions by means of gravitation. And, even if we suppose this accumulation of warm water can be accounted for by some other means, still its presence precludes the possibility of any such surface-flow as that advocated by Dr. Carpenter. For so long as the North Atlantic stands 3½ feet above the level of the equator, gravitation can never move the equatorial waters poleward.

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* The temperature of column C in Dr. Carpenter's section is somewhat less than that given in the foregoing table; so that, according to that section, the difference of level between column C and columns A and B would be greater than my estimate.

There is another feature of this section irreconcilable with the gravitation theory. It will be observed that the accumulation of warm water is all in the North Atlantic, and that there is little or none in the south. But according to the gravitation theory it ought to have been the reverse. For owing to the unrestricted communication between the equatorial and antarctic regions, the general flow of water toward the south pole is, according to that theory, supposed to be greater than toward the north, and consequently the quantity of warm equatorial water in the South Atlantic ought also to be greater. Dr. Carpenter himself seems to be aware of this difficulty besetting the theory, and meets it by stating that "the upper stratum of the North Atlantic is not nearly as much cooled down by its limited polar underflow, as that of the South Atlantic is by the vast movement of antarctic water which is constantly taking place toward the equator." But this "vast movement of antarctic water" necessarily implies a vast counter-movement of warm surface-water. So that if there is more polar water in the South Atlantic to produce the cooling effect, there should likewise be more warm water to be cooled.

According to the wind theory of oceanic circulation the explanation of the whole phenomena is simple and obvious. It has already been shown that owing to the fact that the S. E. trades are stronger than the N. E., and blow constantly over upon the northern hemisphere, the warm surface-water of the South Atlantic is drifted across the equator. It is then carried by the equatorial current into the Gulf of Mexico, and afterward of course forms a part of the Gulf-stream.

The North Atlantic, on the other hand, not only does not lose its surface heat like the equatorial and South Atlantic, but it receives from the Gulf-stream in the form of warm water an amount of heat, as we have seen, equal to one-fourth of all the heat which it receives from the sun. The reason why the warm surface strata are so much thicker on the North Atlantic than on the equatorial regions is perfectly obvious. The surface-water at the equator is swept into the Gulf of Mexico by the trade winds and the equatorial current, as rapidly as it is heated by the sun, so that it has not time to gather to any great depth. But all this warm water is carried by the Gulf-stream into the North Atlantic, where it accumulates. That this great depth of warm water in the North Atlantic, represented in the section, is derived from the Gulf-stream, and not from a direct flow from the equator due to gravitation, is further evident from the fact that temperature sounding A in latitude 38° N. is made through that immense body of warm water, upwards of 300 fathoms thick, extending from Bermuda to near the Azores, discovered by the *Challenger* Expedition, and justly regarded by Captain Nares as an offshoot of the Gulf-stream. This, in Captain Nares's Report, is No. 8 "temperature sounding," between Bermuda and the Azores; sounding B is No. 6 "temperature curve," between Teneriffe and St. Thomas.

There is an additional reason to the one already stated why the surface temperature of the South Atlantic should be so much below that of the North. It is perfectly true that whatever amount of water is transferred from the southern hemisphere to the northern must be compensated by an equal amount from the northern to the southern hemisphere, nevertheless the warm water which is carried off the South Atlantic by the winds is not directly compensated by water from the north, but by that cold antarctic current whose existence is so well known to mariners from the immense masses of ice which it brings from the Southern Ocean.

Thermal Condition of the Southern Ocean.—The thermal condition of the Southern Ocean, as ascertained by the *Challenger* Expedition, appears to me to be also irreconcilable with the gravitation theory. Between the parallels of latitude 65° 42′ S. and 50° 1′ S., the ocean, with the exception of a thin stratum at the surface heated by the sun's rays, was found, down to the depth of about 200 fathoms, to be several degrees colder than the water underneath.* The cold upper stratum is evidently an antarctic current, and the warm underlying water an equatorial under current. But, according to the gravitation theory, the colder water should be underneath.

The very fact of a mass of water, 200 fathoms deep and extending over fifteen degrees of latitude, remaining above water of three or four degrees higher temperature, shows how little influence difference of temperature has in producing motion. If it had the potency which some attribute to it, one would suppose that this cold stratum should sink down and displace the warm water underneath. If difference of density is sufficient to move the water horizontally, surely it must be more than sufficient to cause it to sink vertically.

13. *On the artificial imitation of magnetipolar native platinum;* by M. DAUBRÉE.—M. Daubrée, in a memoir read before the

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* Captain Nares's Report, July 30, 1874.

French Academy of Sciences in March, 1875, and published in vol. lxxx, of the Comptes Rendus, gives the results of a series of experiments on platinum, in which he succeeded in imitating entirely the platinum having magnetic polarity, by combining iron with the metal in fusion.

Daubrée observes, in his introductory remarks, that in washing the auriferous sands of the Ural, some gold is left in the residue associated with ferruginous grains. To separate the latter a strong native magnet is first used; and after this takes up no more, a magnet of native platinum—as Von Kokscharow first made known in 1866—will remove ferruginous grains of notable quantity, as if it were a stronger magnet than the native magnetite magnets. The presence of 12 to 19 per cent of iron in this variety of platinum has long been known, it having been first examined by Berzelius; and Breithaupt made the variety a distinct species, calling it "*Eisenplatin.*" Gustaf Rose, thinking that the iron present was insufficient to account for the magnetic property, supposed that iridium contributed to it.

In a piece of this iron-platinum weighing twelve grams, received from the Ural, Daubrée found three axes and six poles.

Before proceeding with his experiments for reproducing this magnetipolar platinum, Daubrée sought to ascertain what effect fusion would have upon the ore. On fusing it there were some sparks thrown off, due apparently to the combustion of a little iron, and a dull surface film was formed. When cooled again, after fusion for about one minute, the magnetic property was somewhat weakened and polarity was lost, evidently owing to the loss of iron.

In his experiments, 24 grams of platinum were fused with 6 grams of very soft iron wire, the iron being twisted into a cord and added when the platinum was in full fusion. Immediately on the introduction of the iron it was instantly dissolved, giving out, as in the other case, some sparks, and making a surface scoria, although the substance remained in fusion only a fraction of a minute. The button, when taken out from the crucible, proved to have very marked magnetic polarity; and when it was afterward broken to fragments by a hammer, in an attempt to beat it into a bar, each fragment was equally magnetipolar. A very small bar was afterward cast; this had energetic poles of opposite polarity, which remained after the crust of scoria had been removed. These poles were four in number, two at each extremity of the bar. The hardness of the alloy was a little less than that of apatite. An analysis of the product obtained in the first trial, made at the Ecole des Mines, gave iron 16·87, platinum 83·05= 99·92. Its specific gravity was 15·66. The specific gravity of the second alloy was 15·70, showing that in composition it was very like the first. Both are, therefore, closely similar in composition to the native magnetipolar platinum.

Others alloys were made with 50 to 75 per cent of iron; but these had no polarity. An alloy with 21·6 per cent of iron, made by Berthier and preserved in the laboratory of the Ecole des Mines, although imperfectly fused, was magnetipolar. On the other hand, alloys containing but a feeble proportion of iron were found to be not magnetipolar.

The polarity of the alloy obtained by fusion existed in the mass on cooling, and was not imparted by touch. It seemed most natural to refer this polarity to the inductive influence of the earth's magnetism. To ascertain the fact on this point a little platinum bar was placed, while still in fusion, exactly in the plane of the magnetic meridian. After solidifying, it was put, while still very hot, parallel to the dipping needle. It was then found that the bar had two energetic poles, and that the end toward the magnetic north repulsed strongly the north end of a magnetic needle. Afterward, on fusing again and reversing the position, the polarity was reversed.

These decisive experiments of Daubrée illustrate, he says, like those of M. Sidot with magnetic pyrites, the importance of the earth's general action in determining the polarity of different magnetic minerals and rocks at the time when they are formed.

14. *How to teach Chemistry.*—Dr. Edward Frankland's six lectures delivered at the Royal College of Chemistry in June, 1872, have been summarised and edited by George Chalouer, F.C.S.; the Lectures on Chemistry at the Brisbeck Institution in a small 12mo volume of only 83 pages, republished by Messrs. Lindsay and Blakiston of Philadelphia in a neat form, and abundantly illustrated, chiefly by engravings from Bloxam's Textbook of Chemistry.—This is a truly valuable contribution to the means of instruction. The six lectures discuss in synoptic manner: (I.) Natural Forces, Chemical Force, Chemical Action, Examination of Water, with Experiments 1 to 12. (II.) Hydrogen, Oxygen, Exact Composition of Water (Experiments 13 to 17 and 26 to 30). (III.) Properties of Water, Ozone, Hydroxyl, Examination of Hydrochloric Acid, Chlorine, study of Hydrochloric Acid (Experiments 18 to 25 and 31 to 38). (IV.) Oxycompounds of Chlorine, Boron and its Oxyd, Carbon and its Oxyds, Nitrogen, Igniting points (Experiments 39–64). (V.) Oxycompounds of Nitrogen, Ammonia (Experiments 65 to 88.) (VI.) Doctrine of Atomicity, Laws of Condensation in volume, Replacement of Hydrogen with its Compounds, Sulphur, Sulphuretted Hydrogen, Oxyds of Sulphur, Sulphuric acid and its manufacture, Hyposulphurous acid, Conclusion (Experiments 89 to 109). In an Appendix is given (I) Syllabus of Elementary Course in Organic Chemistry. (II.) Extract from a Report by Dr. Frankland, List of Experiments. (III.) List of Apparatus for teaching Elementary Chemistry, Elementary Stage. (IV.) List of Chemical Apparatus for Special Important Illustration.

This little manual will be found an invaluable adjunct to the lecture room and is of special importance to the teacher who seeks trustworthy and novel illustrations for guidance in imparting a knowledge of chemical principles and methods to his classes. The hand of the master is seen on every page.

II. GEOLOGY AND NATURAL HISTORY.

1. *Cotemporaneous formation, in the thermal waters at Bourbonne-les-Bains, of different Mineral Species.* — M. DAUBRÉE, (C. R. Feb. and March, 1875, t. lxxx) describes the following minerals as of recent formation from these thermal waters: *tetrahedrite* (gray antimonial copper), *chalcopyrite* (copper pyrites), *bornite* (variegated copper or phillipsite), *chalcocite* (sulphide of copper), *pyrite*, *galenite*, *anglesite*, *calcite* and *chabazite.* By pumping, the bottom of an old well, at Bourbonne-les-Bains, called the Roman well, was laid bare. The material was a black argillaceous earth containing bits of wood, nuts, etc., and then, at a lower level, thousands of roman coins of bronze, silver and gold, besides statuettes, rings and pins. Besides this archeological discovery, there was found, still lower down, a bed of pebbles partly cemented by substances having a metallic lustre and neatly crystallized. A collection of the specimens were sent by M. Trautmann, Engineer in Chief of the Mines, to the Minister of Public Works, who transmitted them to Prof. Daubrée. Daubrée found abundant evidence that the above-mentioned ores, present in the specimens, were all formed out of the coins through the action of the warm waters. The different copper ores were all in good crystals—chalcocite in twinned hexagonal tables, chalcopyrite in octahedrons, bornite in octahedrons and cubes, tetrahedrite in tetrahedrons—besides being also in crusts.

The temperature of the waters is near 60° C. The substances the waters hold in solution are chiefly chlorides and sulphates of the alkalies, and of lime and magnesia, as well as bromides and carbonates of iron and lime, an alkaline silicate, and traces of arsenic and manganese, with no sulphides. The total weight of the residue on evaporation is 7 to 8 grams per litre. Daubrée observes that the sulphides would have been formed through the reduction of the sulphates by the organic matter present. It is remarkable, as he says, that sulphides so unlike should have been crystallized together under circumstances apparently the same. The antimony is referred by him to the coins, some of which probably contained it. The silver coins are less corroded than the bronze and hence the absence of silver products.

The chabazite was formed in connection with a cement of fragments of brick and lime, in a Roman gallery through which run the hot water. The cavities in the brick, caused by the heat, are sometimes lined with colorless rhombohedral crystals, nearly cubic in form, which have the characters of chabazite. In the lime, small crystals occur of a right rectangular form which have not yet been determined with certainty, but which resemble much those of lime-harmotome found under similar circumstances at Plombières.

Such facts teach us, says Daubrée, how important has been the agency of the water imbibed by, or traversing, rocks, in all parts of the earth's crust, especially in regions where terrestrial heat is a little elevated, giving the moisture special energy in effecting mineral changes.

2. *Man of the Quaternary;* by JOHN EVANS. (From the Address of Mr. Evans, President of the Geological Society of London, Feb. 19, 1875.)—Of the great geological changes in the surface-configuration of our country which Man has witnessed there can be no doubt. But though, from the evidence of the deposits themselves, it appears impossible to arrive at any absolute conclusion as to the time involved in these changes, still, considering the numerous localities in which these implement-bearing beds have now been found, as well as other beds which, though apparently devoid of implements, yet being of the same general character and containing the same fauna, are therefore presumably of the same date, it does appear possible that from their geological position we might ascertain some epoch beyond which their age cannot be carried, and thus be enabled to assign an extreme limit to their antiquity. I believe that I am right in stating that until within the last two or three years it was the generally received opinion of geologists that the earliest known traces of the occupation of this portion of the globe by man were posterior in time to what is known as the Glacial Period. The discovery, however, of a portion of a human fibula during the exploration of the Victoria Cave, near Settle, in a deposit overlain by a stiff glacial clay containing ice-scratched pebbles, has been regarded as conclusive that man lived in England prior to the last interglacial period. I must confess that, in common with some others, I do not regard this question as conclusively settled by any such isolated piece of evidence; and that whatever further testimony may eventually be adduced as to so early an occupation of this country by man, there are, in my opinion, possibilities, in this particular case, of the clay being either to some extent reconstituted or even accidentally redeposited, which make it safer to suspend our judgment until the evidence is corroborated.

Mr. James Geike, however, arguing on more general and therefore on safer grounds, has come to the conclusion that the paleolithic deposits are of preglacial and interglacial age, and do not, in any part, belong to postglacial times. From their comparative absence in the midland and northern counties, and in Wales, Scotland, and Ireland (regions which have again and again been subjected to the grinding action of land ice and the destructive influence of the sea), and from their presence in districts which were never overwhelmed by the confluent ice masses, and in regions which were not submerged during the last great depression of the land in late glacial times, he thinks that in these latter districts the valley-gravels form a continuous series of records from preglacial times to the present day. He maintains that the paleolithic beds dovetail into the glacial drifts, and are overlapped by marine deposits thrown down during the final cold period; and, further, that it may be said for certain that no paleolithic bed can be shown to belong to a more recent date than the mild era which preceded the last great submergence.

If this view could be accepted, there is no doubt that, as Mr. Geikie remarks, many apparent anomalies would receive a simple and satisfactory explanation.

It appears to me, however, that there are great difficulties in accepting this view; and though I do not think that an anniversary address ought in any way to partake of a controversial character, I may venture to point out that the whole of Mr. Geikie's conclusions appear to be based on his correlation of Mr. Searles V. Wood, jun.'s lower and middle glacial beds, and even his upper glacial deposits, with the lower glacial beds and till of Scotland and the northwestern districts of England. Even the general submergence of the eastern districts he assigns to another and a far earlier date than that of the Welsh area; and, indeed, he finds room for the whole of the denudation of the glacial deposits in the east of England, as well as for the excavation of the valleys as we at present find them, and the deposition of all the paleolithic gravels between those two submergences. Such views, founded on long and careful investigation, of the phenomena, though perhaps principally confined to those exhibited in Scotland, cannot be summarily and hastily dismissed; but their author will no doubt pardon those who are not prepared at once to accept them. In the mean time we must rest content with knowing that, so far as the paleolithic deposits of the east of England are concerned (and if the identity in the form of the implements affords any safe chronological index, the beds of the same character in other parts of this country and the north of France are of the same date), they are all distinctly postglacial in the sense in which that term is employed by Mr. Searles V. Wood, jun.

Take, for example, the case of Hoxne, where the implement-bearing beds repose on a trough cut out in the upper glacial boulder-clay, which itself rests on middle glacial sands and gravels.

At Icklingham, again, the paleolithic gravels occur in a valley cut through the upper glacial boulder-clay, and appear to rest on middle glacial beds. At Bedford, the valley of the Ouse is cut through the same boulder-clay, many of the pebbles from which, and the subjacent beds, form constituent parts of the paleolithic gravels. The same feature prevails in the Ealing and Acton gravels, in which transported pebbles of the older rocks are of not unfrequent occurrence.

Again, near Brandon, some of the higher gravels containing the implements show a very large percentage of the quartzite pebbles from the middle glacial beds, some few of which, indeed, have supplied the material from which implements had been chipped. There can therefore be no doubt that they belong to a period subsequent to the submergence during which the middle and upper glacial beds were deposited, and to a time when the old sea-bottom had been long enough converted into dry land for it to become habitable by man and the numerous mammals of the Quaternary fauna.

I do not, of course, wish it in any way to be implied that I regard no part of the surface configuration of our island as of preglacial or glacial date. On the contrary, I am inclined to think that many of our principal valleys were already marked out in preglacial times, and that a large proportion of the whole number were excavated to a portion of their present depth, either by the direct action of ice, or during the last emergence of the land from beneath the waters of the sea.

But though, so far as the makers of the earliest implements hitherto discovered in Britain are concerned, we cannot safely carry back their existence to preglacial times, it by no means follows that the earliest traces of the occupation even of this part of the world by man have as yet been discovered. The Abbe Bourgeois, indeed, would carry man back to Lower Miocene times, relying on implements presumed to have been found in beds of the Calcaire de Beauce, at Thenay, near Pontlevoy. He candidly acknowledges, however, that the implements offer a complete identity with those found on the surface; and I cannot but suspect some possible error of observation as to their occurrence in the beds. Did they really belong to them, we should have the remarkable fact that at that remote period, characterized by mammals as distinct from those of the present day as the *Acerotherium* is from the Rhinoceros, or the *Mastodon* from the Elephant, primæval man was fashioning implements indistinguishable from those of neolithic times; while it is not until we come to the Sables de l'Orléannais, which are superimposed upon the Calcaire de Beauce, that we find the earliest trace of an anthropomorphous ape, in the shape of the *Hylobates antiquus.* The *Dryopithecus*, it will be remembered, belongs to the Upper Miocene.

While speaking of the possible errors of observation, I may mention that in Sweden the Södertelje hut, which has often been cited as affording evidence of the great antiquity of the human occupation of that country, is no longer regarded as belonging to so ancient a period as was formerly assigned to it, but is considered as being of comparatively modern date.

But, returning to the main question, though for the present we seem unable to find any satisfactory evidence of the existence of man in western Europe before the glacial period, it by no means follows that none such will eventually be found. It must, moreover, never be forgotten that it is not in this part of the world that naturalists would be led to look for the cradle of the human race. This is far more probably to be sought in a warmer clime, and amidst a more luxurious vegetation, yielding throughout the year some readily available means of subsistence both to man and to animals that would serve him as food. In the earliest as well as in later times, the center of the migrations of the human race may well have lain in the far East, and the course of their wanderings, as in after days, been even then "westward, toward the setting sun." Most remarkable it is that implements, which in form, though not in material, are indistinguishable from those of our river-drifts, have been found in stratified beds of uncertain age in Southern Africa and in the so-called lateritic deposits of the south of India.

The first discovery of paleolithic implements in Madras was made about ten years ago by Mr. R. Bruce Foote, of the Indian Geological Survey; but at that time the absence of organic remains in the beds containing the implements rendered it almost impossible to arrive at any satisfactory conclusion, either as to their age or mode of deposition. In the Records of the Geological Survey of India for the year 1873, Mr. Medlicott, however, gives an account of a quartzite implement of precisely the same class as those found in Southern India, which was discovered in the ossiferous deposits of the Narbadá valley. These deposits, which, by the late Dr. Falconer, were regarded as Pliocene, Mr. Medlicott sees reason to place among those of Pleistocene age. Whichever view may eventually prove to be correct, we have in India, as in Europe, evidence of man having coexisted with animals now long since extinct; and the *Elephas* (or *Stegodon*) *insignis*, the *Bos* and *Hippopotamus Namadicus* seem there to take the place of the allied members of the European Quaternary fauna as his contemporaries. This Narbadá discovery remains, however, a solitary instance, but must surely lead to other and even more interesting results from the investigations of those engaged in the wide field of geological researches in India.

From Borneo, where I have reason to hope there are, at the present moment, some cavern-investigations being carried on under the auspices of several Fellows of this Society, who have kindly aided me by their support, it is not, I think, unreasonable to expect that some light may be thrown on the antiquity of man in the far East. When we look back upon all the large array of facts which have been accumulated on this subject during the last sixteen years, we may find good ground for encouragement, and rest assured that in this as well as other departments of knowledge that prophecy of the wise man, which Bacon inscribed on the frontispiece of his great work, will be fulfilled—*Multi pertransibunt, et augebitur scientia.*

3. *Protriton petrolei of the Upper Coal-Measures of Muse and Millery, France, a naked-skinned Salamander.*—M. A. GAUDRY, describes the specimens, to which he has given the above name, in the Bulletin of the Geological Society of France, 1875, p. 299, and presents figures of them on plates VII and VIII. Remains of skeletons of seventeen individuals have been found, indicating a variation in length from 35 to 45 millimeters. The head is much larger proportionally than in the Salamander, and the tail much shorter. The orbits are very large. There are 29 vertebræ, 3 cervical, 10 dorsal, 8 lumbar and 8 very small caudal; the ribs are very short; there are only traces of a pelvis, owing probably to its having

been incompletely ossified. The fore and hind limbs are about equal in length and each 4-fingered. M. Gaudry remarks that the *Pelion* (*Raniceps*) *Lyelli* Wyman, of the Ohio Carboniferous, which Wyman was disposed to refer to the Batrachians, was probably related to the Protriton; and that the *Apateon pedestris* of Münster-Appel may perhaps be identical with the *Protriton petrolei.*

4. *Period of Extinction of the ancient Fauna of the Island of Rodriguez;* by M. ALPH. MILNE-EDWARDS.—A manuscript entitled "Relation de l' ile Rodrigue," found in the Ministère de la Marine, along with evidence that it was published anterior to 1730 and probably not earlier than 1729, describes the species of the island existing at the time it was written, and among them all the species now known to be extinct, including the *Solitaire* and the extinct species named by A. Milne-Edwards, *Erythromachus Leguati*, *Ardea megacephala*, *Athene murivora*, and *Necropsittacus Rodericanus.* In 1761, when the astronomer Pingré was living there, the Solitaires had become so rare that he knew of them only from report—none having been seen by him. The extinction of the birds probably went on rapidly between 1730 and 1760, as may be inferred from the documents at the Ministère de la Marine.

The Land-tortoises became extinct somewhat later. These tortoises were part of the regular provisions of the shipping of the Compagnie des Indes. M. Desforge-Boucher, in his reports to the Company in 1759 and 1760, enumerates the vessels sent for the land-tortoises, and shows that they took away in less than 18 months over 30,000. It is not surprising, the author remarks, that these animals, on so small an island, notwithstanding their fecundity, could not withstand such means of destruction. Hungry man was the agent of extermination both for the tortoises and the birds.—*Comptes Rend.*, May, 1875.

These gigantic land tortorses, while extinct on the Islands of Mauritius, Rodriguez, and Réunion, are living on that of Aldabra, another of the Mascarene Group. But there is danger of its extinction there. To prevent this, if possible, a memorial has been addressed to the Governor and Commander-in-chief of Mauritius and its dependencies, signed by the Presidents of the Royal and Royal Geographical Societies of London and other men of science, calling the attention of the Colonial Government to the subject, and asking that some means may be devised for "saving the last examples of a cotemporary of the Dodo and Solitaire." The memorial is given in full in an article in *Nature* of July 29th, by Mr. Albert Günther.

5. *Reliquiæ Aquitanicæ;* being Contributions to Archæology and Palæontology of Périgord and the adjoining Provinces of Southern France; by EDOUARD LARTET and HENRY CHRISTY. Edited by THOMAS RUPERT JONES, F.R.S., F.G.S., &c., Prof. Geol. Roy. Mil. and Staff Colleges, Sandhurst. Part XVI, May, 1875. Pages 225–256 and 183–187. Plate C. IX and X.—This part of the Reliquiæ Aquitanicæ contains, among its interesting facts and illustrations, a figure of a fish in outline, from a Reindeer

jawbone, referable probably to a species of *Squalius*, obtained at Laugerie Basse. A chapter on the birds whose bones occur in the caves, by Alphonse Milne-Edwards, describes relics of three Eagles, *Falco fulvus*, *Aquila clanga*, and *Haliætus albicilla;* the common Buzzard, *Buteo vulgaris;* four Falcons, *Falco communis*, *F. subbuteo*, *F. tinnunculus*, *F. milvus;* two Vultures, *Vultur barbatus* and *V. monachus;* the Owls, *Strix bubo*, *S. brachyotus*, *S. flammea*, *Noctua minor*, *Glaucideum passerinum*, *Nyctea nivea;* also, *Corvus corax*, *C. coronatus*, *C. cornix* (a northern species), *C. monedula* (the Jackdaw), *Pyrrhocorax alpinus*, *P. primigenius*, *Fregilus graculus*, *Nucifraga caryocatactes* (the Scandinavian variety), *Pica caudata* (Magpie), *Loxia curvirostra* (Cross-bill, common now in northern Europe and Greenland), *Ligurinus chloris*, *Fringilla nivalis* (lives now on lofty mountains), *Alauda arvensis* (Sky-lark), *Turdus viscivorus*, *Ampelis garrulus* (Waxwing, rarely now seen in France), *Motacilla phœnicura*, *Hirundo rupestris* (Crag-Martins, now common in the Alps and Pyrenees), *Alcedo ispida* (Kingfisher), *Columba livia* (same with common pigeon), *Lagopus albus* (Willow-Grouse, found at the north and not in temperate Europe), *Lagopus mutus* (Ptarmigan, species of the Alps and Pyrenees), *Tetrao urogallus*, *T. tetrix*, *T. perdix* (Gray Partridge), *Gallus*, the Cock, (found along with bones of *Ursus spelæus*, *Rhinoceros*, &c.), *Œdicnemus crepitans*, *Rallus aquaticus*, *Gallinula chloropus*, *Grus primigenia;* *Cygnus ferus* (Wild Swan, a species of the Polar regions). *Anas boschas* (Wild Duck), *Anas querquedula* (the Summer Teal.

Many of the birds are cold-climate species, like the Reindeer with which their remains are associated. Part of them were taken into the caves for food, while others appeared to have been washed in by streams.

The remaining pages are occupied with a chapter on the Stones, Stone-flakes and implements from the cave at Les Eyzies, and the commencement of a chapter on Fossil Man from La Madelaine and Laugerie Basse, by E. T. Harvy, with two plates illustrating the same.

6. *Structure of Stone Mountain, a granitic mass in Georgia.*—The well-known "Stone Mountain" in DeKalb County, Georgia, twenty miles southeast of Atlanta, on the railroad to Augusta, is a solid bald mass of granite, by estimate 1500 to 2000 feet in height. The northeast side is perpendicular, unbroken, and smooth; the northwest side is inclined so as to be of easy ascent; while the west and southwest are so steep as to be barely accessible. On the inclined surface, the rock breaks off in layers that are from a few inches to several feet thick, which structure may be due to shrinking in cooling and to atmospheric influences, together with the heat of the sun. The rock is perfectly homogeneous with no trace of stratification—a pure, whitish granite. There is no doubt that below the surface lamination, a piece could be quarried out a quarter of a mile in length if man could command the means. This granite exists over a wide region of country and

is much used for building purposes.—*Letter from Mr. E. Hillyer of Rome, Georgia.*

7. *Genus Anomalodonta of S. A. Miller.*—The criticism with regard to the genus Anomalodonta and the use of the name, by Prof. C. A. White, published in this Journal, ix, 318 (April, 1875) is replied to by Mr. Miller in the July number of the Cincinnati Quarterly Journal of Science.

8. *Expansion of rocks by heat.*—M. Pfaff (Z. S. Geol. Ges., xxviv, 401) has determined for the expansion of the granite of the Fichtelgebirge between the ordinary temperature and a red heat (about 1,180 C.), 0·0168; for the red porphyry of the Tyrol, 0·0127; for the basalt of Auvergne, 0·0120.

9. *Geology of Eastern Massachusetts.*—Mr. W. W. Dodge has a paper entitled "Notes on the Geology of Eastern Massachusetts in the Proceedings of the Boston Society of Natural History, February 3, 1875, vol. xvii, p. 388.

10. *South American Geology.*—Mr. A. Hyatt has described, in the Proceedings of the Boston Society of Natural History, xvii, 365, a number of species of Jurassic and Cretaceous Ammonites from northern Bolivia and Peru, collected by Prof. James Orton. For the Cretaceous species which were formerly referred to the genus Ceratites, and which Quenstedt had shown to be not of this genus, but Ammonites, Mr. Hyatt has instituted the new genus *Buchiceras*—named in honor of Von Buch.

11. *On the composition of Coal and on the methods of arriving at it, with deductions and remarks on Coal in general; illustrated on a sample of Coal from the Lower Coal Series of Missouri, and on the Water Supply of Columbia, Missouri;* by P. Schweitzer, Ph.D., Prof. Chem. State University of Missouri. Contributions from the laboratory of the University, pp. 156–193, published in the Catalogue of the University, Jefferson City, 1875.—Prof. Schweitzer gives the results of detailed analyses of the coal, including its impurities. He concludes that the sulphur in the coal above what is combined with iron is in the free state (through the alteration of pyrite, FeS_2), and not in combination with organic matter.

12. *Revue de Géologie pour les Années* 1871 *and* 1872, par M. Delesse et M. de Lapparent. Tome xi, 262 pp. 8vo, Paris, 1875.—Geologists will find this Annual a very valuable aid in keeping up with the progress of the science. It is full in Lithological Geology, and gives abstracts also of the more important papers in Historical, Geographical and Dynamical Geology.

13. *Devonian trachyte of Queensland, Australia.*—The trachyte of Gladstone, intersecting Devonian rocks, has the composition nearly of that of the Puy-de-Dome. Daintree obtained, Silica 67·80, alumina 14·67, sesquioxide of iron 5·35, potash 5·65, soda 4·60, water of constitution 0·70, do. hygroscopic 0·60=99·37.—*Q. J. Geol. Soc.*, xxviii, 312.

14. *Phosphate of Lime of Bamle, Norway.*—Occurs in nearly horizontal beds, two to six feet thick in crystalline schists, and is

whitish and crystalline. An article on the localities of Southern Norway, by MM. Brögger and Reusch, is soon to appear. Among the associated minerals to be described in it, are anorthite in large and beautiful crystals, resembling the lepolite or amphodelite of Orijarfvi, and enstatite in crystals four inches long. The latter occurs changed to serpentine.—*Letter to J. D. Dana from Professor vom Rath, of Bonn.*

15. *Mineralogische Notizen, von Friederich Hessenberg.* Neue Folge, Neuntes Heft. 26 pp. 4to, with three plates. Frankfurt a. M., 1875. (Christian Winter).—This number of the Mineralogical Notices of the excellent crystallographer, F. Hessenberg, recently deceased, was edited by his friend, Dr. F. Scharff. It contains descriptions of crystals of Xenotime from Tavetsch; of Binnite from Imfeld in the Binnenthal, and of Calcite from Rödefiörd in Iceland and Andreasberg, and is illustrated by most excellent figures on the three plates.

This valuable memoir, like the preceding of the two series, is published in the *Abhandlungen der Senkenbergischen Naturforschenden Gesellschaft in Frankfurt a. M.*

16. *Tables for the Determination and Classification of Minerals found in the United States;* by James C. Foye, A.M., Prof. Chem. Phys. in the Lawrence University. 38 pp. 12mo. Chicago, 1875. (Jansen, McClurg & Co.)—The tables contained in this little work are arranged with reference both to the blowpipe and physical characters of minerals, and will be found useful for the purposes intended.

17. *Observations on the Phenomena of Plant life: a Paper presented to the Massachusetts Board of Agriculture;* by W. S. Clark, Pres. State Agric. College, Amherst, Mass. 112 pp. 8vo. Boston, 1875. From the 22nd Ann. Rep. of the Secretary of the Mass. State Board of Agriculture.—The author presents in this paper the results of his new experiments on the phenomena of plant life, in continuation of those in a former Report, noticed in vol. vii of this Journal on page 512, and sustaining the same conclusions.

18. *Forest Flora of N. W. and Central India, a Handbook of the Indigenous Trees and Shrubs of those Countries.* Commenced by the late J. Lindsay Stewart, M.D.; continued and completed by Dietrich Brandis, Ph.D., Inspector General of Forests to the Government of India. London: Allen & Co., 1874. 8vo, and Atlas, 4to. Published under the authority of the Secretary of State for India in Council. Prepared at the Herbarium of the Royal Gardens, Kew.—"The object of this work is entirely practical," and an excellent practical work it is. It was written "especially for those who have the care of the public forests in the different provinces of India," has been preceded by the Flora Sylvatica of Madras, by Colonel Beddome, and is to be followed by the Forest Flora of British Burma, now preparing by Mr. Kurz. There is a good quarto atlas of 70 plates, drawn and lithographed by Fitch. A. G.

19. *Flora Brasiliensis*, Fasc. 66, issued in February last, contains the *Aristolochiaceæ*, elaborated by Dr. Masters: comprises the unique *Holostylis*, and 48 species of *Aristolochia*. Among the figures, one is given of the striking *A. Brasiliensis*, now rather common in conservatories. It appears that one of the kinds of *Guaco*, famous antidotes to snake-bites, is furnished by an *Aristolochia*, viz., *A. maxima* of Guiana and Venezuela. Fasc. 67, published in March last, comprises three or four orders, all small, except the *Vochysiaceæ* (by Warming), which is especially Brazilian. The *Callitrichiaceæ* are treated with much fullness as to general characters, by Hegelmaier. The affinity is still regarded as uncertain; the recent reclamation of *Callitrichiæ* to the *Halorageæ* is not responded to; still it is thought that the rarest known genus is *Hipprius*, but that its relationship is uncertain. It appears that only one species is known in Brazil, *C. deflexa*, of which a variety, *Austini*, is *C. Austini* of Engelmann. As *C. deflexa* and *C. Austini* were published in the same year (1867), we may, perhaps, quite properly retain Engelmann's name.

The *Onagraceæ* are by M. Micheli of Geneva. The most important genus is *Jussiæa*, with 36 species, under three sections. Our northern *J. decurrens* is one of the species, and is well figured. So is the *J. oocarpa* of C. Wright in Grisebach's *Pl. Cubenses*, on which a genius, *Oocarpon* is here established. Of *Epilobium* there is only *E. tetragonum*; of *Fuschia* and *Œnothera* three or four species each. A. G.

20. *Different effects of the same temperature upon the same species of plants under different latitudes.* A communication made by M. Alph. DeCandolle to the Academy of Sciences, Paris, reported in Comptes Rendus, June 7, 1875.—As the temperature rises in spring, it is thought that vegetation is more powerfully or promptly acted upon in higher than in lower latitudes. To test this, M. DeCandolle formerly had the seeds of three or four common annuals sent him from northern and southern Europe, which he raised together at Geneva. One of the selected species, the common Groundsel, confirmed this opinion, the northern seeds showing a decided advantage in precocity; while the uncertain or varying result of the two other plants tried were attributed to the fact that being of somewhat variable species they probably represented unlike forms. It occurred to M. DeCandolle to test the matter in a different way, and upon trees. At some time last winter he had branches sent him from Montpellier of *Populus alba*, *Carpinus Betulus*, *Liriodendron*, and *Catalpa*. These were paired with similar branches taken from trees at Geneva; and, after a common sojourn in a cool room long enough to make sure of complete penetration by the same temperature, the pairs were placed in glasses of water, with some sand at bottom, and kept in a warmed room under exactly the same conditions. The *Catalpa* requiring a higher temperature to start it, and coming, therefore, much later into leaf, was made the subject of a subse-

quent experiment. That with the other three commenced on the 4th of February. The result was that the German trees leafed out first. In the case of the Poplar there was a difference of about 23 days in favor of the individual of the colder locality; in that of the *Carpinus* about 18 days; and in that of the Tulip-tree a similar result was obtained when the comparison was restricted to buds of the same size and degree of development. The *Catalpa* of the northern locality developed 20 days in advance of the other.

Why should the same temperature act more powerfully and promptly upon the plant of the higher latitude? DeCandolle refers it to two causes, attributing, however, most importance to the second. First, to a natural selection of the buds which has induced the boreal precocity. "The buds of a tree are in a continual struggle. The later, like the badly placed ones, develop imperfect branches, which are oftener stifled. The most precocious prevail, unless indeed they suffer from frost. In this way comes a selection and a successive adaptation of the tree to the climate." And he goes on to show that what effectuates this result is, that every peculiarity of a bud is ordinarily reproduced year after year in the succeeding growths. As a case in point he mentions a well known Horse-chestnut tree in the environs of Geneva, upon which the owner, in the year of 1822 or 1823, detected a single branch bearing double flowers; this still continues, and has all along borne double flowers, and shows no tendency to revert to the ordinary simple-flowered condition of the rest of the tree. Grafts have been taken from it, and it is thought to be the original of all the double-flowered Horse-chestnuts in the world. Although this may well illustrate how the precocity may come to pass; yet DeCandolle doubts whether this selection of branches produces much effect. Because, in the north precocity seems as likely to be a disadvantage as an advantage, while at the south it ought generally to be an advantage, yet the southern individual requires more heat for vegetation. And it is yet to be proved that an individual tree becomes any better adapted to the climate as it increases in years. The general idea is that a tree does not acclimatize. The principal cause of this difference in the vegetation of northern and southern individuals, in DeCandolle's opinion, is the complete hibernal repose of the former, rendering it somehow more susceptible to the heat of spring; though in what way is not stated. We would add that if the suggested explanation through bud-selection be not the true one in the case of trees, owing to the general fixity of character in buds giving small room for variation, yet the explanation may be reached on this line by taking the fructification into account. DeCandolle alludes, at the beginning of his paper, to the case of the precocious cereal grains of the north, only to remark that some naturalists are unwilling to argue from cultivated to spontaneous plants. But it seems to us that natural selection would operate similarly upon trees, inducing precocious races better adapted to the short summer, the only difference being that much more time would be required for a tree to fix a race than for an annual. An objection to this view offers itself, however, in the fact that the precocious vernal development was apparent, although less marked, in *Liriodendron* and *Catalpa*, of which very few generations can have been raised in Europe. A. G.

21. DR. JOHN EDWARD GRAY.—A "List of Books, Memoirs and Miscellaneous Papers," commenced in 1871 and added to up to the close of 1873, is Dr. Gray's own record of his publications. Mr. J. Sanders has appended those of still later date, and printed it in an 8vo pamphlet of 58 pages. The total number of papers is 1162! Some curious and characteristic notes are interspersed.

22. *North American Oniscida;* A. STUXBERG has a paper on the North American Oniscida in the Ofversigt K. V.-Ak. Förhandlingar, 1875, No. 2, Stockholm, reviewing the described species, and adding descriptions of several new species.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *American Association for the Advancement of Science.*—The twenty-fourth annual meeting of the Association was opened at Detroit, on Wednesday, the 11th of August, under the presidency of J. E. Hilgard, Esq., of the Coast Survey. An address of welcome to the city was delivered at the opening session, by Hon. C. J. Walker. The address of the retiring president, Dr. John L. LeConte, was delivered on Thursday evening. It will be published in the following number of this Journal, in which also some account of the papers read at the meeting will be given.

2. *Cincinnati Society of Natural History.*—This Society has received a bequest of $50,000 from Mr. Bodman, who had been one of its members.—*Cincinnati Qu. J. Sci.*, ii, 286, July.

3. *Preliminary Report upon a Reconnaissance through Southern and Southeastern Nevada*, made in 1869 by First Lieut. G. M. WHEELER, U. S. A., and First Lieut. D. W. LOCKWOOD, U. S. A., under the orders of Brig. Gen. E. O. C. Ord, commanding Department of California, Engineer Department, U. S. Army. 72 pp. 4to, Washington, 1875.—This report contains a general description of the country passed through; notes on the Indians; brief accounts of the mines and mining settlements; results of astronomical observations for determining longitudes and latitudes, etc.

In latitude 38° 58′, on "Wheeler's" Peak, the upper limit of vegetation was found to be at 11,500 feet above the sea-level.

4. *The Mosaic Account of Creation, The Miracle of To-day;* by CHARLES B. WARRING. 292 pp. 12mo. New York, 1875. (J. W. Schermerhorn & Co.)—The author, who regards his method of interpretation very literal, makes the third day of Genesis, the day of the appearance of dry land and the creation of vegetation, to include all of geological history up to the Glacial period of the Quaternary. A change in the obliquity of the ecliptic is made the event of the fourth day, and the source of the Glacial cold. Then, with regard to the fiat on the fifth day, "Let the waters bring forth," and that of the sixth, "Let the earth bring forth," he says: "The waters, during the melting glaciers, were compar-

atively soon ready for animal life, but the land required a longer time for preparation."

6. *Mineral Collection of Dr. Krantz.*—This extensive collection of minerals has been purchased for the University of Bonn, and is now under the charge of Professor vom Rath.

G. P. DESHAYES, the distinguished paleontologist of Paris, died on the 9th of June last, in his 79th year, at his residence in Boran (Oise).

Proceedings of the Semi-centennial Celebration of the Rensselaer Polytechnic Institute, Troy, N. Y., held June 14–18, 1874. 8 pp. 8vo. Troy, 1875. The Rensselaer School is the oldest Scientific School in the country, dating from 1824.

Papers read before the Pi Eta Society, 1875. Rensselaer Polytechnic Institute, Troy, N. Y. 74 pp. 12mo. Troy, 1875. Printed by the Society. Among the papers included, one, by Prof. H. A. Rowland, treats of sympathetic vibration, and another, by Wm. H. Burr, of "Some of the stresses in the members of the Swing-bridge."

Field and Forest. Devoted to General Natural History. Bulletin of the Potomac-side Naturalists' Club. Charles R. Dodge, Editor. No. 1, vol. i, 8 pp. 8vo. Washington, 1875. Contains description of "a new Grasshopper from Arizona," "*Eremobia magna*," by Cyrus Thomas, and some botanical notes, with the Proceedings of meetings of the Club.

Bulletin of the Bussy Institution (Jamaica Plains, Boston.) Harvard University. Part IV, 1875. Contains the results of much study and investigation of questions connected with the department of Agriculture.

Report of the Commission of Engineers appointed to investigate and report a permanent plan for the reclamation of the alluvial basin of the Mississippi River subject to inundation. 15 pp. 8vo, with maps. Washington, 1875. Contains much valuable matter on the levees, floods and alluvial basin of the Mississippi.

History of the Birds of Europe, including all the species inhabiting the Western Palæarctic Region; by H. E. Dresser, F.Z.S. Parts 35 and 36, completing the 3d volume. Office of the Zoological Society, London.

A Report on the Hygiene of the U. S. Army, with descriptions of Military Posts. 568 pp. 8vo. Washington, 1875. Circular No. 8.: War Department, Surgeon General's Office, Washington, May 1, 1875.

Addition to the Paper on "Volcanic Energy: an attempt to develop its true Origin and Cosmical Relations." By ROBERT MALLET, F.R.S. 9 pp. 4to. Read before the Roy. Soc., May 7, 1874. Phil. Trans., clxv, pt. 1.

On the temperature attainable by rock-crushing and its consequences; by ROBERT MALLET, F.R.S. 13 pp. 8vo. Phil. Mag. for July, 1875.

Supplement to the Extinct Batrachia and Reptilia of N. America; by E. D. Cope. pp. 261–972 of vol. xv of the Trans. Amer. Phil. Soc. Philadelphia. 1875.

Mineral Deposits in Essex Co., Mass., especially in Newbury and Newburyport, with a map and notes; by Charles J. Brockway. 60 pp. 12mo. Newburyport. 1875.

Reports on the Meteorological, Magnetic and other Observatories of the Dominion of Canada, for 1874; being the Annual Reports of the Meteorological Office. 318 pp. 8vo. Ottawa. 1875.

The Gold Fields of Yesso. Geological Survey of Hokkaido. Report by H. S. Munroe. 80 pp. 8vo. Tokio, Japan. 1875.

Notes on the Geology of West Virginia, No. II; by John J. Stevenson, Prof. Geol. Univ. New York. Proc. Amer. Phil. Soc. for Feb., 1875.

The Geological Relations of the Lignitic Groups; by John J. Stevenson. Proc. Amer. Phil. Soc. for June, 1875. Decides in favor of the Cretaceous age of the Lignitic beds, but without bringing forward any more decisive facts than were before published.

Abstracts and Results of Magnetical and Meteorological Observations at the Magnetic Observatory, Toronto, Canada, from 1841 to 1871 inclusive. 60, 26, 56 and 108 pages.

THE

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[THIRD SERIES.]

ART. XXXIII.—*Address of Dr. John L. LeConte, the retiring President of the American Association for the Advancement of Science, at the meeting in August*, 1875, *at Detroit.*

THE founders of science in America, and the other great students of nature, who have in previous years occupied the elevated position in which I now stand, have addressed you upon many momentous subjects. In fulfilling the final duty assigned to your presidents by the laws of the Association, some have spoken to you in solemn and wise words concerning the duties and privileges of men of science, and the converse duties of the nation toward those earnest and disinterested promoters of knowledge. Others again have given you the history of the developement of their respective branches of study, and their present condition, and have, in eloquent diction, commended to your gratitude those who have established on a firm foundation the basis of our modern systems of investigation.

The recent changes in our constitution, by which you are led to expect from your two vice-presidents, and from the chairman of the Chemical Sub-section, addresses on the progress made during the past year, restrain me from invading their peculiar fields of labor, by alluding to scientific work which has been accomplished since our last meeting. While delicacy forbids me from so doing, I am equally debarred from repeating to you the brief sketch I endeavored to give at a former meeting * of the history, and present condition of Entomology in the United States.

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* Proceedings Am. Assoc. Adv. Sci., xxi, Portland.

But it has appeared to me that a few thoughts, which have impressed themselves upon my mind, touching the future results to be obtained from certain classes of facts not yet fully developed on account of the great labor required for their proper comparison, may not be without value. Even if the facts be not new to you, I hope to be able, with your kind attention, to present them in such way as to be suggestive of the work yet to be done.

It has been perhaps said, or at least it has been often thought, that the first mention of the doctrine of evolution, as now admitted to a greater or less degree by every thinking man, is found in Ecclesiastes, i, 9: "The thing that hath been is that which shall be; and that which is done is that which shall be done; and there is no new thing under the sun. Is there anything whereof it may be said, see, this is new? It hath been already of old time, which was before us."

Other references to evolutionary views in one form or another occur in the writings of several philosophers of classic times, as you have had recent cause to remember.

Whether these are to be considered as an expression of a perfect truth in the very imperfect language which was alone intelligible to the nation to whom this sacred book was immediately addressed on the one hand; and the happy guesses of philosophers, who by deep intuition had placed themselves in close sympathy with the material universe on the other hand, I shall not stop to inquire. The discussion would be profitless, for modern science in no way depends for its magnificent triumphs of fact and thought upon any utterances of the ancients. It is the creation of patient, intelligent labor of the last two centuries, and its results can be neither confuted nor confirmed by anything that was said, thought or done, at an earlier period. I have merely referred to these indications of doctrines of evolution to recall to your minds that the two great schools of thought, which now divide philosophers, have existed from very remote times. They are, therefore, in their origin, probably independent of correct scientific knowledge.

You have learned from the geologists, and mostly from those of the present century, that the strata of the earth have been successively formed from fragments more or less comminuted by mechanical action, more or less altered by chemical combination and molecular rearrangements. These fragments were derived from strata previously deposited, or from material brought up from below, or even thrown down from above, or from the debris of organic beings which extracted their mineral constituents from surrounding media. Nothing new has been added, everything is old; only the arrangement of the parts is new, but in this arrangement definite and recognizable unchanged fragments of the old frequently remain. Geological observation is now so extended and accurate that an experienced student can tell from what formation, and even from what particular locality, these fragments have been derived.

I wish to show that this same process has taken place in the organic world, and that by proper methods we can discover in our fauna and flora the remnants of the inhabitants of former geologic times, which remain unchanged, and have escaped those influences of variation which are supposed to account for the differences in organic beings of different periods.

Should I succeed in this effort we will be hereafter enabled in groups of animals which are rarely preserved in fossil condition to reconstruct, in some measure, the otherwise extinct faunæ, and thus to have a better idea of the sequence of generic forms in time. We will also have confirmatory evidence of certain changes which have taken place in the outline of the land and the sea. More important still, we will have some indications of the time when greater changes have occurred, the rock evidence of which is now buried at the bottom of the ocean, or perhaps entirely distroyed by erosion and separation. Of these changes, which involved connections of masses of lands, no surmise could be made except through evidence to be gained in the manner of which I am about to speak.

My illustrations will naturally be drawn from that branch of zoology with which I am most familiar; and it is indeed to your too partial estimate of my studies in that science that I owe the privilege of addressing you on the present occasion.

There are, as you know, a particular set of Coleoptera which affect the seashore; they are not very numerous at any locality, but among them are genera which are represented in almost every country of the globe. Such genera are called cosmopolitan, in distinction to those which are found only in particular districts. Several of these genera contain species which are very nearly allied, or sometimes, in fact, undistinguishable and therefore identical, along extended lines of coasts.

Now it happens that some of these species, though they never stray from the ocean shore inland, are capable of living upon similar beaches on fresh water lakes, and a few are found in localities which are now quite inland.

To take an example, or rather several examples together, for the force of the illustration will be thereby greatly increased.

Along the whole of the Atlantic, and the greater part of the Pacific coast of the United States, is found in great abundance on sand beaches, a species of Tiger-beetle, *Cicindela hirticollis*, an active, winged, and highly predaceous insect; the same species occurs on the sand beaches of the great lakes, and were it confined to these and similar localities, we would be justified in considering it as living there in consequence solely of the resemblance in the conditions of its existence. But it is also found, though in much less abundance, in the now elevated region midway between the Mississippi and Rocky Mountains. Now, this is the part of the continent which, after the division of the great intercontinental gulf in Cretaceous times, finally emerged from the bed of the sea, and was in the early and middle Tertiary converted into a series of fresh water lakes. As this insect does not occur in the territory extending from the Atlantic to beyond the western boundary of Missouri, nor in the interior of Oregon and California, I think that we should infer that it is an unchanged survivor of the species which lived on the shores of the Cretaceous ocean, when the intercontinental gulf was still open, and a passage existed, moreover, toward the southwest, which connected with the Pacific.

The example I have given you of the geographical distribution of *Cicindela hirticollis* would be of small value were it an isolated case; nor would I have thought it worthy of occupying your time on an occasion like this, which is justly regarded as one for the communication of important truths. This insect, which I have selected as a type for illustrating the methods of investigation to which I invite your attention, is, however, accompanied more or less closely by other Coleoptera, which like itself, are not particular as to the nature of their food so long as it be other living insects, and apparently are equally indifferent to the presence of large bodies of salt water. First, there is *Cicindela lepida*, first collected by my father near Trenton, N. J., afterward found on Coney Island, near New York, and received by me from Kansas and Wisconsin; not, however, found west of the Rocky Mountains. This species, thus occurring in isolated and distant localities, is probably in progress of extinction, and may or may not be older than *Cicindela hirticollis.* I am disposed to believe, as no representative species occurs on the Pacific coast, and from its peculiar distribution, that it is older. Second, there is *Dyschirius pallipennis*, a small Carabide, remarkable among the other species of the genus by the pale wing covers, usually ornamented with a dark spot. This insect is abundant on the Atlantic coast from New York to Virginia, unchanged in the interior parts of the Mississippi Valley, represented at Atlantic City, New Jersey, by a large and quite distinct specific form, *D. sellatus*, and on the Pacific coast by two or three species of larger size and different shape, which, in my less experienced youth, I was disposed to regard as a separate genus, *Akephorus* This form is, therefore, in a condition of evolution – how, I know not—our descendants may. The Atlantic species are winged, the Pacific one, like a large number of insects of that region, are without wings.

Accompanying these are Coleoptera of other families, which have been less carefully studied, but I will not trespass upon your patience by mentioning more than two. *Bledius pallipennis* (*Staphylinidæ*) is found on salt marshes near New York, on the Southern sea-coast and in Kansas—*Ammodonus fossor*, a wingless Tenebrionide, Trenton, sea-shore near New York, and Valley of the Mississippi at St. Louis; thus nearly approximating *Cicindela lepida* in distribution.

We can thus obtain by a careful observation of the localities of insects, especially such as affect sea-shore or marsh, and those which being deprived of their favorite surroundings have shown, if I may so express myself, a patriotic clinging to their native soil, most valuable indications in regard to the time at which their unmodified ancestors first appeared upon the earth. For it is obvious that no tendency to change in different directions by "numerous successive slight modifications"* would produce a uniform result in such distant localities, and under such varied conditions of life. Properly studied, these indications are quite as certain as though we found the well preserved remains of these ancestors in the mud and sand strata upon which they flitted or dug in quest of food.

Other illustrations of survivals from indefinitely more remote times I will give you, from the Coleopterous fauna of our own country, though passing time admonishes me to restrict their number.

To make my remarks intelligible, I must begin by saying that there are three great divisions of Coleoptera, which I will name in the order of their complication of structural plan: 1. Rhynchophora; 2. Heteromera; 3. Ordinary or normal Coleoptera; the last two being more nearly allied to each other than either is to the first. I have in other places exposed the characters of these divisions, and will not detain you by repeating them.

From paleontological evidence derived from other branches of zoology we have a right to suppose, if this classification be correct, that these great types have been introduced upon the earth in the order in which I have named them.

Now, it is precisely in the first and second series that the most anomalous instances of geographical distribution occur; that is to say, the same or nearly identical genera are represented by species in very widely separated regions, without occurring in intermediate or contiguous regions. Thus there is a genus *Emeax*, founded by Mr. Pascoe, upon an Australian species, which, when I saw it, I recognized as belonging to

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* Origin of Species, 1869, 227.

Nyctoporis, a California genus, established many years before, and in fact barely specifically distinct from *N. galeata*. Two other examples are *Othnius* and *Eupleurida*, United States genera, which are respectively equivalent to *Elacatis* and *Ischalia*, found in Borneo. Our native genera, *Eurygenius* and *Toposcopus*, are represented by scarcely different forms in Australia. All these belong to the second series (*Heteromera*), and the number of examples might be greatly increased with less labor on my part than patience on yours.

A single example from the Rhynocophora, and I will pass to another subject.

On the sea-coast of California, extending to Alaska, is a very anomalous insect whose affinities are difficult to discern, called *Emphyastes fucicola*, from its occurrence under the sea-weed cast up by the waves. It is represented in Australia by several species of a nearly allied genus *Aphela*, found in similar situations.

In all entomological investigations relating to geographical distribution we are greatly embarrassed by the multitude of species, and by the vague and opinionative genera founded upon characters of small importance. The Coleoptera alone, thus far described, amount to over 60,000 so-called species, and there are from 80,000 to 100,000 in collections. Under these circumstances it is quite impossible for one person to command either the time or the material to master the whole subject, and, from the laudable zeal of collectors to make known what they suppose to be new objects, an immense amount of synonymy must result. Thus, in the great Catalogus Coleopterorum of Gemminger and Harold, a permanent record of the untiring industry of those two excellent entomologists, species of the genus *Trechicus*, founded by me upon a small North American insect, are mentioned under five generic names, only one of which is recognized as a synonym of another. These generic headings appear in so remote pages of the volume as 135, 146 and 289.

The two closely allied genera of Rhynchophora mentioned above are separated by no less than 168 pages. It is, therefore, plain that, before much progress can be made in the line of research which I have proposed to you, whereby we may recover important fragments of the past history of the earth, entomology must be studied in a somewhat different manner from that now adopted. The necessity is everyday more apparent that descriptions of heterogeneous material are rather obstructive than beneficial to science, except in the case of extraordinary forms likely to give information concerning geographical distribution or classification. Large typical collections, affording abundant material for comparison, for the approximation of allied forms, and the elimination of doubtful ones, must be accumulated; and in the case of such perishable objects as those we are now dealing with, must be placed where they can have the protecting influences both of climate and personal care.

At the same time, for this investigation the study of insects is peculiarly suitable: not only on account of the small size, ease of collecting, and little cost of preserving, the specimens, but because, from their varied mode of life in different stages of development, and perhaps for other reasons, the species are less likely to be destroyed in the progress of geological changes. (For a fuller discussion of these causes and of several other subjects which are briefly mentioned in this address, the reader may consult an excellent memoir by my learned friend, Mr. Andrew Murray, "On the Geographical Relations of the Chief Coleopterous Faunæ," Journal of Linnæan Society, Zoology, vol. xi.) Cataclysms and submergences, which would annihilate the higher animals, would only float the temporarily asphyxiated insect, or the tree trunks containing the larvæ and pupæ to other neighboring lands. However that may be, I have given you some grounds for believing that many of the species of insects now living existed in the same form before the appearance of any living genera of mammals, and we may suppose that their unchanged descendants will probably survive the present mammalian fauna, including our own race.

I may add, moreover, that some groups, especially in the Rhynchophora, which, as I have said above, I believe to be the earliest introduced of the Coleoptera, exhibit with compact and definite limits, and clearly defined specific characters, so many generic modifications that I am compelled to think that we have in them an example of long sought unbroken series extending, in this instance, from early Mesozoic to the present time, and of which very few forms have become extinct.

I have used the word species so often that you will doubtless be inclined to ask, what, then, is understood by a species? Alas! I can tell you no more than has been told recently by many others. It is an assemblage of individuals, which differ from each other by very small or trifling and inconstant characters, of much less value than those in which they differ from any other assemblage of individuals. Who determines the value of these characters? The experienced student of that department to which the objects belong. Species are, therefore, those groups of individuals representing organic forms which are recognized as such by those who from natural power and education are best qualified to judge.

You perceive, therefore, that we are here dealing with an entirely different kind of information from that which we gain from the physical sciences; everything there depends on accurate observation, with strict logical consequences derived therefrom. Here the basis of our knowledge depends equally on accurate and trained observation, but the logic is not formal but perceptive.

This has been already thoroughly recognized by Huxley* and Helmholtz,† and others, but we may properly extend the inquiry into the nature and powers of this æsthetic perception somewhat further. For it is to this fundamental difference between biological and physical sciences that I will especially invite your attention.

Sir John Lubbock,‡ quoting from Oldfield,§ mentions that certain Australians "were quite unable to realize the most vivid artistic representations. On being shown a picture of one of themselves, one said it was a ship, another a kangaroo, not one in a dozen identifying the portrait as having any connection with himself.

These human beings, therefore, with brains very similar to our own, and, as is held by some persons, potentially capable of similar cultivation with ourselves, were unable to recognize the outlines of even such familiar objects as the features of their own race. Was there any fault in the drawing of the artist? Probably not. Or in the eye of the savage? Certainly not, for that is an optical instrument of tolerably simple structure, which cannot fail to form on the retina an accurate image of the object to which it is directed. Where then is the error? It is the want of capacity of the brain of the individual (or rather of the race in this instance) to appreciate the resemblance between the outline, the relief, the light and shade of the object pictured, and the flat representation in color: in other words, a want of "artistic tact" or æsthetic perception.

A higher example of a similar phenomenon I have myself seen; many of you, too, have witnessed it, for it is of daily occurrence. It is when travelers in Italy, having penetrated to the temple of Art, even the hall of the Tribune at Florence, stand in presence of the most perfect works of art which it has been given to man to produce, and gaze upon them with the same difference that they would show to the conceptions of mediocre artists exhibited in our shops. Perhaps they would even wonder what one can find to admire in the unrivalled collection which is there assembled. There is surely wanting in the minds of such persons that high, æsthetic sense, which enables others to enter into spiritual harmony with the great artists whose creations are before them. Creations I said, and I use the word intentionally. If there is one power of the human soul, which more nearly than any other approaches the faculty of creation, it is that which the almost inspired artist develops out of a rude block of stone, or out of such mean materials as canvas and metallic pastes of various colors, figures which surpass in beauty and in power of exciting emotion the objects they profess to represent.

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* "A species is the smallest group to which distinctive and invariable characters can be assigned."—*Principles and Methods of Palæontology, Smithsonian Report*, 1869, 378.

† "I do not mean to deny that in many branches of these sciences, an intuitive perception of analogies and a certain artistic tact play a conspicuous part. In natural history * * * it is left entirely to this tact, without a clearly definable rule, to determine what characteristics of species are important or unimportant for purposes of classification, and what divisions of the animal or vegetable kingdom are more natural than others."—*Relation of the Physical Sciences to Science in General; Smiths. Report*, 1871, 277.

‡ Prehistoric Times, p. 440.

§ On the Aborigines of Australia, Trans. Ethnological Soc., New series, vol. iii.

Yet these unæsthetic and nonappreciative persons are just as highly educated, and in their respective positions as good and useful members of the social organism, as any that may be found. I maintain only, they would never make good students of biology.

In like manner, by way of illustrating the foregoing observations, there are some, who, in looking at the phenomena of the external universe, may recognize only chance, or the "fortuitous concourse of atoms," producing certain resultant motions. Others, having studied more deeply the nature of things, will perceive the existence of laws, binding and correlating the events they observe. Others again, not superior to the latter in intelligence, nor in power of investigation, may discern a deeper relation between these phenomena, and the indications of an intellectual or æsthetic or moral plan, similar to that which influences their own actions, when directed to the attaining of a particular result.

These last will recognize in the operations of nature the direction of a human intelligence, greatly enlarged, capable of modifying at its will influences beyond our control; or they will appreciate in themselves a resemblance to a superhuman intelligence which enables them to be in sympathy with its actions. Either may be true in individual instances of this class of minds; one or the other must be true; I care not which, for to me the propositions are in this argument identical, though in speculative discussions they may be regarded as at almost the opposite poles of religious belief. All that I plead for is this, that those who have not this perceptive power, and who in the present condition of scientific discussion are numerically influential, will have tolerance for those who possess it; and that the idea of the latter may not be entirely relegated to the domain of superstition and enthusiasm.

In the case of want of perception of the Australian, a very simple test can be applied. It is only to photograph the object represented by the artist, and compare the outlines and shades of the photograph with those of the picture. If they accord within reasonable limits, the picture is correct to that extent; at least, however bad the artist, the human face could never be confounded with a ship or a kangaroo.

Can we apply a similar test to the works of nature? I think we can. Suppose that man—I purposely use the singular noun to indicate that all human beings of similar intelligence and education working toward a definite end, will work in a somewhat similar manner—suppose, then, I say, that man, endeavoring to carry out some object of importance, devises a method of so doing, and creates for that purpose a series of small objects, and we find that these small objects naturally divide and distribute themselves in age and locality, in a similar manner to that in which the species of a group of organisms are divided in space, and distributed in time; and that the results of man's labor are thus divided and distributed on account of the necessary inherent qualities of his intelligence and methods of action, is not the resemblance between human reason and the greater powers which control the manifestations of organic nature apparent?

I now simply present to you this investigation. Time is wanting for me to illustrate it by even a single example, but I feel sure that I have in the minds of some of you already suggested several applications of it to the principle I wish to teach—the resemblance in the distribution of the works of nature to that of human contrivances evolved for definite purposes.

If this kind of reasoning commends itself to you, and you thus perceive resemblances in the actions of the Ruler of the Universe to those of our own race, when prompted by the best and highest intellectual motives, you will be willing to accept the declaration of the ancient text, "He doeth not evil, and abideth not with the evil inclined. Whatever he hath done is good";* or that from our own canon of Scripture, "With him is wisdom and strength, he hath counsel and understanding.†

The æsthetic character of natural history, therefore, prevents the results of its cultivation from being worked out with the precision of a logical machine, such as with correct data of observation and calculation would be quite sufficient to formulate the conclusions of physical investigation. According as the perception of the relations of organic beings among themselves becomes more and more enlarged, the interpretation of these relations will vary within limits; but we will be continually approximating higher mental or spiritual truth.

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* Desatir, p. 2.

† Job, xii, 13.

This kind of truth can never be revealed to us by the study of inorganic aggregations of the universe. The molar, molecular and polar forces, by which they are formed, may be expressed, so far as science has reduced them to order, by a small number of simply formulated laws, indicative neither of purpose nor intelligence, when confined within inorganic limits. In fact, taking also the organic world into consideration, we as yet see no reason why the number of chemical elements known to us should be so large as it is, and go on increasing almost yearly with more minute investigation. To all appearance the mechanical and vital structure of the universe would remain unchanged if half of them were struck out of existence.

Neither is there any evidence of intelligence or design in the fact that the side of the moon visible to us exhibits only a mass of volcanoes. Yet upon the earth, without the volcano and the earthquake and the elevating force of which they are the feeble indications, there would be no permanent separation of land and water; consequently no progress in animal and vegetable life beyond what is possible in the ocean. To us, then, as sentient beings, the volcano and the earthquake, viewed from a biological standpoint, have a profound significance.

It is indeed difficult to see in what manner the student of purely physical science is brought to a knowledge of any evidences of intelligence in the arrangement of the universe. The poet, inspired by meditating on the immeasurable abyss of space, and the transcendent glories of the celestial orbs, has declared,

> "The undevout astronomer is mad."

And his saying had a certain amount of speciousness, on account of the magnitude of the bodies and distances with which the student of the stars is concerned. This favorite line is, however, only an example of what an excellent writer has termed "the unconscious action of volition upon credence," and it is properly in the correlations of the inorganic world that we may hope to exhibit, with clearness, the adaptations of plan prefigured and design executed.

In the methods and results of investigation, the mathematician differs from both the physicist and the biologist. Unconfined, like the former, by the few simple relations by which movements in the inorganic world are controlled, he may not only vary the form of his analysis, almost at pleasure, making it more or less transcendental in many directions, but he may introduce factors or relations, apparently inconceivable in real existence, and then interpret them into results quite as real as those of the legitimate calculus with which he is working, but lying outside of its domain.

If biology can ever be developed in such manner that its results may be expressed in mathematical formulæ, it will be the pleasing task of the future analyst to ascertain the nature of the inconceivable (or imaginary as they are termed in mathematics) quantities which must be introduced when changes of form or structure take place. Such will be analytical morphology, in its proper sense; but it is a science of the future, and will require for its calculus a very complex algebra.

In the observation of the habits of interior animals we recognize many complications of action, which, though directed to the accomplishment of definite purposes, we do not entirely comprehend. They are, in many instances, not the result of either the experience of the individual, or the education of its parents, who, in low forms of animals, frequently die before the hatching of the offspring. These actions have been grouped together, whether simple or complex, as directed by what we are pleased to call instinct, as opposed to reason. Yet there is every gradation between the two.

Among the various races of dogs, the companions of man for unnumbered centuries, we observe not only reasoning powers of a rather high order, but also distinct traces of moral sentiments similar to those possessed by our own race. I will give no examples, for many may be found in books with which you are familiar. Actions evincing the same mental attributes are also noticed in wild animals which have been tamed. You will reply that these qualities have been developed by human education; but not so, there must have been a latent capacity in the brain to receive the education and to manifest the results by the modification of the habits. Now it is because we are vertebrates, and the animals of which I have spoken are vertebrates, that we understand, though imperfectly, their mental processes, and can develop the powers that are otherwise latent. Could we comprehend them more fully we would find, and we do find from time to time in the progress of our inquiries, that what was classed with instinct is really intellection.

When we attempt to observe animals belonging to another sub-kingdom, Articulata, for instance, such cases as bees, ants, termites, etc., which are built upon a totally different plan of structure, having no organ in common with ourselves, the difficulty of interpreting their intellectual processes, if they perform any, is still greater. The purposes of their actions we can divine only by their results. But anything more exact than their knowledge of the objects within their scope, more ingenious than their methods of using those objects, more complex yet well devised than their social and political systems, it is impossible to conceive.

We are not warranted in assuming that these actions are instinctive, which, if performed by a vertebrate, we would call rational. Instead of concealing our ignorance under a word which thus used comes to mean nothing, let us rather admit the existence here of a rational power, not only inferior to ours, but also different.

Thus, proceeding from the highest forms in each type of animal life to the lower, and even down to the lowest, we may be prepared to advance the thesis that all animals are intelligent in proportion to the ability of their organization to manifest intelligence to us, or to each other; that wherever there is voluntary motion there is intelligence, obscure, it may be, not comprehended by us, but comprehended by the companions of the same low grade of structure.

However this may be, I do not intend to discuss the subject at present, but only wish, in connection with this train of thought, to offer two suggestions.

The first is that, by pursuing different courses of investigation in biology, we may be led to opposite results. Commencing with the simplest forms of animal life, or with the embryo of the higher animals, it may be very difficult to say at what point intelligence begins to manifest itself; our attention is concentrated, therefore, upon those functions which appear to be the result of purely mechanical arrangements, acted upon by external stimuli. The animal becomes to our perception an automaton, and, in fact, by exercising some of the nervous organs last developed in its growth, we can render an adult animal an automaton, capable of performing only those habitual actions to which its brain, when in perfect condition, had educated the muscles of voluntary motion. On the other hand, commencing with the highest group in each type, and going downward, either in structural complication, or in age of individual, it is impossible to fix the limit at which intelligence ceases to be apparent.

I have in this subject, as in that of tracing the past history of our insects, in the first part of this address, preferred the latter mode of investigation: taking those things which are nearest to us in time or structure, as a basis for the study of those more remote.

The second consideration is, since it is so difficult for us to understand the mental processes, whether rational or instinctive (I care not by what name they are called) of beings more or less similar, but inferior to ourselves, we should exercise great caution when we have occasion to speak of the designs of One who is infinitely greater. Let us give no place to the crude speculations of would-be teleologists, who are, indeed, in great part refuted already by the progress of science, which continually exhibits to us higher and more beautiful relations between the phenomenon of nature "than it hath entered into the mind of man to conceive." Let not our vanity lead us to believe that because God has deigned to guide our steps a few paces on the road to truth, we are justified in speaking as if He had taken us into intimate companionship, and informed us of all His counsels.

If I have exposed my views on these subjects to you in an acceptable manner, you will perceive that in minds capable of receiving such impressions biology can indicate the existence of a creative or directive power, possessing attributes, some of which resemble our own, and controlling operations which we may feebly comprehend. Thus far natural theology, and no farther.

What then is the strict relation of natural history or biology to that great mass of learning and influence which is commonly called theology; and to that smaller mass of belief and action which is called religion?

Some express the relation very briefly, by saying that science and religion are opposed to each other. Others again that they have nothing in common. These expressions are true of certain classes of minds; but the greater number of thinking and educated persons see that though the ultimate truths taught by each are of quite distinct nature, and can by no means come in conflict, inasmuch as they have no point in common, yet so far as these truths are embodied in human language and manipulated by human interests, they have a common dominion over the soul of man. According to the method of their government, they may then come into collision, even as the temporal and spiritual sovereigns of Japan occasionally did, before the recent changes in that country.

In answering the query above proposed, it will be necessary to separate the essential truths of religion from the accessories of tradition, usage, and most of all, organizations and interpretations, which have, in the lapse of time, gathered around the primitive or revealed truth.

With the latter the scientific man must deal exactly like other men; he must take it, or reject it, according to his spiritual gifts, but he must not, whatever be his personal views, discuss it or assail it as a man of science, for within his domain of investigation it does not belong.

With regard to the accessories of traditions, interpretations, etc., our answer may be clearer when we have briefly reviewed some recent events in what has been written about as the Conflict of Religion and Science. Some centuries ago, great theological disgust was produced by the announcement that the sun and not the earth was the center of the planetary system.

A few decades ago profound dissatisfaction was shown that the evidence of organic life on the planet was very ancient. Recently some annoyance has been exhibited because human remains have been found in situations where they ought not to have been, according to popularly received interpretation: and yet more recently much apprehension has been felt at the possible derivation of man from some inferior organism; an hypothesis framed simply because in the present condition of intellectual advancement no other can be suggested.

Yet all these facts, but the last, which still is an opinion, have been accepted, after more or less bitter controversy on both sides, and the fountain of spiritual truth remains unclouded and undiminished. New interpretations for the sacred texts, supposed to be in conflict with the scientific facts, have been sought and found without difficulty. These much feared facts have, moreover, given some of the strongest and most convincing illustrations to modern exhortation and religious instruction.

Thus, then, we see that the influence of science upon religion has been beneficial. Scholastic interpretations, founded upon imperfect knowledge, or no knowledge, but mere guess, have been replaced by sound criticisms of the texts, and their exegesis in accordance with the times and circumstances for which they were written.

It must be conceded by fair minded men of both sides that these controversies were carried on at times with a rudeness of expression and bitterness of feeling now abhorrent to our usages. The intellectual wars of those days partook of the brutality of physical war, and the horrors of the latter, as you know, have been ameliorated only within a very few years.

I fear that the unhappy spirit of contention still survives, and that there are yet a few who fight for victory rather than for truth. The deceptive spirit of Voltaire still buds forth occasionally; he, who, as you remember, disputed the organic nature of fossil shells, because in those days of schoolmen their occurrence on mountains would be used by others as a proof of a universal Noachian deluge. The power of such spirits is fortunately gone for any potent influence for evil, gone with the equally obstructive influence of the scholastics with whom they formerly contended.

Since then there is no occasion for strict science and pure religion to be in conflict, how shall the peace be kept between them? By toleration and patience. Toleration toward those who believe less than we do, in the hope that they, by cultivation or inheritance of æsthetic perception, will be prepared to accept something more than matter and energy in the universe, and to believe that vitality is not altogether undirected colloid chemistry.

Toleration also toward those who, on what we think misunderstood or insufficient evidence demand more than we are prepared to admit, in the hope that they will revise additional texts which seem to conflict, or may hereafter conflict, with facts deduced from actual study of nature, and thus prepare their minds for the reception of such truths as may be discovered, without embittered discussions.

Patience, too, must be counseled, for much delay will ensue before this desired result is arrived at; patience under attack, patience under misrepresentation, but never controversy.

Thus will be hastened the time when the glorious, all-sufficient spiritual light, which though given through another race, we have adopted as our own, shall shine with its pristine purity, freed from the incrustations with which it has been obscured by the vanity of partial knowledge and the temporary contrivances of human polity.

So, too, by freely extended scientific culture, may we hope that the infinitely thicker and grosser superstitions and corruptions will be removed, which greater age and more despotic governments have accumulated around the less brilliant though important religions of our Asiatic Aryan relatives. These accretions being destroyed, the principal difficulty to the reception by those nations of higher spiritual truths will be obviated, and the intelligent Hindoo or Persian will not be tardy in recognizing, in the pure life and elevated doctrine of the sincere Christian, an addition to and fuller expression of religious precepts with which he is familiar. In this manner alone may be realized the hope of the philosopher, the dream of the poet, and the expectation of the theologian—a universal science, and a universal religion, co-operating harmoniously for the perfection of man and the glory of his Creator.

ART. XXXIV.—*On the Temperature attainable by Rock-crushing, and its Consequences*;* by ROBERT MALLET, F.R.S.

IN developing the theory of volcanic heat and energy embraced in his paper "On the Nature and Origin of Volcanic Heat and Energy" (Phil. Trans., Part I, 1873), the main object of the author was to prove that the annual work of secular contraction in our globe, when transformed into heat, was more than adequate for the supply of volcanic activity existing upon our planet. While indicating generally the circumstances which must attend as results of the descent of the exterior shell

* From the Philosophical Magazine for July, 1875.

upon the more rapidly contracting nucleus, it was not necessary to his argument to follow into detail the mechanism of local dislocation and crushing due to such descent. Nor would the limits of his paper admit of his entering into much detail as to the circumstances attending subterranean dislocation and crushing of rocky matter, or pointing out how some of these must greatly tend to exalt the temperatures due to the transformation of the mechanical work locally done. It was necessary to a truthful examination of the question whether or not the annual supply of heat transformed from the work of secular contraction were sufficient to meet the demands of existing volcanic action, that he should not overrate the work so transformed; and accordingly, in determining by experiment a measure for the amount of that work, the author viewed the work of crushing of unconfined or unsupported masses alone as the source of heat, this method being that only which could afford perfectly trustworthy experimental results. He paid no regard to the additional work that must attend the collision and friction of already crushed masses in the further progress of their deformation and forced transport to points more or less distant from those at which the crushing had taken place. The work of crushing in free air was capable of rigid determination; the work of subsequent deformation and transportation can only admit of estimation upon assumed data, and these necessarily of a somewhat arbitrary character, seeing how little we know accurately of the nature and disposition of the rocky materials of our earth's crust, except at the most inconsiderable depths from its surface. Nor were any of the circumstances pointed out by which high temperatures are capable of being attained locally in rocky masses crushed beneath our surface and which we must assume as those actually occurring in nature. The writer's object here is to point out, 1st, that, taking the annual supply of heat from transformed work of contraction, by experiment in the way he has done, the result, though more than sufficient to sustain his theory, affords alone no complete measure of the highest temperature that may through its means be locally developed; 2ndly, to answer some doubts which have been raised as to whether the temperature to which subterranean rocky masses can become raised by the heat evolved in their crushing and transportation of particles can be sufficient to bring more or less of these at such foci of crushing and dislocation to the fusing-point of such materials, which the author in his original paper assumes to be 2000° Fahr.

Professor Hilgard, occupying the chair of geology in the University of Michigan, U. S., in an able paper published in the American Journal of Science, vol. vii, June, 1874,* has, in terms as clear as they are courteous, pointed out the *lacunæ* in the author's original paper in the following passage:—

"One point, however, must strike every reader of the original memoir, viz: the preëminence given by Mallet to the *crushing of solid* rock as the means of producing heat and fusion. One would naturally look to the results of his experiments on this subject for the proof of the efficiency of this agency. But we find that the maximum *temperature* resulting from the crushing to powder of the hardest rock is something over 217° Fahr. This, then, represents the maximum increment of temperature that can be rendered efficient toward the fusing of rocks by the crushing process under the most favorable circumstances, viz: upon the supposition that it takes place instantaneously, or under such circumstances that the heat cannot be conducted away, and, further, that the resistance of the rock has not been materially diminished by the downward increase of hypogeal temperature. At the most moderate depths at which volcanic phenomena can be supposed to originate the last-mentioned factor must exert a very considerable influence, reducing materially the available heat-increment. Hence the numerical results of Mallet's laborious experiments on rock-crushing, however interesting and useful as affording a definite measure of the thermal effects producible by this means, yet fail to carry conviction as to the efficacy of this particular *modus operandi* in reducing large masses of solid rock to fusion, unless essentially supplemented by friction, not so much of rock walls against each other, but more probably by the heat produced within more or less comminuted *detrital* or *igneoplastic* masses by violent pressure and deformation.

"It may be doubtful what would be the physical and thermal effect of enormously great pressure upon rock powder such as was produced in Mallet's experiments; but it would seem that if *made* to yield, the frictional effect must produce very high temperatures. *A fortiori*, solid detrital masses of variously sized fragments intermingled (such as, rather than powder, would be likely to result from steady pressure), yielding rapidly under great pressures, might, under the *combined influence of friction and rock-crushing*, well be supposed to reach the temperature of fusion, which a simple crushing of a solid mass by pressure would have failed to produce. Mallet mentions the probable influence of friction and of the squeezing of igneoplastic masses, but does not attach to these agencies such importance as they seem to me to deserve.

"Of the complex thermal effects of the movements of detrital masses under great pressure Mallet's figures of course offer no measure whatsoever; nor is this, or even the thermal coefficients resulting from his rock-crushing experiments, at all necessary to the establishment of the postulates of his theory."

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* Phil. Mag., July, 1874, p. 41.

Subsequently the Rev. O. Fisher, in a paper read before the Geological Society of London, May 12, 1875, entitled "Remarks upon Mr. Mallet's Theory of Volcanic Energy," has repeated the observations of Professor Hilgard, and extended his objections to the author's theory in general in a way which appears not warranted. It will be sufficient here to quote the following from Mr. Fisher's paper:—"Indeed the form in which the objection to Mr. Mallet's reasoning suggested itself to my mind on first reading his paper was simply this. If crushing the rocks can induce fusion, then the cubes experimented upon ought to have been fused in the crushing; and I still adhere to this simple mode of expressing my objection." Again:—"He considers that the heat so developed may be localized, and that the heat so developed by crushing, say 10 cubic miles of rock, may fuse 1 cubic mile. But I ask why so? The work is equally distributed throughout; why should not the least be so also? Or if not, what determines the localization? For example, suppose a horizontal column 10 miles in length and 1 in sectional area to be crushed by pressure applied at its ends, which of the 10 cubic miles is the one to be fused? But if no cause can assign one more than another, it is clear that they will all be heated by 170° and none of them fused."

If a cube of rock, which in free air is found to crush under a certain pressure, be imagined situated deep within a mass of similar rocks and there crushed, it does not admit of dispute that the work necessary to effect crushing must be largely increased; the particles of the cube and the entire mass of surrounding rocks are under the insistent pressure of the superincumbent rock in a state of elastic equilibrium. It follows, therefore, that the pressures of the surrounding rock produce the same effect upon the cube as regards resistance to crushing as if they were cohesive forces acting within the cube; and the work necessary to crush the cube by its finally giving way, in whatever direction this *encastrement* by pressure may be least, will be increased over that which would crush it in free air nearly in the ratio in which the imaginary cube is exposed to external pressure greater than that in air. Thus, if the cube of Guernsey granite (No. 12, Table I, Phil. Trans., part 1, 1873, p. 186) which required 4,336,712 lbs. per square foot to crush it in air, equivalent to a superincumbent column of the same rock of the mean specific gravity 2·858, or weighing 178·3392 lbs. per cubic foot, be supposed situated at a depth of ten to twenty statute miles, it will require rather more than 2·14, or, at twenty miles, 4·28 times as much pressure upon two opposite faces to crush it that it did when in air; and if we assume the displacement of the crushed particles after crushing to be the same as in the case of the cube crushed in air, then the work and the heat due to its transformation will be also 2·14, or 4·28 times as great. And, as in the case of the cube crushed in air the heat developed was sufficient to fuse at (at 2000° Fahr.) 0·108 of its own volume, or, in other words, the crushing of 10 cubic feet of the rock would be required to raise to that point one cubic foot, then in the case of the imaginary cube situated at the depth of ten miles enough heat would be evolved by the work of crushing each cubic foot to fuse 0·231 cubic foot, or, at twenty miles, to fuse 0·462 cubic foot of the same rock, or nearly half the volume crushed,—and this assuming that the initial temperature of the rock at 10 or 20 miles depth was only 57° Fahr. as in the author's experiments, instead of from 500° to 1000° Fahr. or more as it may be at 10 to 20 miles depth. Therefore, under the pressure due to a depth of 20 miles and an initial temperature of 1000° Fahr., the heat developed by the work of crushing each cubic foot of rock will be sufficient to fuse its own volume. Thus also if we assume the fusing-point of the rocks not to be 2000° Fahr., as indicated by the author's experiments on the cooling of slags, but considerably higher, say 2500° or more, we have still a sufficient supply of heat due to crushing alone to bring 0·8 of the entire volume to the fusing-point.

These considerations, apart from all others yet to be adverted to, appear fully sufficient to refute the Rev. O. Fisher's first objection above quoted; indeed the statement that if under any circumstances and in the rock-masses of nature "crushing can induce fusion, then the cubes experimented upon ought to have been fused in the crushing," seems as unsupportable as it would be to affirm that no heat is developed by the slow oxidation (*eremacausis*) into water and carbonic acid of a pound of wood, which when burned develops a well-known amount of heat.

The depths above assumed do not widely differ from those at which the foci of earthquakes have been found by the author (Report on Neapolitan Earthquake) in 1857, and by others since that time, and which may be presumed to indicate in some degree the possible depth of volcanic activity.

The writer now proceeds to reply to the second objection of the Rev. O. Fisher as above quoted, which appears to him based entirely on a misconception of the physical conditions involved. Let us consider what will happen in the case of a prism or column of rock crushed against the face of an unyielding mass. If the prismatic mass be not homogeneous throughout, crushing will commence at the weakest place; if it be perfectly homogeneous, crushing will commence and continue where the prism is in contact with a fixed mass, and that whether the prism be crushed at one or both ends—because it is at such surface of contact that the compression of the particles of the prism is greatest, and where therefore the elastic limit of their cohesion is first and successively overpassed. This may be seen illustrated in the stonework of buildings the material of which is overloaded, where crushing or spalling off of the ashlar stones only occurs at and near the joint.* In either case, whether the prism be homogeneous or not, the crushing must be localized either to the end or ends of the prism, or to the plane of weakness where it first yields, and which then becomes the crushing surfaces of two opposed prisms. It is these physical conditions which "determine the localization" of crushing in the prism, and which conditions have been disregarded in the Rev. O. Fisher's objection. Let us now consider the subsequent effects of the successive crushing of a column of prismatic mass of rock, one extremity of which is continually urged against the face of a fixed mass of rock which does not yield, a case which approximates to that which most frequently occurs in nature, and which, to fix our ideas, we may suppose presents a face for crushing one square foot; and being continually urged forward, and the pressure being greatest where the pressing column comes into contact with the fixed mass of rock, the extremity of the column supposed homogeneous, or the parts adjacent thereto, are continually crushed by a succession of *per saltum* movements. The first cubic foot of the column that is crushed has its temperature raised, let us suppose, by the minimum of 217°. The crushed fragments at this temperature are pushed aside by the advancing column, whose extremity is thus surrounded by crushed materials at a temperature of 217°, and the second foot in length of the column becomes crushed. But the material of this second cubic foot is at a higher temperature before it is crushed than was the first cubic foot; so that the heat due to the transformed work of crushing of each successive cubic foot of rock raises its temperature to a higher point than that of the preceding one, because each successive cubic foot at the instant before crushing is at a temperature already higher than the preceding ones, resulting from the heat taken up by the uncrushed column from the hotter portions of material surrounding it that have already been heated by crushing; so that, if T be the temperature produced in the first cubic foot crushed, and t be the temperature of the crushed material which communicates a portion of its heat to the next cubic foot crushed, the temperatures of successive cubic feet crushed may be illustrated by some such series as the following:—

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* See also E. Hodgkinson's experiments on the directions of fracture of crushed material, Brit. Assoc. Report, vol. vi; and Tredgold on Cast Iron, by Hodgkinson, part 2, p. 319, and plate 1.

Cubic feet crushed.

No. I.	No. II.	No. III.
T	$T+\frac{t}{n}$	$T+\frac{t}{n}+\frac{t}{m}$ . . . &c.

We have here supposed the column crushed at atmospheric pressure; but if crushed under an insistent column of 20 miles, then the temperature T would be 4·28 times 2·17°=928°, and the subsequent temperatures correspondingly increased.

No limit arises to this continual augmentation of temperature while the rock retains its rigidity; after that has been seriously impaired or lost, any further exaltation of temperature apart from the detrusion or transport of fragmentary matter, as hereafter referred to, becomes dependent upon the deformation and detrusion of a more or less plastic mass. It is well ascertained, however, by observation on a great scale, that granite remains rigid at a temperature nearly approaching the softening point of cast iron;* so that a large range of rigidity must exist for the exaltation of its temperature in the way above suggested; and in the state of aggregation which we are warranted in supposing rocky masses to exist in at considerable depths, it is probable that this range of rigidity would be even further extended than in the case of granites found at or near the present surface of our globe.

There is a close analogy between the conditions of gradual exaltation of temperature above sketched, and those by which aërolites, flying at an immense velocity through our atmosphere, are heated from the temperature of the stellar spaces to that of incandesence or even fusion of those bodies. The aërolite, which, according to Schiaparelli, may in some instances be forced through our atmosphere with a relative velocity exceeding 3500 feet per second (one enormously exceeding that at which air can rush into a vacuum), compresses the stratum of air immediately in advance in almost the same manner as if at the first instant of contact the air were a rigid body. The temperature developed is greater as the velocity of compression is so, and as the volume compressed is less; the most highly heated air is therefore the stratum directly in contact at any instant with the stone; and the latter licks up more or less of the heat as it passes through a succession of such compressed strata, and so receives continual accessions of heat until the temperature of the meteoric stone itself reaches the limit given by that of the stratum of compressed air in immediate contact with it at any instant. If a body as mobile and compressible as air can thus by sufficiently rapid compression be raised above the temperature of incandescence, we may readily conceive how great an exaltation of temperature may be produced in the rigid materials of our earth's crust when exposed to a pressure which may be viewed as limitless in reference to the resistance opposed to it, and which, in consequence of the conditions of elastic resilience hereafter referred to, may give rise to motion and crushing with velocities even exceeding those with which aërolites traverse our atmosphere.

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* The observations upon which these statements are founded have been made after various great conflagrations of stores or warehouses at London, Liverpool, and Dublin, into the construction of which granite blocks and cast iron in columns, girders, &c., largely entered. The cast iron was either melted or softened to the consistence of soap; the granite heated to like temperature, except being split in various directions, was found unaltered, except more or less in color, after having been again cooled.

The well-known experiment of cutting a hard steel file in two by the rapid rotation of a thin disk of soft sheet iron pressed against it is another example. The heat developed at the working-point, so far as it is communicated to the disk, is rapidly carried off and dissipated by its rotation, and it thus remains cool enough to be touched by the hand, although the heat developed by it and accumulated at and near the working-point in the file is sufficient to raise that to the temperature at which cast steel becomes softened and approaches fusion.

The cutting of steel railway bars across when at a very low red heat by a rapidly revolving circular saw, which revolves partially immersed in cold water, and from whose action a torrent of incandescent fragments of steel is discharged, is a like case.

Besides the heat transformed from the work of compression and crushing, a large amount of heat must also be generally produced by transformation of the work expended in friction and detrusion. No experiments have as yet, to the author's knowledge, been made upon the amount of heat developable in fragmentary pulverulent masses, such as sand, by the forcible transposition of more or less of the particles; nor do we know with certainty the conditions under which external mechanical pressure is transmitted through sand or like discontinuous matter. As in rigid solids exposed to unequal mechanical pressures there exist planes or surfaces within the mass such as have been denominated by Moseley "planes of easiest shearing," or sliding, so in masses of pulverulent matter, whatever be the shape or size of the particles, provided these be small in relation to the whole mass, and their mutual adhesion (if any) small also, such planes must by unequal mechanical pressure be brought into existence. Along any such plane we may imagine the sand or other pulverulent matter forced to move over itself in opposite directions at opposite sides of the plane; that is to say. we may suppose the sand forced along such a plane much in the same way that a mass of sandstone or of granite would be forced along such a shearing plane as had been produced in it previously by mechanical pressure. If this reasoning be admitted, we must suppose that heat would be developed along such a plane and at short distances from it in a way more or less analogous to that produced by forcing one rough surface of stone over another. What the coefficient of friction in this case would be can only be determined by experiment; but we may justifiably conclude that the amount of friction per unit of surface would increase proportionately to the pressure applied externally to the entire mass—exposed to more or less of which, motion at any such surface of friction must take place. Coulomb, Morin, and others have found the friction of some sorts of rough stone upon other rough stone to reach as much as three-fourths the pressure; and should this coefficient increase proportionately under the enormous pressures to which a discontinuous mass at several miles depth may be subjected, we can readily see that the transformed heat of friction produced by internal movements taking place in such materials after crushing has occurred, must be the source of a large amount of heat over and above that originally due to the crushing itself. Thus, for example, if we assume a surface of such disintegrated material sliding over a similar surface, or over a rough surface of coherent rock, and under the pressure of ten miles of rock of the specific gravity of granite, at the rate of one foot per second, and if we take the coefficient of friction as low as 0·5, we have 4,326,600 foot-pounds of frictional work per second, which, divided by J (=772), gives 5604 units of heat evolved per second from each square foot of surface; and to this development there is no limit while the circumstances continue the same, except that of the distance that one surface is forced over the other. And great as is this evolution of heat under such enormous pressures, it would be further increased in the event of the fragmentary particles being heated so as to present incipient viscosity of surface and more or less of mutual agglutination.

Temperature with respect to any given solid material is dependent upon the units of heat present in a unit of mass or of volume of the substance. If for the same total heat we diminish the mass or volume, the temperature is proportionately increased. When the material is surrounded by matter capable of carrying off heat by conduction, or evection, or radiation, and the heat is evolved within the mass by work done upon it, then another condition, that of time, has to be taken into account: for the shorter the time within which a given amount of heat due to transformation of work is evolved within the unit of mass, the less of that total is dissipated by conduction, &c.

A familiar example of this is every day seen in the light and heat elicited by abrasive friction, or collision between hard bodies. When two lumps of granite or other hard stone are struck together, heat and light are instantly evolved at the small surface of contact, where the material of one or both masses is crushed. The work done may be but that of the crushing of a fraction of a grain of the stone; but it is great in reference to the extremely brief instant of time during which the work is performed: the crushed particles are raised to a temperature of luminosity for a brief moment because there is not time for the surrounding surfaces of the cold stone to carry off the heat evolved by conduction, though the dissipation of heat thus produced is such that the luminosity again instantly disappears. The temperature at the crushing-point is greater as the work done in a unit of time and upon a given weight of the material is greater. That the temperature capable of being thus produced approaches that of the fusing-point of steel, is of a gun-lock by the flint. In the case of small masses of rock, evident from the phenomenon of a spark struck from the steel such as the 1½-inch cubes of the author's experiments, crushed between two opposite surfaces of steel, the actual temperature of the crushed particles can never be found to reach that due to the work of crushing; for the heat of relatively small mass of the crushed cube in close contact with far larger masses of cold steel of high conductivity is carried off almost as fast as it is evolved; and as the total amount of heat evolved from the crushing of such a cube of the hardest rock experimented upon by the author (namely, number 12, Table I, Phil. Trans., part 1, 1873, p. 186) could raise its own mass only through 217°, if the temperature of fusion of the rock may be taken at 2000°, it is obvious that such a cube could not be fused by the work of crushing alone, even though all the heat due to the crushing work remained in the cube, none being dissipated to surrounding objects.

In the case of a cube such as this losing heat by dissipation, the temperature of the crushed mass depends upon the time in which the work of crushing is done. In the author's experiments the crushing of each cube in column 12 occupied a mean time somewhat greater than that in which a heavy body could fall freely through a space of 0·09 foot (No. 12, Table I. *l. c.*, col. 19)—that is, 0·075 of a second; for more rapidly than that the crushing surfaces could not approach each other. If, however, the conditions had been such that but $\frac{1}{n}$th the above time were expended in the crushing, then a proportionately less quantity of the heat evolved would have been dissipated; and this, we shall see further on, must be the case in nature. When two rock-surfaces are urged against each other in the shell of our globe by the gradual withdrawal of support by contraction of the nucleus, the rocky masses for great distances from the opposed surfaces are brought into a state of elastic compression, gradually increasing up to the crushing-point somewhere, when a greater or less portion of rock suddenly gives way by crushing and is more or less removed by detrusion in some lateral direction. The material of the rock for a greater or less distance from the crushing-point is therefore in the condition of a compressed spring which is suddenly released. When so released, the velocity of resilience depends principally upon the modulus of elasticity of the rock; and the velocity of release of the spring, which is that with which the crushing is performed, is extremely great in the case of hard granite or generally similar homogeneous rock, in which it probably exceeds 10,000 feet per second, though in very much less-elastic rocks falling considerably short of this.* But had the rock-specimen crushed in the author's experiments been a cubic foot in place of an inch and a half cube, the time of crushing must have been *greater* than ·249 second (or nearly a quarter of a second), or the crushing would have been performed with nearly a velocity of 2 feet per second, and that would be less than $\frac{1}{5000}$ of the velocity with which the same would have been crushed if circumstanced as in the shell of our globe. And if we extend our view from the crushing of a cubic foot or two to that of a cubic mile or more, we see that there would be very little of the total heat evolved lost by dissipation, there being scarcely any time in which that could occur, the possible rate of crushing of a cubic mile being less than half a second.

In the case of a very large mass of rock crushed simultaneously, or nearly so, as every portion of the rock evolves heat proportionate to the crushing-work done upon it, so the heated portions of crushed material situated near the exterior of the entire mass act as "*jacketing*," so as to preserve the deeper-seated portions of the heated mass almost absolutely from any dissipation of heat for a considerable length of time, that time depending, *cæteris paribus*, on the conductivity of the crushed material and the difference in temperature between the crushed material and the uncrushed rock adjacent to it. There are some experimental grounds for concluding that the absolute crushing-force per unit of volume in any given rock increases as the absolute volume of the mass simultaneously crushed becomes greater; thus it has been found by Rondelet that large cubes of stone required a proportionately larger crushing-force than smaller ones; much stress, however, cannot be laid upon this, as we cannot assume with any certainty what are the precise conditions under which rock-surfaces in our earth's shell are forced together, and the distribution of the crushing-pressure may be indefinitely varied.

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* See the author's "Earthquake-wave Experiments," Phil. Trans., 1862, vol. clii.

In the author's experiments the cubes crushed by pressure on two opposite faces were free upon the other four; it cannot be doubted that, had only two opposite faces been free and the pressure applied simultaneously upon the four other faces, two and two respectively, the pressure necessary for crushing or the work thereof would have been considerably increased. Further, if none of the faces were free, and all those except the two opposite faces to which the crushing pressure is supposed applied, had the motion outwards of any of their particles opposed by an initial pressure, such as that of an insistent mass of rock, the work to be done to produce crushing would be necessarily increased by the amount required to overcome such initial pressure, as has been already pointed out.

Another and further source of heat arises after crushing and detrusion of the fragmentary matter, and after the latter has arrived at a temperature at which the fragments have become more or less viscous and adherent by reason of the further work expended in the deformation and detrusion by forcing forward through highly irregular or constricted rock-channels of the heated and now viscous mass. There do not exist at present sufficient data by which to calculate the amount of work necessary to a given amount of deformation in viscous masses; and hence we cannot calculate the amount of heat that in nature might arise from it. Hirn, however, has shown that in the case of plastic bodies, such as lead, the heat developed is proportionate to the work done in deformation; so that, if we knew the pressure per unit of surface necessary to produce a certain deformation in an already heated mass of given viscosity, we could calculate how much its temperature would be exalted by the work of the assigned deformation.

Examples, however, are not wanting which prove that a very large exaltation of temperature can thus be produced—as, for example, in the old-fashioned method by which blacksmiths were accustomed to light their fires. A thin square rod of very good tough iron was hammered at its extremity by a succession of rapidly given blows from a light hand-hammer. After a minute or so the rod, for the portion of its length hammered at the extremity, became red, and in a second or two more of distortion of its form by continuance of the hammering, the iron could be made nearly white hot.

A similar example, on a much larger scale, occurs in the process of rolling iron or steel. When a heavy billet of iron or steel, heated to a brilliant yellow heat, is passed between the rolls of the iron mill, and the massive lump is rapidly elongated into a bar, its temperature, notwithstanding that it is rapidly and constantly losing heat by radiation and evection, is observed visibly to increase, so that the mass becomes at a certain stage white or welding hot by the transformation into heat of the work of deformation so rapidly and powerfully applied to it.

The action of the machine employed in the arsenal at Woolwich for making lead rods to be afterward pressed into bullets affords another striking example. In this machine a cylindric block of lead, maintained at a temperature of 400° Fahr., is by a steady pressure upon the end, which is 8″·5 in diameter, of 16,700 lbs. per square inch of its surface, forced through an aperture at the other extremity into a rod of 0″·525 diameter, at such a rate that five inches in length of the cylindric block becomes a rod of about 100 feet in length of the above diameter per minute. We have thus 393,906 foot-pounds of work done upon the lead per minute, dividing which by J we have 510·2 British units of heat developed per minute from the transformed work. In the actual machine the whole of this is ultimately dissipated and lost; but if none of it were dissipated, as the cylindric block of lead of 8·5 in. diameter by 0·416 ft. (5 inches) weighs 116·3 lbs., and the specific heat of lead is $=0·029$ (or perhaps a little more at 400° Fahr.), it follows that the heat developed by its deformation from the short cylindric block of 5 inches length to a rod of about 100 feet length, is enough to raise the temperature of the lead through 151° Fahr., or, were no heat lost, to raise its temperature from 400° to 551° or thereabouts—that is, within about 50° of its melting point. If, therefore, we could by a reverse process squeeze the 100-foot rod back into the original block of 8″·5×5″, we should find the lead in the latter not only liquid, but considerably above its temperatue of fusion, or at nearly 700° Fahr. It is obvious, therefore, that any viscous or plastic body such as lava, continually forced through apertures varying in area and form and suffering continual deformation, as when forced through a volcanic tube or vent, must have its temperature continually exalted so long as it continues thus to be urged forcibly forward, assuming, as is very nearly the truth in nature, that an extremely small proportion of the heat developed in the process can be dissipated by conduction to the walls of the tube.

The preceding remarks appear to the writer sufficient to show that there is no physical difficulty in the conception involved in his original memoir (Phil. Trans. 1873), but not there enlarged upon in detail, that the temperatures consequent upon crushing the materials of our earth's crust are sufficient locally to bring these into fusion.

ART. XXXV.—SIR CHARLES LYELL.

No European geologist was so well known, personally, in the United States as Lyell. His two visits to this country, in 1841 and 1845, recorded in four volumes of travel characterized by great good judgment, large mindedness and catholicity, made his name familiar throughout the land, and gave a degree of popularity here to his philosophical and technical writings which they would otherwise have hardly obtained.

Called by Mr. Lowell to Boston in 1841 to deliver a course of twelve lectures on Geology before the "Lowell Institute," Lyell was the first European, of eminence in science, who appeared upon the platform as a lecturer before an American audience. That his lectures were highly esteemed is well known, and it was a sufficient evidence of this that he was again invited to Boston on a like commission in 1845–6, and before the same institution. The personal relations and friendships commenced on these occasions endured to the end, and were rendered doubly interesting by the charm shed over every social relation by Lady Lyell, who won universal esteem by those qualities of manner which were less prominent in her often abstracted husband. The following familiar private letter from the late Dr. Mantell, written in 1841 to Prof. Silliman (the elder), gives a vivid sketch of Lyell as he appeared to his scientific associates at the time of his first visit to the United States. As all the parties named in this letter are now passed away there can be no objection to its reproduction in this connection.

"LONDON, *June* 14th, 1841.

"MY VERY DEAR FRIEND:

"I was about to write you to inform you of Mr. Lyell's intentions which he communicated to me but a short time since. I dined with him last week—a farewell party. His charming little wife, a daughter of Mr. Leonard Horner, accompanies him. I have said so much of you and yours to her that she is quite anxious to visit New Haven; if she does I am sure you will be delighted with her. And now for a strictly private sketch of my old friend. About twenty years or more ago, one beautiful summer evening, a young Scotchman called at Castle Place (Lewes*) and announced himself as Mr. Lyell, stating that he was fond of geology, had been attending Jameson's lectures at Edinburgh, and had visited his former Alma Mater, Midhurst Grammar School, in the west of Sussex; and that, while rambling about the

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*** Mantell's place of residence at that time.**

neighborhood, he found some laborers quarrying in stone which they called whin. As this term is *Scoticé* trap: the young traveler was much puzzled to know how such a rock appeared in the south of England, and upon inquiry of one of the laborers why the stone was so called, the man referred him to 'a monstrous clever mon as lived at Lewes, a doctor who knowed all about them things and got curiosities out of the chalk pits to make physic with.' The man, in short, had been formerly a Lewes quarryman, and one of my collectors. Mr. Lyell being alone and on horseback and having nothing better to do, rode gently over the South Down, some twenty-five miles, and at the close of the day found himself at my residence. We were mutually pleased with each other; my few drawers of fossils were soon looked over, but we were in gossip until morning, and then commenced a friendship which has continued till now.

Mr. Lyell was educated for the bar. He practiced on the western circuit seven or eight years, and he allowed me to correspond with him only during the vacations. His father, who is a Scotch Laird, is still living, and there are several sons and daughters. Mr. Lyell is the eldest, and at the death of the father inherits the family estate, which, I believe, is moderate. However, about seven or eight years after our acquaintance, Mr. Lyell with great good sense, abandoned his profession, with his father's consent, and devoted himself wholly to geology, content with a moderate income, and living in a very unostentatious manner in an unfashionable part of the city. A few years ago he married Miss Horner, who is much younger than himself (Lyell is 45 or 46), and a more suitable companion he could not have found. He has no children. In person, Lyell presents nothing remarkable except a broad expanse of forehead. He is of the middle size, a decided Scottish physiognomy, small eyes, fine chin and a rather proud or reserved expression of countenance. He is very absent, and a slow but profound thinker. He was Professor in King's College, London, and gave lectures there and at the Royal Institution, but it so happened that I never heard him lecture. He always takes part in the discussions at the meetings of the Geological Society, but he has not facility in speaking; there is hesitation in his manner, and his voice is neither powerful nor melodious, nor is his action at all imposing. As a popular lecturer he would stand no chance with Buckland or Sedgwick. He is providing himself with very beautiful illustrations for his lectures at Boston; and I should suppose the prestige of his name and his European reputation will insure him a flattering reception. * * * * There is a hauteur or reserve about Mr. Lyell to strangers that prevents his being so popular among our society as he deserves to be. I believe him to have an excellent heart, and he is very kind and affectionate when his better feelings are called upon." * * * *

This criticism of Lyell's style and manner as a public speaker was certainly well founded, as all will agree who ever heard him lecture. But despite all infelicities, so great was the value and richness of his matter, that he commanded the most respectful and interested attention from his auditors. The reader of his "Principles" could not fail, however, to be struck with the fact that the classic elegance of Lyell's style, for which his more important productions are so justly celebrated, must have been the result of much labor.

We cite from the Geological Magazine edited by Henry Woodward, F. R. S., the following notice of his life and labors. A more elaborate memoir may be expected in the next annual address of the President of the Geological Society of London.

"On Monday, the 22d of February, at his residence in Harley Street, and in his seventy-eighth year, Sir Charles Lyell, after a long life of scientific labor, passed peacefully from amongst us, to his honored rest.

"To the outside world it may seem strange that the death of a man who was neither statesman, soldier, nor public orator, should arouse our sympathies so strongly, or that he should be so highly esteemed all over the world; but geologists know well what Lyell has done for them since he published the first volume of 'The Principles of Geology' in 1830.

"It is in the character of historian and philosophical exponent of geological thought that Lyell has achieved so much for our science: nor can we fail to remember that those clear and advanced views, for which he became so justly celebrated, were advocated by him forty-five years ago, at a time when scientific thought was still greatly trammelled by a strong religious bias, and men did not dare to openly avow their belief in geological discoveries nor accept the only deduction which could be drawn from them.

"Born at Kinnordy, his father's seat near Kerriemuir, in Forfarshire, on the 14th of November, 1797, Lyell received his early education at a private school at Midhurst, and completed it at Exeter College, Oxford, where he took his Bachelor's degree in 1819, obtaining a second-class in Classical honors in Easter Term. On leaving the University, he studied for the Bar, but never practised that profession, his tastes having been led by Dr. Buckland's lectures to the study of geology as a science. In 1824 he was elected an Honorary Secretary of the Geological Society of London, of which he was one of the earliest Fellows. On the opening of King's College, London, a few years later, he was appointed its first Professor of Geology. He had already contributed some important papers to the 'Transactions' of the Geological Society, including one 'On a Recent Formation of Freshwater Limestone in Forfarshire, and on some Recent Deposits of Freshwater Marl, with a comparison of recent with ancient Freshwater Formations, and an Appendix on *Gyrogonites*, or Seed-Vessels of Chara;' also one 'On the Strata of the Plastic Clay Formation exhibited in the Cliffs between Christchurch Head, Hampshire, and Studland Bay, Dorsetshire;' another 'On the Freshwater Strata of Hordwell Cliff, Beacon Cliff, and Barton Cliff, Hampshire;' and an elaborate paper on the 'Belgian Tertiaries.' In 1827 he contributed to the *Quarterly* a review of Mr. Poulett Scrope's 'Geology of Central France' (the perusal of which is said first to have stimulated him to prepare and publish 'The Principles of Geology' on which his reputation as a philosophical writer mainly rests). These lesser works all showed a power of observation and of generalization which prepared the learned world for some greater and more important treatise from his pen, which should deal, not with local details, but with the general principles of the science. Nor were they disappointed when his *magnum opus*, 'The Principles of Geology,' appeared in three successive instalments, published respectively in 1830, 1832, and 1833. The work, subsequently enlarged into two volumes, has passed through numerous editions, and is still in as much demand as ever among students of the science. The work was subsequently divided into two parts, which have been published as distinct books, viz. 'The Principles of Geology, or the Modern Changes of the Earth and its Inhabitants, as illustrative of Geology,' and secondly, 'The Elements of Geology, or the Ancient Changes of the Earth and its Inhabitants, as illustrated by its Geological Monuments.' The substance of the last-named work has also been published under the title of 'The Manual of Elementary Geology,' a French translation of which was issued under the auspices of the famous Arago.

"Already, some time previous to the publication of this work, Mr. Lyell had been chosen a Vice-President of the Geological Society; and in 1828 he had undertaken a journey into the volcanic regions of Central France, visiting Auvergne, Cantal, and Velay, and continuing his journey to Italy and Sicily. He published the results of this expedition in the 'Edinburgh Philosophical Transactions,' and also in the 'Annales des Sciences Naturelles.'

"It was, however, the publication of his 'Principles of Geology' that gave him that established reputation which he ever since continued to enjoy. 'Which of us,' asked Prof. Huxley, in his Anniversary Address to the Geological Society in 1869, 'has not thumbed every page of the "Principles of Geology?"' And he adds, 'I think that he who writes fairly the history of his own progress in geological thought will not easily be able to separate his debt to Hutton from his obligations to Lyell.' This cordial testimony of a fellow-laborer in the cause of scientific enlightenment exactly indicates Sir Charles Lyell's place in the history of that task. He was a man of singularly open mind, one of those who stand above their contemporaries and hail the dawn of new truths upon the world. His own works mark the progress of his own as well as of the public opinion on the great problems raised by scientific discovery, and he remained to the end of his life always ready for the reception of new facts, and for the corresponding modifications of opinion.

"Sir Charles Lyell had traveled and seen much. Thus in early manhood he explored many parts of Norway, Sweden, Belgium, Switzerland, Germany, and Spain, including the volcanic regions of Catalonia. In 1836 he visited the Danish Islands of Seeland and Monen, to examine their Cretaceous and Tertiary strata. In 1841 he was induced to cross the Atlantic, partly in order to deliver a course of lectures on his favorite science at Boston, and partly in order to make observations on the structure and formation of the Transatlantic Continent. He remained in the United States for a year, traveling over the Northern and Central States, and extending his journey as far southward as Carolina, and northward to Canada and Nova Scotia, his exploration ranging from the basin of the St. Lawrence to the mouths of the Mississippi. On returning from this journey, he published his 'Travels in North America,' a work of considerable interest to other persons besides geologists, and showing that he could extend his observations to the stratification of society around him as well as that of the earth beneath his feet. He paid a second visit to America in 1845, when he closely examined the geological formation of the Southern States and the coasts that border on the Atlantic and the Gulf of Mexico, and more especially the great sunken area of New Madrid, which had been devastated by an earthquake 30 or 40 years previously. Upon reaching England, he published his 'Second Visit to the United States,' a companion to his former work. For his other scientific papers we must refer our readers to the 'Proceedings' of the Geological Society, 1846–49, and its 'Transactions.'

"Late in life, about ten or twelve years ago, Sir Charles Lyell published another very important work, on 'The Antiquity of

Man,' summarizing and discussing all the important facts accumulated up to that time in favor of the high antiquity of the human race, viewed from the standpoints of the archæologist, the geologist, and the philologist.

"Numerous honors were conferred on Lyell in recognition of his services to science. As far back as 1836 he was elected to the Presidential Chair of the Geological Society, to which he was re-elected in 1850. He received from Her Majesty the honor of knighthood in 1848, and in 1855 the honorary degree of D.C.L. of the University of Oxford was conferred upon him. He had been for many years a Fellow of the Royal Society, and in 1833 received one of the Royal Society's Gold Medals for his 'Principles of Geology.' In 1858 the Royal Society conferred upon him the highest honor at their disposal—the Copley Medal; and in 1864–5 he filled the Presidential Chair of the British Association for the Advancement of Science. He received the Wollaston Gold Medal from the Geological Society of London in 1865 (his continued official connection with which had precluded his receiving it earlier). He was raised in 1864, on the recommendation of the then Prime Minister, Lord Palmerston, to a Baronetcy, which now becomes extinct by his decease. He was a Deputy-Lieutenant for his native county of Forfarshire.

"Sir Charles Lyell has been so long and so honorably known among the scientific teachers of the time, that though he had arrived at his seventy-eighth year, and the period of his chief intellectual and physical activity had long passed away, probably even the younger men of the present generation will feel that science is poorer by his loss.

"At the meeting of the Geological Society of London, held in the Society's room, Burlington House, Picadilly, on Wednesday last (February 24th), the President, John Evans, Esq., F.R.S., before commencing the business of the meeting, alluded to the great loss which all present had sustained. He little expected, when speaking on the last occasion, at the Anniversary Meeting, of the services which Sir Charles Lyell had rendered to science for the previous fifty years, that he should have on the present occasion to announce and lament his irreparable loss. Sir Charles Lyell had been a true philosopher and a sincere friend. He had lived to see the extension of science which he had so eagerly desired realized. In future times, wherever the name of Lyell shall be known, it will be as that of the greatest, the most philosophical, the most enlightened geologist of Great Britain or Europe.

"In accordance with the wish of the Council of the Royal Society, Sir Charles Lyell will rest beside his old friend and fellow-laborer in science, Sir John Herschel, in Westminster Abbey."

We add the following appreciative remarks from Nature of March 4th.

"Lyell's claim to fame lies in this, that he organized the whole method of inquiry into the history of the formation of the crust of the earth, and established on a sound footing the true principles of geological science; his theory being that, by the uniform action of forces such as are now in operation, the visible crust of the earth has been evolved from previous states.

"Lyell was not only a keen investigator of natural phenomena; he was also a shrewd observer of human nature, and his four interesting volumes of travel in America are full of clever criticism and sagacious forecasts. His mind, always fresh and open to new impressions, by sympathy drew towards it and quickened the enthusiasm of all who studied nature. Had he done nothing himself, he would have helped science on by the warmth with which he hailed each new discovery. How many a young geologist has been braced up for new efforts by the encouraging words he heard from Sir Charles, and how many a one has felt exaggeration checked and the faculty of seeing things as they are strengthened by a conversation with that keen sifter of the true from the false!

"Though by nature most sociable and genial, yet Sir Charles often withdrew from society where the object of his life, the pursuit of science, was not promoted; but when anything interesting turned up he always tried to share his pleasure with all around. Many of us will remember the cheerful and hearty 'Look here'—'Have you shown it to so and so?'—'Capital, capital'

"The little wayside flower, and, from early happy associations, still more, the passing butterfly, for the moment seemed to engross his every thought. But the grandeur of the sea impressed him most; he never tired of wandering along the shore, now speaking of the great problems of earth's history, now of the little weed the wave left at his feet. His mind was like the lens that gathers the great sun into a speck and also magnifies the little grain he could not see before. He loved all nature, great and small.

"Much we owe to Leonard Horner, himself a good geologist, for having inspired the young Charles Lyell. In after years, when already well known, Charles Lyell chose as his wife the eldest daughter of his teacher and friend. Many have felt the charm of her presence—many have felt the influence of the soul that shone out in her face; but few know how much science directly owes to her. As the companion of his life, sharing his labor, thinking his success her own, Sir Charles had an accomplished linguist who braved with him the dangers and difficulties of travel, no matter how rough; the ever-ready prompter when memory failed, the constant adviser in all cases of difficulty. Had she not been part of him she would herself have been better known to fame. The word of encouragement that he wished to give lost none of its warmth when conveyed by her; the welcome to fellow-workers of foreign lands had a grace added when offered through her. She was taken from him when the long shadows began to cross his path; but it was not then he needed her most. When in the vigor of unimpaired strength he struggled amongst the foremost in the fight for truth, then she stood by and handed him his spear or threw forward his shield. He had not her hand to smooth his pillow at the last, but the loving wife was spared the pain of seeing him die.

"It doubtless occurred to many a one among the crowd who saw him laid to rest among the great in thought and action, that he might have been eminent in many a line besides that he chose.

"His was a well-balanced judicial mind, which weighed carefully all brought before it. A large type of intellect—too rare not to be missed. But it was well that circumstances did not combine to keep the young laird on his paternal lands among the hills of Forfarshire: it was well for science that he was induced to prefer the quieter study of nature to the subtle bandying of words or the excitement of forensic strife. Failing health had for some time removed him from debates. Still to the last his interest in all that was going on in this scientific world never failed, and nothing pleased him more than an account of the last discussion at the Geological Society, or of any new work done. As a man of science his place cannot be easily filled; while many have lost a kind, good friend."

The number of *Nature* for August 26, contains an excellent portrait of Sir Charles Lyell, accompanying a biographical notice by Prof. Giekie.*

A list of Lyell's memoirs to the close of 1863 will be found in the Royal Society Catalogue, numbering, with his elaborate works, no less than seventy-one separate communications in his own name, and five more in connection with others.

*** Artist's proofs of this portrait (engraved on steel by C. H. Jeem) may be had at the office of *Nature*, 29 Bedford Street, Strand, London, W. C. Price 5*s*. each.**

ART. XXXVI.—*On the Arithmetical Relations between the Atomic Weights;* by M. D. C. HODGES.

THE group of elements, fluorine, chlorine, bromine and iodine, have, as their atomic weights, 19, 35, 5, 80 and 127 respectively. These numbers form a series, in which the difference between the succeeding terms is at first 16·5, then 44·5, and finally 47. The following groups of elements give similar series.

			Li=7	
C=12	O=16	N=14	Na=23	Mg=24
Si=28	S=32	P=31	K=39	Ca=40
	Se=79·4	As=75	Ru=85·5	Sr=87·5
	Te=128	Sb=122	Cs=133	Ba=137

The grouping of some of the above elements may be objected to, but there is no great change from that generally accepted.

If a group of such elements as copper, silver, cadmium, mercury, lead and bismuth is taken, the series of atomic weights gives a similar, but somewhat different result; in this case each term of the series is composed of several members.

Cu=63·4	(Zn=65·2)	
Ag=108	Cd=112	
Hg=200	Pb=207	Bi=210

The new difference, 96+, for the higher terms occurs.

Zinc may be added to the first term, and corresponds to the second member of the second term. The lower terms are here wanting. Similar to these are the following:

Ru=104·4	Rh=104·4	Pd=107	
Au=196·7	Pt=197·4	N=197·4	Os=199·4

Many of the remaining elements may be placed in the following series:

Be=9·4	Ti=50	In=75·8	Y=68	V=51·3
Al=27·4	Nb=94	U=120	E=112·6	Mo=96
Th=231	Ta=182	Tl=204		W=184

Sn(=118) may be placed as a second member of the fourth term of the series containing arsenic and antimony.

There remain chromium, manganese, iron, nickel, cobalt, zirconium, cerium, lanthanum, didymium, hydrogen and boron. The position of these may be better seen in the annexed table, in which all the elements, with the exception of hydrogen, are arranged on lines, the succeeding elements in the same line differing in their atomic weights by a few units or parts of units, and those in different lines from the corresponding elements in the preceding or following line by one of the differences mentioned.

		2·6				·00125	·00143		·972	1·743
(1.)	Br 9·4	Bo 11	C 12	----	----	N 14	O 16	F 19	Na 23	Mg 24
	2·56		2·49			1·826	2	·003	·865	1·577
(2.)	Al 27·4	----	Si 28	----	----	P 31	S 32	Cl 35·5	K 39	Ca 40
					7·42	5·63	4·24	3·1872	1·52	2·542
(3.)	----	----	----	----	In 75·8	As 75	Si 79·4	Br 80	Rb 85·5	Sr 87·5
				7·29	18·33	6·7	6·183	4·95		
(4.)	----	----	----	Sn 118	U 120	Sb 122	Te 128	I 127	Cs 133	Ba 137
(5.)										
(6.)	M 231	----	----	----	----	----	----	----	----	----
(1.)										
						5·5	6·8	7·2	7·8439	8·8
(2.)	----	----	----	----	Ti 50	V 51·3	Cr 52	Mn 55	Fe 56	Ni 58
						8·64				
(3.)	Zr 89	Ta 90	Ce 91·2	Di 94	Nb 94	Mo 96	----	----	----	----
(4.)										
						19·13				
(5.)					Ta 182	W 184	----	----	----	----
(6.)	----	----	----	----	----	----	----	----	----	----
(1.)										
	8·5					8·9		6·8		
(2.)	Co 59	----	----	----	----	Cu 63·4	----	Zn 65·2	----	Y 68
		11·2	12·1	11·4		10·57		8·7		
(3.)	----	Ru 104·4	Rh 104·4	Pd 107	----	Ag 108	----	Cd 112	----	E 112·6
(4.)										
		19·5	21·5	21·13	21·4	13·59	11·86	11·4	9·9	
(5.)	----	Cu 196·7	Pt 197·4	Ir 197·4	Os 199.4	Hg 200	Th 204	Pb 207	Bi 210	----
(6)	----	----	----	----	----	----	----	----	----	---

The double line marks the positions possibly not occupied by any elements.

In this table all of the above series are given, and the series, consisting of several members for each term, also, find their place. When we start from Be=9·4 and notice the following elements in the same line, we find that the atomic weights continually increase till Mg=24 is reached; there are no more elements in the same line, the next highest being Al(=27·4) in the second line; the case is similar for this line, the atomic weights increasing to Ca(=40), the next above being in the third line. The difference between the first and second lines is about sixteen units; it becomes forty-four plus for the second and third, and third and fourth; and is, finally, about ninety-three for the last term of the series, from Ti, Nb, Ta to Be, Al, Th. These three differences do not seem to be constant, but to increase with the values of the atomic weights; this is most apparent for the largest; the smallest appears constant.

It would seem probable that the lower terms of the series after magnesium, and as far as aluminum, should be wanting; as, otherwise, there would be two elements of equal atomic weights and differing largely in their chemical natures; which last would be shown by their having to be placed in different parts of the table, which does not occur. From Ba(=137) to the next highest, Ta(=182) is forty-five units, about the value of the middle difference. If there were other elements between these, there would be a similar anomaly to that just mentioned. The last terms of the series having the largest difference for their last terms, are Ta(=182) to Th(=231); the increase in these is forty-nine units. It may be that Th(=231) is the largest atomic weight.

One advantage of the table is its bringing elements of similar chemical and physical characters together. The metals and metalloids are separated almost completely.

There are certain objections to the classification of some of the elements in the series in which they have been placed. It appears that the chemical properties do not always exactly correspond in the members of the same series; this is best shown in the case of the soluble oxides and sulphides; others are those of thallium and lead, and perhaps silver. The boundaries in neither of these cases are to be considered sharp, and in both are oblique.

There is a singular fact in regard to the specific gravities of the elements, when thus arranged. The specific gravities of the elements in the same line vary regularly and have one maximum and minimum; the maximum is in each case in the neighborhood of the members of the gold group, and the minimum among the metalloids; the positions of these two points are farther to the right, the lower the line; this is best seen for the minimum. The numbers above the symbols give the specific weights. Uranium is an exception.

From what precedes it appears: 1, that the atomic weights of elements of similar properties form series, the difference between the lower terms of which series being sixteen, for the middle terms forty-four to forty-seven, and for the largest eighty-eight to ninety-six; 2, that certain terms of some of these series may be wanting.

This law, together with the law in regard to the variation of the specific gravities in the different lines of the given scheme of the elements, could be used in fixing the value of a new atomic weight after its determination by analysis. The specific gravity being known, there could be two points equally distant from the maximum or minimum in each line, in which, in general, the element could be placed; the value as found by analysis, or one of its multiples, would satisfy one of these positions.

It is somewhat strange that there is no position for hydrogen in the table.

ART. XXXVII.—*On Southern New England during the melting of the great Glacier*; by JAMES D. DANA. No. II.*

II. ABSENCE OF MARINE LIFE FROM LONG ISLAND SOUND THROUGH THE GLACIAL AND PART OF THE CHAMPLAIN PERIODS.

THE fact that the stratified estuary and seashore deposits of the New Haven region, and of other parts of the Connecticut coast bordering on the Sound, afforded me no trace of marine life was a puzzle so long as I looked upon them as true beach-made accumulations. I have continued my search at various localities, up to the present time, and in no case have I met with shells or other sea-relics. The evidence proves that the deposits are not of beach origin, but drift deposits made by the waters and gravel or sand of the melting glacier as they together hurried onward in floods to the sea, conjoined there with the action of the tidal currents and waves; and it establishes also, as I believe, that true shell-bearing sea-beaches do not here exist.

The same absence of marine products appears to characterize the drift along the northern shore of Long Island, or southern side of the Sound. The extended investigations of Professor Wm. W. Mather, while geologist of the State of New York, brought nothing of the kind to light; and my own recent examinations of the drift on the Island have been as barren of discovery. Moreover, I could gather no evidence from persons living in the sea-border towns that any shells had been found in the drift deposits *

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* For No. I. of this Memoir, see page 168.

Long Island Sound—the strip of water, 100 miles long and 5 to 20 miles wide, separating Long Island from Connecticut—is a very shallow trough. Its depth from its western limit to the mouth of the Connecticut river nowhere exceeds (as the Coast Survey charts show) 170 feet, and in general is between 75 and 100 feet; and through the small eastern portion, east of the Connecticut, it is for the most part under 200 feet. The depth was probably less than this in the Glacial era, since there is strong evidence that the water line was then at least 100 feet below its present level. However this be, so shallow a trough should have been occupied throughout with ice, if the glacier over Southern Connecticut were 1 500 feet in thickness, or even 1,000 feet; moreover the trough would have been the course at bottom of one of the largest of New England subglacial streams, since all the subglacial rivers passing through the State of Connecticut, including the chief river of New England, the Connecticut, would have been its tributaries. As the Champlain period opened the land subsided, giving greater depth of water to the Sound than now exists. But, with the melting going forward, the great stream of the Sound would have been swollen immensely in volume, and finally have borne icebergs seaward.

Such being the glacial conditions, the continuance of molluscan, articulate and verbetrate marine life in the Sound, like that now existing there, would have been impossible, and an extermination nearly or quite complete of the old fauna must hence have taken place.

After the melting of the glacier had ended. the return of the old species to the Sound sooner or later began; and it was probably accomplished before the Champlain period—the era of low level and great rivers—ended; but, with no elevated beaches to refer to as proof, the evidence of this fails us. The glacier had probably exterminated the temperate-climate life also of the seas just south of Long Island; for numerous bowlders, from one ton to two thousand tons in weight, of New England origin, show that the ice stretched on from the Sound in full force over Long Island, and must have had its terminal ice-cliffs somewhere to the south in the shallow water of the

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* Prof. Mather in his New York Geological Report (p. 262) gives, from the verbal reports of others, statements respecting the discovery of shells on Long Island, which are, in part at least, of uncertain credibility, and are supposed by him to indicate, where of value, the presence of Tertiary deposits or of Tertiary and upper Cretaceous, beneath the Drift. Besides the localities on Long Island, at depths varying form thirty to more than a hundred feet, shells are stated to have been obtained on Governor's Island, in New York Harbor, at a depth of about 100 feet. None of the shells or localities have ever been seen by a geologist, so that the precise value of the evidence is still in doubt.

Atlantic border, if not along the southern shores of this island. Under these circumstances the old life, which might early have repeopled the southern shores of Long Island and the seas on the east, would have been slow in repossessing itself of Long Island Sound.

Although the Sound has yet afforded no evidence of molluscan life in the Champlain period, there is a deposit full of shells, according to Mr. Sanderson Smith,* on Gardiner's Island, off the east end of Long Island at the entrance to Peconic Bay. The bed has a height of fifteen or twenty feet above the sea level. The species mentioned in the article are: *Nassa trivittata*, *N. vibex* Say, *Fusus decemcostatus ?* Say, *Purpura lapillus* Lam., *Columbella lunata* Say, *Natica duplicata* Say, *Chemnitzia interrupta* Stimp., *Crepidula unguiformis* Lam., *C. fornicata* Lam., *Bulla canaliculata* Gould, *Ostrea borealis* Lam., *Pecten Islandicus* Chemn., *P. Magellanicus* Lam., *Arca transversa* Say, *Cardita borealis* Conrad, *Astarte sulcata* Fleming, *Venus mercenaria* (very thick and heavy shells, broken but not rolled and making the bulk of the deposit), *Mactra lateralis* Say, *Mya arenaria* Linn., besides three other doubtful species.

In the above remarks I have assumed that the ice which carried the bowlders to Long Island was that of the glacier and not of detached icebergs; in other words, that the glacier did not terminate along the Connecticut shores in the early part of the Champlain era during the melting, and give off icebergs for farther southward transportation. This point, which has some arguments in its favor, I propose to consider in a future paper on the geology of Long Island. Even if the glacier had its southern limit along Southern New England, the Sound would have been filled with the floating masses, and its waters, greatly freshened by the under-glacier streams, poured in along the whole coast and by the melting of icebergs; the exclusion, therefore, of the molluscan, articulate and vertebrate life of existing species from the Connecticut coast would have been nearly or quite complete.

ART. XXXVIII.—*Corals at the Galapagos Islands*, by L. F. POURTALÈS.

THE Galapagos Islands are, as is well known, an important point in the geographical distribution of corals, being almost exactly on the boundary of the coral-producing part of the Pacific Ocean, and that portion which is destitute of them on account of the low temperature of the water. All the writers on the subject have placed this group of islands in this latter portion. During the visit of the United States Coast Survey steamer Hassler, a number of specimens of corals, of which the following is the list, were picked up on the beaches of several of the islands:

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* Ann. Lyc. Nat. Hist. of New York, viii, 149, May, 1865.

Ulangia Bradleyi Verrill, Indefatigable Island.
Pavonia gigantea Verrill, James Island.
Pavonia clivosa Verrill, Indefatigable Island.
Pavonia, sp., James Island.
Astropsammia Pedersenii Verrill.
Pocillipora capitata Verrill, Jervis and Charles Islands.
Porites, sp.

The undetermined *Pavonia* is a massive species with larger calicles than those of the two other ones, and more porous and lighter. The specimen is too much rolled for nearer determination. The *Porites* is massive. also, and in the same condition.

The species are all, or nearly all, identical with those found at Panama. They are mostly reef-builders, but here live probably isolated and at a certain depth, having never been observed *in situ*. In individual growth they are fully equal to those from more favored localities, the rolled pieces of *Pavonia* measuring six or seven inches in diameter, thus indicating masses of considerable size originally. They are not confined to the northernmost islands of the group, where we should more naturally look for them, from the greater proximity to the warm current, but as the list shows, a *Pocillipora* was found at Charles Island, one of the southernmost. The probability of fragments drifting from one island to the other is very small, owing to the considerable depth of water between them.

ART. XXXIX.—*A Comparison between the Ohio and West Virginia sides of the Alleghany Coal-field*; by E. B. ANDREWS.

[Read before the American Association, at the meeting at Detroit.]

IN the study of the Alleghany coal-field it is necessary to have some well defined geological horizon to serve as a datum line with which to collate the various strata. Sometimes the underlying formation—conglomerate in western Pennsylvania and Waverly in Ohio—is taken for such base line and measurements are made to the strata above and the coal-seams are lettered or numbered accordingly. But the conglomerate is an uncertain formation and the Waverly presents a very undulating and uneven surface, as shown by Mr. M. C. Read and myself in the Ohio Geological Reports. In this discussion, I prefer to take the horizon of the Pittsburgh seam of coal as the base of measurement.

This seam is of wide extent, being 225 miles long and about 100 miles wide, and is easily recognized by geologists. It has been asserted that this seam was formed in a trough with the Alleghany mountains forming a sloping side on the one hand and the Cincinnati uplift on the other, and as the subsidence—which is conceded to have been regular and uniform—continued, the marsh, with its accumulated vegetable matter, crept up either slope. In time the center or lowest part was buried by sediments and new land surfaces were formed on which vegetation grew and other resulting seams of coal stretched across the trough from side to side like the chords of an arc, but all coalescing with the great seam ever rising along the margins to meet them. I know of no proof whatever that the Alleghany mountains were uplifted just before the era of the upper coal-measures, nor is there any that the slope on the side of the Cincinnati uplift differed at the time the Pittsburgh seam was formed from what it had been during the era of the lower coal-measures. There is, furthermore, no proof that the upper and lower coal-measures are unconformable, as they must necessarily be if the theory controverted be true. Moreover, as a matter of fact, in Ohio, in locations which I have carefully examined, where the seams above the Pittsburgh seam are said to coalesce with the latter, there is no such coalescing, but the seams continue westward long distances beyond the points where the union is supposed to have taken place.

Disregarding, therefore, the discredit attempted to be thrown upon the great Pittsburgh seam as exceptional in its mode of formation, I shall confidently assume it as a trustworthy datum line to guide us in our investigations.

This seam occupies nearly the center of the northern and wider part of the Alleghany coal-field and extends through portions of Pennsylvania, Ohio, and West Virginia. It does not reach Kentucky, its limit of extension to the southwest being not far from the mouth of the Guyandotte river in West Virginia.

Now, if we take this seam of coal and follow around its line of outcrop and measure from it down to the base of the productive coal-measures, we find the intervals quite uniform through the larger part of the circuit. In Ohio, according to Dr. Newberry's measurements and my own, it is from 700 to 800 feet. In Pennsylvania on the Alleghany river north of Pittsburgh it is reported by Prof. H. D. Rogers to be from 600 to 700 feet; or 800 feet including the conglomerate underneath. In the northern part of West Virginia, a little south of the Pennsylvania line, it is reported to be 500 feet. But in West

Virginia farther south, the interval is far greater, as will be shown. If we make a section across the whole coal-field, including the Ohio side of the synclinal axis, taking the general line of the Kanawha river, we find the southeastern side of that axis not only very much wider territorially, but also containing a much greater thickness or depth of productive coal-measures. The Pomeroy seam of coal, which I have shown in the Ohio Geological Reports to be identical with the Pittsburgh seam, dips to the southeast under the Kanawha river a little above its mouth. A few miles higher up the river, it reappears on the other side of the synclinal axis and has been identified by Prof. W. B. Rogers and others. From this point we descend the geological series in going to Charleston, or perhaps to a point a little above Charleston, through an interval estimated to be 700 feet. Professor Fontaine, who has contributed some valuable articles to the American Journal of Science on the Geology of West Virginia, writes me that he thinks it may be greater than this. From Charleston to the Kanawha falls we descend in the series from 1,100 to 1,200 feet. I measured the interval between the coarse conglomeratic sandrock of the falls and one of the upper coal-seams on Cotton mountain, a seam which is believed to dip below the river a little above Charleston, and found it nearly 1,200 feet. Prof. Fontaine estimates 1,200 feet as the thickness of the coal-measures between the top of the conglomeratic sandrock of the falls and the southeastern outcrop of the measures up New river. A thousand feet have been measured and he estimates 200 feet for a space not measured. This makes a total of 3,100 feet from the horizon of the Pittsburgh seam to the base of the Productive measures. This is exclusive of 2,132 feet of lower shales and limestones; the Lewisburg limestone, according to Prof. Rogers, being 822 feet thick. This limestone is perhaps local, as Prof. J. P. Lesley makes no mention of finding it in his investigations in Tazewell, Russell and Wise Counties, to the southwest, where he found the lower coals and underlying Devonian rocks caught in curious lines of faults and upthrows which have brought to the surface lower Silurian limestones. But the Lower Carboniferous limestones appear in full force farther southwest in Tennessee. We have, therefore, about 2,400 feet more depth of Productive coal-measures below the Pittsburgh seam in West Virginia than in Ohio and Western Pennsylvania. My own personal explorations on the tributaries of the Kanawha, and on the upper waters of the Guyandotte and of the Tug fork of Big Sandy lead me to the belief in a corresponding thickness of coal-measures in all that portion of the State extending southwestward into Kentucky.

Perhaps the most valuable range of bituminous coals in the United States is that belt, which, beginning upon Elk river, passes the Kanawha between Charleston and Kanawha falls, includes the Coal river field, and crosses the Guyandotte, the upper Twelve Pole, and Tug and Louisa forks of Big Sandy. This belt belongs geologically to a space in the vertical series, which is below the horizon of the base of the coal-measures in Ohio and Western Pennsylvania. Yet below this group comes in another, which Prof. Fontaine designates as the Conglomerate series. If we replace the strata, now inclined on either side of the synclinal axis, in their original horizontal position, we find the proof of a vast depression of the surface in this portion of West Virginia at the beginning of the series of Productive coal-measures. It was doubtless a trough, a geosynclinal, according to Prof. Dana's nomenclature, and a part of the great Appalachian system of the wrinkling and folding of the continent. To the southeast there were at the time elevated lands of older formations, stretching along the same Appalachian line of disturbance and upheaval.

On the northern and western side of this vast basin lay the marginal plateau of the Waverly more than 2,000 feet high, with the yet higher lands of the Cincinnati uplift in the distant western background. This trough had somewhere a connection with the ocean and gradually sank below the water, while at the same time the rivers from the highlands, more or less remote, brought in sediments of various kinds. The muds and sands sometimes accumulated so as to be above the water and, during intervals of repose in the subsidence, vegetation crept out over the newly formed surfaces and formed great marshes, which, when subsequently buried, gave to mankind seams of coal. The spaces between the seams of coal were at first chiefly filled with sand which now constitutes the massive sandrocks which characterize the whole region of the lower coals of West Virginia. Sometimes the sands were coarse and gravelly and formed true conglomerates. The conditions favorable to the formation of limestone did not exist, and in the lower 2,000 feet of accumulations, I have seen, in all my extended explorations of the region, only a single limestone layer, and that in Webster County, on the head of Elk river. I had no opportunity to examine it for fossils.

When at last, in the subsidence, this basin was filled, the waters came over the marginal plateau to the north and west, a region remarkable for its stability and for the uniformity and evenness of its subsidence. At first, over the newly submerged space was spread a more or less continuous sheet of coarse sand and gravel which now constitutes the coal-measure conglomerate of Ohio and Western Pennsylvania. The continuity of the sheet is often broken and large areas around the margin of our Ohio coal-field show no conglomerate whatever. The first coal marshes grew in the hollows and depressions of the Waverly, depressions formed, it may be, wholly or in part, by erosions from surface drainage during the long period in which the formation had remained high and dry land. After the whole plateau had been submerged the same process of filling up, and marsh-forming and coal-making, went on, as has already been described, with the exception that the waters were more habitable and the layers of fossiliferous limestone show the remains of crinoid, mullusk and fish.

Less sand and more mud was brought in and shales abound, although sandstones are not uncommon, even those that are conglomeratic in character. The Pittsburgh seam of coal, which I have used as my basis of measurement, was by no means the last in the upward series. There are above it in West Virginia, probably 1,200 to 1,500 feet of coal-measure rocks, with several seams of coal. This would give from 4,300 to 4,500 feet of productive coal-measures in that favored State. No other State in the Union contains so great a vertical range.

This great Kanawha basin or trough originally extended to the south or southwest so as to include the area in which the coals of Montgomery county, Va., were formed, for these coals are doubtless the equivalents of the lowest on New river, as are also the remnants and outliers of coal in Tazewell, Russell, and Scott counties.

The subsequent upheavals and erosions have thrown great difficulties in the way of determining the course and extent of this ancient trough. I have no sections along the southeastern side of the coal-field in Kentucky—for these we must look to Prof. Shaler, now engaged in the prosecution of the Kentucky Survey; but in northern Tennessee, on the same side of the field, Prof. Safford reports about 2,500 feet of coal-measures above the horizon of the Mountain limestone. It is impossible, in the absence of sections through Kentucky, to determine whether these 2,500 feet in Tennessee were formed in a trough of equal depth with that on the Kanawha. It was doubtless approximately so, and we have here a clue to the direction of the trough to the southwest. If the Lewisburg, West Virginia, limestone is the equivalent of the Mountain limestone of Tennessee, we may infer that this formation, wherever it existed along the line of the geosynclinal depression, was carried down, while the lower Carboniferous limestone of Ohio and Northeastern Kentucky, on the northwestern side of the synclinal axis, were not involved in the movement. In southern Tennessee, Prof. Safford reports in one of his sections only about 800 feet of coal-measures above the Mountain limestone, the upper 1,700 feet having doubtless been removed by denudation. If not thus removed, the limestone was not carried down in the geosynclinal, but remained on the higher margin as in Ohio, and the original trough by which connection was had with the sea lay probably to the east.

Did the same geosynclinal trough extend in the opposite direction from the Kanawha valley, i. e., to the northeast through Pennsylvania? If the equivalency of the Pittsburgh seam has been rightly determined in the Cumberland and Broad Top coal-fields, we have in the former, according to Mr. Tyson, 1,100 feet of productive measures below that seam, and in the latter about 700, according to Prof. Lesley. Prof. Lesley has recently reported finding in the Broad Top field thin coals in the lower Carboniferous, in strata which he regards as equivalents of the Waverly Berea grit of Ohio. This is a discovery of the highest interest, but it will not effect this discussion, since I confine myself to what we term the Productive coal-measures. The geological interval between the middle of the Waverly, with the Chester limestone above, and these measures is a very great one. It may be remarked in this connection that there are doubtless areas where the lowest portions of these measures contain no register of themselves in seams of coal, the conditions of coal-making not existing, but we have so little definite information on this point that it appears to be necessary to make our limit of depth the lowest known coals of the proper coal-measures. It is quite possible, for example, that the shales, &c., between the lowest coal-seam on New River, West Virginia, and the Lewisburg limestone may somewhere contain seams of coal, but it is not best in this paper to assume the existence of such facts.

In the anthracite coal fields of Pennsylvania there is so much uncertainty as to an equivalent of the Pittsburgh seam that I fear we shall gain little or nothing from them to bear upon the question of the original depth of the basin in which they were formed. Prof. Lesquereux, in his chart of grouped sections in the fourth volume of the Kentucky Survey, places all the coal-seams of Wilkesbarre, Pittston, Scranton and Carbondale below the horizon of the Pittsburgh seam. In the Pottsville region Mr. Daddow claims that seam G. of his series, is the equivalent of the Pittsburgh seam. The seam G. is 650 feet above the lowest seam in the series, one imbedded in conglomerate. If we accept this equivalency we find no proof of the existence of the deep geosynclinal trough in that direction. If, again, the conglomerate under the anthracite basins is the equivalent of the conglomerate of Western Pennsylvania and Ohio, and they were once one and continuous as the Pennsylvania geologists affirm, then, since to the west, the conglomerate was not deposited until after the great West Virginia trough was filled and the waters in the general subsidence came over the great marginal plateau already described, we may well infer that there could not have been an extension through the anthracite region of the geosynclinal valley. There was only enough of greater depth to include the increased thickness of the conglomerate.

The comparison of the two sides of the Alleghany coal-field suggests a very great difficulty in regard to the determination of the place of what is termed the coal-measure Conglomerate. Is there an established horizon for such Conglomerate in American geology? In Ohio we now are in the habit of calling that rock the Conglomerate which, when found at all, lies upon the Waverly (although formerly the Waverly conglomerate was also thus designated) and is approximately from 600 to 800 feet below the horizon of the Pittsburgh seam of coal. The same Conglomerate extends under the bituminous coal-field of Western Pennsylvania and holds the same relation to the Pittsburgh coal. This conglomerate horizon should have the same relative distance below the Pittsburgh seam in West Virginia, and I think I have probably found lithological proof of it in a very coarse conglomerate on Elk River—a branch of the Kanawha which enters the latter at Charleston. At the Kanawha Falls is a conglomerate more than 1000 feet lower in the series. This rock has been honored with a capital C, and the coals in the 1200 feet below it are spoken of as the Conglomerate series. Still below this series and below all the coals, Prof. W. B. Rogers finds another conglomerate in the New River valley as does Prof. Lesley under the lowest coals in the faults and upthrows to the southwest. Now if the coal-measure Conglomerate is a geological horizon and not merely a rock, these various strata cannot all be the true Conglomerate. We find conglomerates everywhere in the vertical range of the coal-measures. They are found even above the horizon of the Pittsburgh seam. Heavy conglomerates abound along the western side of the eastern Kentucky coal-field, at various elevations above the lower Carboniferous limestone, and the same is true in Tennessee.

In the Indiana and Illinois coal-field, the coal seams of which have never been synchronized with those of the Alleghany coal-field, we have reported a conglomerate or millstone grit, but no one knows its exact place in the great time scale, or whether it is the equivalent of any of the half dozen well-marked conglomerates of the Alleghany field. So far as we now know it may have no equivalency any where.

In Arkansas there is a conglomerate called in the Geological Reports the Millstone grit. The available coals of the State are said to lie below it, and are therefore called the Sub-Conglomerate coals, although Prof. Lesquereux asserts that they belong to the true coal-formation and cannot be separated from it. The conglomerate in this case can have nothing more than a lithological significance.

In the South Joggins coal-field at the head of the Bay of Fundy, Sir Wm. Logan reports 14,570 feet of coal-measures. In this vast series of strata, and not very far from the middle of it, Dr. Dawson, in his admirable work on Acadian Geology, finds a group of coarse sandrocks which he denominates the Millstone Grit series. Here the millstone grit is not known to be the equivalent of the millstone grit of Great Britain, nor of any of our conglomerates of the United States.

In view of this confusion, I think all will agree with me that the terms *millstone grit* and *coal-measure conglomerate* should either be used to designate a uniform horizon in the Carboniferous system or be abandoned as geological terms and retained only for their lithological meaning.

I close this paper with another remark by way of inference, viz: that there is need of an entire revision of the classification of the coal-seams of the great Alleghany coal-field. It seems strange to begin in the middle of the vertical series and call a seam of coal No. 1, or letter A, and enumerate upward, while all the seams below are left without enumeration. But the data for such revision are not yet sufficient.

ART. XL.—*Instinct? in Hermit Crabs*; by ALEXANDER AGASSIZ.

WHILE tracing the development of one of our species of Hermit Crabs I raised from very young stages a number of specimens till they reached the size when they need the protection of a shell for their further development. I was of course curious to see how they would act the first time when supplied with the necessary shells. For this purpose, a number of shells, some of them empty, others with the animal living, were placed in the glass dish with the young crabs. Scarcely had the shells reached the bottom before the crabs made a rush for the shells, turned them round and round, carefully examining them, invariably at the mouth, and soon a couple of the crabs decided to venture in, which they did with remarkable alacrity; and after stretching backward and forward, they settled down into their shell with immense satisfaction. The crabs who were so unfortunate as to obtain for their share living shells, remained riding round upon the mouth of their future dwelling and on the death of the mollusk, which generally occurred soon after in captivity, commenced at once to tear out the animal, and having eaten him, proceeded to take its place within the shell.

It is of course very difficult to apply to Invertebrates many of the laws of natural selection, and thus far we know so little of the habits of most of our marine animals that it is idle to speculate upon the effect of causes which may effectually modify the life of higher animals. In the case above mentioned there is no possible connection between the embryo and the parent to account for the young having learned from the former the use of the shell and its value for his existence. We can, therefore, only explain the faculty of performing this act as inherited, or else as a simple mechanical act rendered necessary by the conditions of the young hermit crab. The latter seems the more probable case from the nature of the test of the hermit crab in its younger stages. While the young hermit crab soon after leaving the egg is still provided with its powerful temporary swimming feet, and while the feet of the adult can only be traced as mere rudiments behind them, the whole test of the cephalothorax and abdomen (which are symmetrical) is of considerable consistency up to the last moults preceding the stage when it seeks a shell. At that time the young are no longer symmetrical, the feet, which are now fully developed being largest on the right side and the abdomen, beginning to curve in the same direction away from the longitudinal axis. When the moult has taken place which brings them to the stage at which they need a shell, we find important changes in the two hind pairs of feet, now changed to shorter feet capable of propelling the crab in and out of the shell; we find also that all the abdominal appendages except those of the last joint are lost, but the great distinction between this stage and the one preceding it is the curling of the abdomen; its rings, so distinctly marked in the previous stages are quite indistinct and the test covering it is reduced to a mere film, so that the whole abdomen becomes of course very sensitive. It is therefore natural that the young crab should seek some shelter for this exposed portion of his body, and from what I have observed, any cavity will answer the purpose; one of the young crabs having established himself most comfortably in the anterior part of the cast skin of a small isopod, which seemed to satisfy him as well as a shell, there being several empty shells at his disposal. This mechanical explanation still leaves unanswered the eagerness with which the crabs rushed for the shells, their careful examination of its openings, their taking the animal out and occupying his place; all acts which seem to require considerable intelligence?, and to show remarkable forethought?.

Newport, Aug. 23, 1875.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the Constitution of Ammonium Compounds.*—In their previous paper,* **MEYER** and **LECCO** established the identity of the two di-methyl di-ethyl derivatives of ammonium, whether obtained from di-methylamine and ethyl iodide on the one hand, or from di-ethylamine and methyl iodide on the other, by the complete equality of their physical properties, and of their chemical reactions in solution, by the identity of their decomposition-products at high temperatures, the equal solubility of their platinum salts, which were identical in crystalline form, and by the exact similarity of fusing point of their picrates. To these particulars, they now add the crystallographic identity of their picrates as determined by Groth and Arzruni. Lossen having objected that the conclusion drawn from the above facts—i. e., that nitrogen was a pentad in these compounds—was not warranted unless it could be shown that no exchange of radicals took place within the molecule, the authors have made a series of experiments to test this question. By acting on tetra-methylammonium iodide with ethyl iodide, there would result, if an exchange did actually take place, the following reaction:

$$N(CH_3)_4I+C_2H_5I=N(CH_3)_3C_2H_5I+CH_3I.$$

The two substances were heated together in a sealed tube, first to 100° and then to 150° (the original reaction having been completed at 80°); but no trace of any reaction could be detected. The same result was obtained when methyl iodide was heated with tetrethylammonium iodide to 70°, to 150, and even to 180° (the previous experiments having been conducted at ordinary temperatures). The authors hence reiterate their conclusion that the ammonium derivatives are not molecular but atomic compounds and that in them, the nitrogen is quinquivalent.—*Ber. Berl. Chem. Ges.*, viii, 936, July, 1875. G. F. B.

2. *On Retene.*—Among the products obtained in the destructive distillation of wood, is a heavy oil often employed in lubrication. This oil is very rich in the hydrocarbon retene, and **EKSTRAND** has extracted this substance from it and submitted it to an investigation. On pressing the cooled oil, a grayish soapy mass is obtained having the odor of creosote and melting at 84°–85°. After washing with ether, solution in boiling alcohol, decolorization with animal charcoal and crystallization, pure retene is obtained in white micaceous scales, fusing at 98°·5 and having the composition $C_{18}H_{18}$. Crystallized retene has a density of 1·13, and fused of 1·08. It is readily soluble in boiling alcohol and glacial acetic acid, and in carbon disulphide, benzene, and ether. It unites directly with chlorine and bromine at ordinary temperatures and is readily oxidized by nitric acid. The picrate crystallizes

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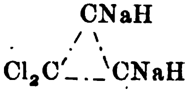
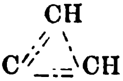
* This Journal for June, 1875, p. 462.

in orange needles. Di- and tetra-bromo-retene, and a sulpho-conjugated acid and its salts, are described. In acetic acid solution, chromic acid oxidizes it to dioxyretistene $C_{16}H_{14}O_2$ and to two other bodies, $C_{16}H_{16}O_3$ and $C_{18}H_{17}O_2$, both monobasic acids, forming well defined salts. Dioxyretistene reduced by zinc powder, yields dibenzyl. Hence he regards the retistene of Wahlforss as probably only a mixture of retene and dibenzyl.—*Bull. Soc. Ch.*, II, xxiv, 55, July, 1875. G. F. B.

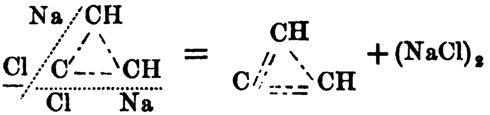
3. *On Propargylene, a new hydrocarbon, C_3H_2.*—By the action of strong bases on croton-chloral, dichlorallylene, $C_3H_2Cl_2$ is obtained. PINNER has now observed that sodium is capable of uniting directly with this body, and that the product when decomposed by water, yields a gas having the composition C_3H_2, which he calls propargylene. If fragments of sodium be placed in dichlorallylene at ordinary temperatures, a slight almost imperceptible evolution of gas takes place, but in the course of half an hour, the sodium sinks to the bottom, having been converted into a brown dull mass, with a notable increase of volume. Finally, when somewhat less than a molecule of sodium has been added to one of dichlorallylene, the latter has entirely disappeared, the dry mass being the sole product. Water, alcohol, and ether decompose it, evolving torrents of gas and forming sodium chloride. It has not been obtained pure and its analyses are discordant; but its mode of formation gives it the probable formula $C_3H_2Cl_2Na_2$. If it be treated with water and the evolved gas be passed through bromine, this is absorbed, and a heavy oil is obtained, having the formula $C_3H_2Br_4$. By the action of potassium hydrate upon this, tribromallylene C_3HBr_3 is obtained and this unites directly with Br_2 to give C_3HBr_5 in white crystals. Propargylene gas passed through ammoniacal silver solution, gives a precipitate of fine white needles, which rapidly blacken even in the dark, and have the probable composition $(C_3H_2)_2Ag_2O$. Pinner regards the sodium compound as composed thus:



, and propargylene thus:



; and the decomposition of the former by water is represented as follows:



Hence this dichlorallylene is not a derivative of allylene proper, but is propargylene dichloride.—*Ber. Berl. Chem. Ges.*, viii, 898, July, 1875. G. F. B.

4. *Synthesis of a dextro-rotatory Malic acid.*—BREMER has published a preliminary note on a new malic acid which rotates the polarized ray to the right, produced from dextro-rotatory tar-

taric acid. Ordinary tartaric acid, iodine, phosphorus and a little water, were enclosed in sealed tubes and heated in a water-bath for several days. The mass was treated with water, the succinic acid crystallized out, the mother liquor diluted and the phosphoric and unattacked tartaric acid separated by milk of lime. Ammonium nitrate was added and the solution evaporated. On the addition of alcohol, calcium malate and succinate were deposited. They were converted into lead salts, the lead separated by H_2S, and the acids obtained by evaporation, were separated by the deliquescence of the former. Recrystallization of the acid ammonium salt afforded a pure malate, having the same form as the natural salt and fusing at 170° C. Examined with the Polaristrobometer of Wild, the salt was found to rotate to the right. The author is now engaged in the attempt to produce a left-handed malic acid from lævotartaric acid, in the hope that from the two an inactive malic acid may result, as racemic comes from the two tartaric acids.—*Ber. Berl. Chem. Ges.*, viii, 861, July, 1875. G. F. B.

5. *On Ethyl and Amyl-sulphocarbonates as remedies for the Phylloxera.*—Dumas, having shown that potassium sulphocarbonate, when mixed with the soil, evolves hydrogen sulphide and carbon disulphide, recommended this substance for destroying that pest of the grape culture, the phylloxera, since it was well known that carbon disulphide was rapidly fatal to them. Moreover, he showed that the hydrogen sulphide evolved simultaneously, did not, under these circumstances, seriously injure the grape plants, being rapidly oxidized in the soil. Experiments made by Zöller and Grete at the Agricultural High School of Vienna, have confirmed the excellence of potassium sulphocarbonate for this purpose; but they have also shown the equal usefulness of another compound, which, while at the same time evolving in the soil the noxious carbon disulphide, does not set free the injurious hydrogen sulphide. Moreover, while the former substance recommended by Dumas is difficult to prepare and is consequently high in price, the new compound can be made with facility and at a moderate price. This new body is potassium ethylsulphocarbonate or xanthate. A solution of this salt mixed with the soil, evolves CS_2 copiously. It is however better to mix the dry salt with a small quantity of earth and of superphosphate, and to scatter the dry powder about (or better directly upon) the roots. In this way, both the potash and the phosphoric acid become useful finally to the plant.

In a subsequent paper, the same authors give the results of some experiments on the action of the xanthate on plants. They found that even tender succulent plants growing in only half a liter of earth, could be treated with a gram of the above salt, with no injurious effect other than the loss of a few leaves. Shrubs were entirely unaffected when treated with from 3 to 5 grams. They also reply to Dumas's statement that the xanthate is too costly in France to be used practically, owing to the high price of alcohol, supposing the fused potassium hydrate out of the question. They would replace the ethyl by amyl, forming thus the amylsulphocarbonate. This they find by experiment is equally efficacious with the other, has even less injurious action on plants and is far cheaper. Moreover, it may be made easier. If concentrated potassium hydrate solution be agitated with crude amyl alcohol, and carbon disulphide be added, the whole mass becomes warm and deposits potassium amylsulphocarbonate, in a form best suited for use. It may be employed either in solution or better mixed with superphosphate. As to cost, the authors estimate that at the present cost of amyl alcohol (fusel oil) in Vienna, it can be made for $15 a hundred weight. It is not only useful to exterminate the phylloxera, but also many other root parasites of plants.—*Ber. Berl. Chem. Ges.*, viii, 802, 955, June, July, 1875.

G. F. B.

6. *On the occurrence of Ethyl Alcohol in Plants.*—The occurrence of ethyl alcohol in the unfermented juices of plants is rendered probable by the fact that these juices contain not only acetic acid, its oxidation product, but also its homologues, methyl, hexyl, and octyl alcohols. GUTZEIT has made, in Geuther's laboratory, a somewhat extended investigation of this subject, using for the purpose *Heracleum giganteum*, *Pastinaca sativa*, and *Anthriscus cerefolium*. Six and a quarter kilograms of the fruit of the first plant being distilled with 18 kilograms of water, gave 12 kilos. of a distillate, from which the oil on the surface was separated by means of a syphon. The residue on distillation gave 12 to 15 grams of volatile liquids, which after rectification boiled between 72° and 77°, and consisted of methyl and ethyl alcohols, the latter consituting two thirds. By examining the fruit at various stages of growth, the author infers that as ripening approaches, the ethyl compounds gradually diminish, being converted into more condensed bodies; while the methyl compounds remain constant. The other plants mentioned gave a similar result.—*Liebig's Annalen*, clxxvii, 344, June, 1875.

G. F. B.

7. *On Arbutin.*—HLASIWETZ and HABERMANN have made a research upon arbutin, a glucoside extracted by Kawalier from the *Arctostaphylos uva-ursi* or bearberry. Its discoverer observed that it was easily split into glucose and a body which he called arctuvin, but which Strecker asserted to be hydroquinone. The authors find, however, that the body thus obtained is not pure hydroquinone, but is a mixture of this and its methyl derivative, methyl-hydroquinone $C_6H_4 \left\{ \begin{matrix} OCH_3 \\ OH \end{matrix} \right.$ isomeric with saligenin $C_6H_4 \left\{ \begin{matrix} CH_2OH \\ OH \end{matrix} \right.$. Hence they assign to arbutin the formula $C_{25}H_{34}O_{14}$ and express its splitting by ferments, as follows:

$$\underset{\text{Arbutin.}}{C_{25}H_{34}O_{14}} + \underset{\text{Water.}}{(H_2O)_2} = \underset{\text{Hydroquinone.}}{C_6H_6O_2} + \underset{\text{Methyl-hydroquinone.}}{C_7H_8O_2} + \underset{\text{Glucose.}}{(C_9H_{12}O_6)_2}$$

One hundred parts of arbutin yield 19·7 hydroquinone, 22·5 methyl-hydroquinone and 64·7 sugar.—*Liebig's Annalen*, clxxvii, 334, June, 1875.

G. F. B.

8. *A new Base obtained in the Aniline manufacture.*—JACKSON has examined in Hofmann's laboratory a new base obtained from the last run of the still in the manufacture of aniline. This product—a black tarry liquid with a disagreeable odor—was dissolved in hydrochloric acid, diluted and filtered. Upon addition of sodium hydrate, the bases were set free as a light oil swimming on the surface. This was fractionated, and the portion boiling between 280° and 320°, separated from naphtylamine by sulphuric acid, gave a black oil, which converted into the nitrate and purified, was proved to have the composition $C_{13}H_{13}N$ or $C_{13}H_{11}NH_2$, a primary amine. Its nitrate, sulphate, and chloride crystallize well. Farther investigation showed it probably to be tolyl-phenyl-amine, $(C_7H_6.C_6H_5.)NH_2$.—*Ber. Berl. Chem. Ges.*, viii, 968, July, 1875. G. F. B.

9. *Viscosity of Saline Solutions.*—M. A. SPRUNG has repeated the experiments of Poiseuille to determine the relation of the viscosity of liquids to their temperature and chemical composition. A volume of 21·15 cms.^{3} of the liquid was forced through a capillary tube 300 mms. long, and ·46 mms. in diameter, under a pressure of 158 cms. of water. Observations were made with solutions of chloride of ammonium of five different strengths, at intervals of temperature of 5°, from 0° to 60°. A graphical representation shows that the curve of viscosity of the liquid cuts that of water at a temperature as much greater as the solution is stronger, and that at low temperatures the viscosity is less, at high temperatures greater, than that of water. The same result was shown by eight other salts.

By varying the acid it appears that salts of potash, soda and ammonia arrange themselves in the following order with the acids: H_2SO_4, HCl, HNO_3, $HClO_3$, HB_2 and HI. In each of the three series the viscosity diminishes from sulphuric acid to iodhydric acid. The acids remaining the same, these salts stand in the order NaOH, KOH and NH_4OH, in which the soda salts flow most slowly and the ammonia salts most rapidly. Sulphate of ammonia, however, flows more slowly than sulphate of potash.

If we compare the velocity of flow, of salts of similar constitution, we find that at all temperatures the viscosity of the solution is as much greater with the same base as the molecular weight of the acid is less, and with the same acid as the molecular weight of the base is less.

The viscosity of the same solution of sulphate of oxide of chromium is different according as the salt belongs to the violet or green modification, being greater in the first case than in the second.—*Bib. Univ.*, ccx, 112. E. C. P.

10. *Photographic Irradiation.*—Capt. ABNEY states that the commonly received view, that the increase in the apparent size of the image of a luminous object when photographed, is due to simple reflection from the back of the plate, is quite untenable. If examined under a microscope, it will be seen that the film consists of minute particles separated by considerable distances from one

another. Their diameter is several times the length of a wave of light, and hence we may treat the question as a case of simple geometrical reflection. Computing by Fresnel's formula the amount of light reflected by the collodion and glass at various angles, we find the maximum amount of light would fall at a distance $2t \cot v$, in which t is the thickness of the glass, and v the critical angle. On development, therefore, a ring might be formed around the image with this distance as a radius, and shading off more abruptly inside than outside. To test these results an image of the sun was formed by a lens of 6″ focus on a plate ·12″ thick. A well defined annulus was formed shaded as described above. With a plate of double thickness, the mean diameter of the annulus was doubled. Two plates were then employed separated by a thin card. A ring was formed on the first plate and a diffused image on the second. A measurement of the intensities agreed closely with the result of theory. Holes and slits were cut in platinum foil, placed on a dry plate and exposed directly to sunlight. Rings were obtained in one case, and parallel lines joined by semi-circles in the other, showing that the effect is not due to the lens of the camera.

Three methods suggest themselves for diminishing irradiation: first, to coat the back of the plate with some non-actinic color; secondly, to increase the opacity of the film; thirdly, to connect the particles of bromo-iodide of silver with a body of as nearly as possible the same density. The first is the usual method adopted by photographers in dry plate processes. This diminishes the irradiation from the back of the plate, but does not completely eliminate it, as will be understood when the nature of the resulting reflection is considered. The second method is easily carried out, and is more effective than the first. With albumen preservative particularly, the amount of irradiation is reduced materially, even though the film appears nearly transparent.—*Phil. Mag.*, l, 46. E. C. P.

11. *Conical Refraction.*—M. NODOT has endeavored to find a substitute for the crystal of arragonite commonly employed to show the phenomena of double refraction. The rarity of crystals of sufficient thickness of this substance, has led many physicists to improve the optical conditions so as to enlarge the angle of the exterior cone; but the field is still open, to find natural or artificial crystals capable of rivaling arragonite in specific refractive energy, thickness and transparency. Judging by the values of the three indices of numerous biaxial crystals, arragonite will be the most favorable mineral; but chemical substances are so numerous that we may hope to find crystals superior in these respects.

Three substances appear capable of replacing arragonite, sugar, bichromate of potassium and tartaric acid. The method of cutting the two first requires no trial, for a natural face of the sugur and a cleavage face of the bichromate are normal to one of the optical axes. For tartaric acid it is not the same, and the direction of the faces through which we shall see one of the axes must be found by trial. We succeed easily by employing an Amici's microscope and are recompensed by the result, as crystals of equal thickness give cones twice as open as those of arragonite.—*Journ. Phys.*, iv, 166. E. C. P.

12. *Dielectric Capacity of Gases.*—M. L. Boltzmann has endeavored to determine this constant experimentally for gases, in order to determine its probable important relation to the index of refraction. According to the electromagnetic theory of light, of Maxwell, the square root of the dielectric capacity of a transparent medium should be equal to the index of refraction for rays of large wave-length. The capacity was determined by a condenser whose plates were separated by the gases to be tested, the potential being measured by a Kirchhoff's lecture electrometer, and a battery of 300 small Daniel cells being employed.

In the following table the first column gives the name of the gas, the second the square root of the dielectric constant, and the third the index of refraction:

Gas.	$\sqrt{k}$.	n.
Air	1·000295	1·000294
Carbonic acid	1·000473	1·000449
Hydrogen	1·000132	1·000138
Carbonic oxide	1·000345	1·000340
Nitrous oxide	1·000497	1·000503
Olefiant gas	1·000656	1·000678
Marsh gas	1·000472	1·000443

—*Pogg Annal.*, clv, 403. E. C. P.

13. *The human body rendered luminous by Phosphuretted Hydrogen.*—Dr. Geo. Maclean of Princeton, in a private communication to the editors, says:

"Several years ago, after spending a portion of the day in experimenting with this gas (PH_3) prepared from phosphorus and solution of potash, on retiring to bed I found my body quite luminous, from a glow like that of phosphorus exposure to the air. Either some of the gas escaping combustion, or the product of its burning must have been absorbed into the system, and the phosphorus afterwards separated at the surface have there undergone eremecausis. I was conscious of no feeling that could be attributed to it, nor was my health apparently in any way affected by it."

Probably a repetition of the trial would reproduce the results observed by Dr. Maclean, which have apparently escaped notice heretofore because unsought.

II. Geology and Natural History.

1. *Pseudomorphism and Metamorphism: a correction;* by James D. Dana.—In my notice of Mr. Hunt's Essays in this Journal, volume ix (the number for Feb., 1875), I say, on page 109: "With the exception of the year 1858, I have never held

nor taught that "metamorphism is pseudomorphism on a broad scale." I would here correct the statement by dropping the exception, so as to make it read simply—

"I have never held that metamorphism is pseudomorphism on a broad scale."

When the sentence on page 109 was written I had the strongest personal conviction that I had never entertained this view as a broad general principle, notwithstanding the fact that the sentence stands in a book-notice of mine in this Journal for 1858. For, in 1871, when I first knew that the doctrine was attributed to me, I had no recollection of the sentence, or of passing through such a phase of belief. In addition, my manuscript lectures, which were written out in full for my geological course in 1854, and which were delivered in 1858, as they had been before and were subsequently, do not contain such a principle, any more than does my Manual of Geology, published in 1862, in which those lectures were embodied.

I can refer now to published evidence that the broad principle was never in my mind; and this evidence is afforded by the book-notice (xxv, 445, 1858) which contains the statement. I there—after expressing my confidence in Bischof's views respecting the "agency of alkaline silicates in pseudomorphism and metamorphism," and a reference to papers in volumes xlv, xlvii and xlviii (1843–1845) of this Journal, where I had advocated them—say as follows (the words in italics are those to which attention is directed):

"*On page* 92 *of the same paper* [that of vol. xlviii, 1845] metamorphism is spoken of as pseudomorphism on a broad scale."

The expression here cited certainly implies that the doctrine referred to had been taught by me as early as 1845. But on the page referred to there is no such general statement, in fact no general statement on the subject. Only the case of magnesian silicates is mentioned, besides that of silica;* and such silicates I have ever regarded as affording examples of pseudomorphism on a broad scale.† The introduction of an omitted word, so as to make the sentence read—

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* The paragraph on page 92 is as follows:

"The importance of these discussions [on pseudomorphism] is not to be measured by the length of the catalogue of pseudomorphs. The same process that has altered a few crystals to quartz has distributed silex to fossils without number scattered through rocks of all ages. The same causes that have originated the steatitic scapolites occasionally picked out of the rocks have given magnesia to whole rock formations and altered throughout their physical and chemical characters. If it be true that the crystals of serpentine are pseudomorphous crystals altered from chrysolite, it is true also, as Breithaupt has suggested, that the beds of serpentine containing them are likewise altered, although often covering square leagues in extent. The beds of steatite, the still more extensive talcose formations, contain everywhere evidence of the same agents. The deposits of the so-called Rensselaerite [a variety of the magnesian mineral, talc] in Northern New York are other examples of the widely extended influence of the pseudomorphic agencies."

† The "extensive talcose formations" are now excepted, as they have been proved not to be magnesian or talcose.

"On page 92 of the same paper, metamorphism is spoken of as *sometimes* pseudomorphism on a broad scale"—
places it in harmony with my knowledge of my own scientific opinions and the statements in my various mineralogical and geological writings from the first to the last.

2. *On Triarthrus Beckii, supposed to have been found in a boulder in the Connecticut Valley;* by W. W. Dodge. (Letter to J. D. Dana, dated Oct. 22, 1873.)—Having seen last evening, in the October number of the American Journal of Science, the statement of your views there presented as to the age of the "Taconic" rocks, I communicate the following circumstance for what it is worth.

I saw recently in a friend's collection a cast labelled "*Triarthrus*—Connecticut Valley." The collector of the cabinet in which I found it died some years ago, and the present possessor can give no information as to the original. This may be from some well-known locality of whose existence I am ignorant, but as I saw at the same time numerous rock specimens labelled as from various towns in Western Massachusetts, I am inclined to think the cast is from a specimen owned by some friend to whom I have not access; and if really found in the Connecticut Valley, probably an erratic.

From what I have observed as to the distance at which boulders are usually found from the place where the rock was *in situ*, the presence of this fossil anywhere in the Connecticut Valley seems to me interesting.

3. *Forms and Origin of the Lead and Zinc Deposits of Southwestern Missouri;* by Adolf Schmidt. (Trans. St. Louis Acad. Sci.)—The following is an abstract of this important paper:

These deposits are mainly in the Keokuk group (Subcarboniferous)—a group consisting of thin-bedded chert, or of limestone, or of chert and limestone interstratified, or of a calcareous chert called *silico-calcite*. The rocks are much altered and fissured and partly rendered magnesian, in the vicinity of the ores, and the cavities containing the ores appear to have been made when these alterations took place.

The lead and zinc ores of the "runs"—deposits of one horizontal direction not over 5,000 feet in height and width—are always associated with dolomite, and were evidently formed along fissures in a layer of limestone limited above and below by a chert layer, the rock adjoining which had been dolomized by "solutions of bicarbonate of magnesia;" the rock became cracked at the same time from the accompanying diminution of volume and made spaces for the ores. Crystals of galena isolated in the dolomite show that part at least of it was deposited in the rock "while the latter was in a magmatic condition."

In another kind of deposits called "openings," the ores occupy spaces which extend from a chert bed downward, but not through the limestone layer; they show no evidence of origin about vertical fissures, but rather from horizontal openings below a chert bed.

Again fissured chert beds are impregnated with lead and zinc ores—mostly sulphides—in the vicinity of the runs and openings, or else alone.

In still another variety of deposit, common in the Joplin district, galena occurs in loose accumulations of broken chert; and in part of Joplin creek valley the accumulations have a depth exceeding 70 feet. The ore appears to have been formed in the fissures of the chert beds, and, afterward, the chert beds were broken up through undermining by the removal of the limestone. The galena extends into the masses of chert and sometimes also is crystallized on their surfaces, as if its deposition had commenced before the breaking up of the chert but was continued during its progress.

There are also seams and crystals of galena and blende imbedded, or occupying pockets, in quartzyte, a gray to brownish and often bituminous variety. The quartzyte in such cases contains also irregular crystals of dolomite; and the cavities left by the removal of this dolomite are often filled with carbonate of lead or silicate of zinc.

These various deposits have evidently been very slowly formed through a long period of time, and the larger part after the partial dolomization of the beds along vertical and horizontal fissures, this making the spaces for the ores. There are no true veins. The layers of rock in which the ores occur lie only from 40 to 120 feet below the surface. Whether lead deposits exist below has not been ascertained. Exploration thus far is unfavorable to it.

4. *Carboniferous Conifers.*—Little attention seems as yet to have been given to the remains of Coniferous trees found in the Carboniferous rocks of the United States. In Nova Scotia several species are known, and are to some extent characteristic of definite horizons. In the Carboniferous sandstones of the United States such remains seem to be frequent; but I have seen no detailed account of them, and the only well characterized specimens which have come into my hands are portions of two trees from Ohio kindly sent to me by Dr. Newberry, and a very finely preserved fragment from Mazon, Grundy Co., Illinois. Both of these show the characteristic structure of *Dadoxylon* (Unger), *Araucaroxylon* of Schimper's recent work, and are closely allied to *D. materiarium*, the common species of the upper and middle coal formations of Nova Scotia. The specimen from Illinois is probably identical with that species. One of these from Ohio presents some peculiarities in the structure of the medullary rays, which may indicate a distinct species.

The subject is deserving of the attention of microscopists in the coal districts, as there can be little doubt that several interesting species remain to be discovered, and other kinds of structures; as for example the curious *Dictyoxylon* of Williamson, found also in Nova Scotia, would very possibly reward patient slicing of trunks showing structure.

The Devonian has also treasures of the same kind. In the

United States it has already afforded *Dadoxylon Halli* from New York, and *D. Newberryi* from Ohio, besides the curious *Ormoxylon Erianium.* No doubt other species remain to be discovered, especially in the Upper and Middle Devonian.

The writer of this note would willingly correspond with any one engaged in such researches, as he has now under examination a number of specimens from different parts of British America, and would be glad to have opportunities of comparison with those of the United States. J. W. DAWSON.

McGill College, Montreal.

5. *Fossil Trees of Craigleith Quarry, Scotland.*—Six trunks of large trees have been obtained at Craigleith quarry since 1826. The largest, 36 feet long and 12 to 14 feet in girth, has been taken to the British Museum, and is to be set up erect there. Another is nearly 30 feet long and has been removed to the Botanic Garden. The trees were Conifers. The surface of each is bituminous coal, varying from one-twentieth of an inch in thickness to two inches. The trunks inside of this coaly exterior consist of carbonate of lime, carbonate of magnesia, carbonate of iron and free carbon in varying proportions. The coaly exterior is attributed, by Sir R. Christison, to bitumen passing to the surface as the destructive distillation of the wood was going on within.—*Proc. Roy. Soc., Edinburgh*, Jan., 1874.

6. *Volcanic History of Ireland;* by Prof. EDW. J. HULL (Roy. Soc., Ireland, vol. iv, part I).—Professor Hull describes as of *Lower Silurian* age, younger than Cambrian, sheets of "felstones" and porphyries of Wicklow, Wexford and Waterford, accompanied by beds of ash and volcanic breccia, interstratified with Lower Silurian slates and grits; also similar beds in the valley of the Boyne which become less frequent in the districts of Down, Armagh and Cavan. They are the representatives of the felstones and porphyries of North Wales, described by Sedgwick, Murchison and Ramsay, and those of Cumberland.

The Irish *Upper Silurian* contains igneous beds in the mountainous region of West Mayo and Galway; felstones, etc., along the western shores of Lough Mask; beds of ash, volcanic conglomerate and some of trap on the Promontory of Dingle, the thickness, according to Jukes, 2,500 feet, and interstratified with fossiliferous Wenlock beds.

The Irish *Devonian*, in the Killarney Mountains south of Lough Guitane, where a bold rock, 1490 feet high, a mass of columnar felstone, "indicates the position of an old 'neck' or volcanic throat, from either side of which, for considerable distances, beds of feldspathic ash and conglomerate, with large 'balls' of felstone, often hollow in the center, which were probably bombs shot out of the vent at various stages of the eruption." These beds of ash re-occur at Lough Garagarry, Lough Managh and the Devil's Punchbowl, and rise into the summit of Crohane (2,162 feet) and the southern slopes of Killeen Mountain. To the south, there is another district containing cotemporaneous trap rocks at the mouth of Kenmare River. The shores of Lough Kay and Valentia Harbor are "another volcanic center of eruption during the period," beds of felstone, greenstone, ashes and conglomerates being there associated with those of the Old Red Sandstone and intersecting the latter in dikes.

In the *Lower Carboniferous*, occur the trap rocks of the Limerick basin, cotemporaneous with the toad-stones of Derbyshire, the trap and porphyrite ashes of the central valley of Scotland forming the Kilpatrick, Campsie and Dalry Hills. These Limerick trap rocks are of two times of eruption, the earlier being the most important. The trap is chloritic and contains chrysolite. Another district is that at the entrance of Bantry Bay, where the rocks are felstones, greenstones, beds of ash, breccia and conglomerate.

No volcanic ejections of the Permian or of the Mesozoic periods occur. The next in age and last are those of the Miocene Tertiary. They occur over the area of northeastern Ireland, and extend thence to the west of Scotland and the inner Hebrides. The area in Ireland is 2,200 to 2,300 square miles. These rocks are briefly described, from an article by Professor Hull, in the last volume of this Journal, at page 147.

7. *Asphaltic Coal from the shale of the Huron River, Ohio ;* by Prof. A. R. LEEDS.—In the number of the Annals of the Lyceum of Natural History of New York for June, Professor Leeds describes a coaly substance from Ohio, resembling much the asphaltic coal or albertite of New Brunswick. The seam averages two inches; and it is traversed by innumerable sheets of sulphate of baryta.

Dr. J. S. Newberry adds a note to the paper stating that there are many localities of similar material in Ohio and Kentucky; and that it occurs in fissures of the Huron shale and was formed from distilled petroleum, like the albertite and grahamite. He observes that this shale, "which is the equivalent of the Portage group of the New York geologists," contains throughout from 10 to 25 per cent of carbonaceous matter, and is the source whence most of the oil is derived both in Pennsylvania and Ohio.

8. *Eruption of Tridimitic Ashes.*—Dr. A. BALTZER, of Zurich, has announced that the Island Volcano, one of the Lipari group, ejected, on the 7th of September, 1873, a whitish ashes for three hours, which formed a deposit over the interior of the island, in some parts three to four centimeters deep, which consisted of silica in the state called tridimite. He found the material to agree with that species in specific gravity, degree of solubility in alkali, action of polarized light and other respects. His announcement was made before the Society of Natural History of Zurich, on the 4th of January, 1875.—*R. Com. Geol. d'Italia*, 1875, 197.

9. *Exploration of the Colorado River of the West, and its Tributaries*, explored in 1869 to 1872 under the direction of the Secretary of the Smithsonian Institution; by Prof. J. W. POWELL. 292 pages, 4to, with many plates, 8 maps.—The Smithsonian Institution has shown, by this volume of Professor Powell, that

Congress did well in placing this exploration of the Colorado under its direction. The volume has been, in part, foreshadowed by the magnificent photographs of the Colorado region, by the artist of the expedition, which have been widely distributed through the country. But the descriptions of Professor Powell, and the new birds,eye views and sections inserted alongside of the photographs which are reproduced in admirable style, have added ten-fold to the instruction from the latter. The work is to a large extent a treatise on denudation, or the making of valleys—and of mountain forms, too, by circumdenudation—and never was the subject more appropriately and grandly illustrated. The author divides the valleys of the region into two classes: 1st, the *Transverse*, running across the strike, and, 2d, the *Longitudinal*, running with the strike. Under each class he makes three kinds. Of *Transverse:* (*a*) those which pass through a fold, the *diaclinal;* (*b*) those running in the direction of the dip, the *cataclinal;* (*c*) those running against the dip, the *anaclinal*. Of *Longitudinal:* (A) Those which follow anticlinal axes, the *anticlinal;* (B) those which follow synclinal axes, the *synclinal;* and (C) those which follow the direction of the strike between the axes of the fold, the *monoclinal*. The subject of faults in the Colorado region is fully illustrated. Many readers of Professor Powell's article in vol. v, of this Journal (p. 456, 1873) were no doubt unsatisfied for the want of sections. These are here fully supplied.

The volume also gives much information on the Indians of the Colorado region. It closes with a chapter of 65 pages on the genera *Geomys* and *Thomomys* by Dr. Elliott Coues, U. S. A. There is also an Addendum on the Salamander of Florida (*Geomys tuza*), by Prof. G. Brown Goode, the direct connection of which with the subject of Professor Powell's Report is not apparent to us.

10. *Report of the Geological Survey of Ohio.* Vol. II, *Geology and Paleontology.* Part I, GEOLOGY. Officers of the Survey, J. S. NEWBERRY, Chief Geologist; E. B. ANDREWS and EDWARD ORTON, Assistant Geologists; F. G. WORMLEY, Chemist; F. B. MEEK, Paleontologist. 702 pp. 8vo, with plates, wood-cuts, and colored geological county maps. Columbus, Ohio. Published by authority of the Legislature of Ohio.—The survey, of which this volume is one of the series of Reports in course of publication, was carried forward during the years 1869 to 1874. The volume just issued is the second on the Geology of the State, although this might not be inferred from the ambiguous title-page. The first was issued two years since and is noticed in this Journal in vol. vi. The new volume commences with chapter xxx. The title-pages of the two volumes are intended to indicate—not "volume I, Geology and Paleontology, Part I, Geology," and "volume II, Geology and Paleontology, Part I, Geology," as they read—but Part I, Geology, vol. I, and Part. I, Geology, vol. II. A third volume, vol. III, will finish the Part I, Geology; and two volumes more will complete Part II, Paleontology, volume I having been published in 1873.

The Reports, as we have before said, are very valuable contributions to geological science, and as important to the economical interests of the State The paleontological volumes, if published with all the plates, according to the plan proposed, will redound to the honor of the State, and contribute to the progress of knowledge throughout it, if bringing no return to its treasury.

The present volume commences with a chapter on the Quaternary, or, as it is here called, Surface Geology, by Dr. Newberry —a chapter which has been noticed in the last volume of this Journal. Dr. Newberry has, next, valuable chapters on the Carboniferous System of Ohio and on the Geology of Erie and Lorain Counties. The succeeding chapters on Ottawa, Crawford, Morrow, Delaware, Van West, Union, Paulding, Hardin, Hancock, Wood, Putnam, Allen, Anglaize, Mercer, Henry and Defiance Counties are by N. H. Winchell; those on the Surface Geology of Southeastern Ohio, on Washington, Noble, Guernsey (southern half), Belmont (southern half), Monroe, Pickaway and Fairfield Counties, by E. B. Andrews, who has devoted much time to the coal formation; and those on Pike, Ross, and Greene Counties, concluding the volume, by Edward Orton.

The facts in these Reports have in part appeared in the Annual Reports published in the course of the survey, and have been the subjects of notices in this Journal. Without further discussion of their contents, we repeat that they are all worthy of thorough study. The geological maps of the counties, inserted in the volume, are well colored.

11. *Sixth Annual Report of the Geological Survey of Indiana made during the year* 1874; by E. T. Cox, State Geologist; assisted by Prof. JOHN COLLETT, Prof. W. W. BORDEN and Dr. G. M. LEVETTE. 288 pp. 8vo, with maps and plates.—This volume contains a Report by the State Geologist, Mr. Cox, on the Quaternary of Indiana, some of its economical products, its Indian antiquities, and the Geology of Jackson County, with a Geological map; a Report on the Geology of Brown County, with a map, by Prof. Collett; on the Geology of Scott and Jefferson Counties, with maps, by Prof. Borden; on the Sisco of Lake Tippecanoe and its relatives, by Prof. D. S. Jordan, M.D., with a wood-cut of *Argyrosomus sisco* Jordan; a synopsis of the genera of Fishes to be looked for in Illinois, by Prof. Jordan; and a partial list of the Flora of Jefferson Co., by J. M. Coulter.

Mr. Cox gives the following list of fossil shells found in the lœss near New Harmony: *Macrocyclis concava* Say, *Zonites indentatus* Say, *Patula perspectiva* Say, *Helix lineata* Say, *H. labyrinthica* Say, *H. hirsuta* Say, *H. monodon* Rack., id. var. *fraterna*, *Punctum minutissimum* Lea, *Succinea elongata* Say, *Pupa armifera* Say, *Helicina occulta* Say, *Cyclostoma lapidaria* Say. The upper surface of the lœss is 165 feet above the Wabash river in Posey Co., and in Perry Co., on the Ohio, about 200 feet.

The "Black shale," the formation which overlies the Corniferous limestone, has afforded Prof. Borden, at Lexington in Scott

Co., the following fossils—identified by R. P. Whitfield: *Leiorhynchus quadricostata* Hall, *Chonetes lepida* Hall, *Tentaculites fissurella* Hall, a fragment of a *Cardiola* near *C. radians*. The *Leiorhynchus* is a Genesee shale species; the *Tentaculites* is found as well in the Marcellus and Hamilton beds; the *Chonetes* belongs to the Hamilton and possibly also to the Genesee; while the *Cardiola* is a Devonian genus having species in the Upper Helderberg and Hamilton; which facts appear to establish the equivalency of the New Albany Black Shale and the Genesee shale; while the Goniatite shale, which rests upon it, represents the Kinderhook group (Subcarboniferous) of Illinois.

Kaolin, or white porcelain clay, occupying "pockets" in the Carboniferous rocks of Pope County, has been long known under the name of Golconda clay. In Lawrence County the clay constitutes a bed interstratified with the Carboniferous rocks, which is five to six feet thick (one-third pure white) and has a wide extent. It is underlaid by a bed of limonite five feet and less in thickness. It lies beneath the Millstone grit and seems to correspond to an upper member of the Archimedes limestone. The Kaolin is in part (A) Snow-white and pulverulent; in part (B) white to purplish-brown, and cutting like putty; and (C) in concretionary masses of a pea-green color fading in the light, with an unctuous feel. It is called *Indianaite* by Mr. Cox. The kaolin contains masses of allophane of a transparent emerald-green color, one specimen of which afforded Dr. J. Lawrence Smith: Silica 20 per cent, alumina 40, water 40. All degrees of gradation exist between the allophane and kaolin.

Analysis of the kaolin afforded—

	$\ddot{Si}$	$\bar{\bar{A}}l$	$\bar{\bar{F}}e$	$\bar{M}n$	$\dot{C}a$, $\dot{M}g$	$\dot{H}$
Var. A	45·90	40·34	—	—	—	13·26
Var. B	47·05	37·14	*tr.*	·03	·03	15·55
Var. C	47·13	36·76	*tr.*	—	·04	15·13
Golconda	42·28	43·05	—	—	*tr.*	14·66

The several County Reports with their maps present much valuable matter on the Quaternary, Devonian, Carboniferous and Subcarboniferous rocks. Prof. Collett states that the boulder clay of Northern Illinois has its southern limit in the northern part of Brown Co., with little thickness, but in Northern Illinois it becomes 150 to 250 feet thick; and he adds some facts respecting erosions by the waters from the melting glacier. Various sections illustrating the relations of the Black Shale to the other rocks are given by Professor Borden, whose discoveries in it are mentioned above.

12. *The Geological and Natural History Survey of Minnesota: The Third Annual Report*, for the year 1874; by N. H. Winchell, State Geologist. Submitted to the President of the University of Minnesota, Dec. 31, 1874.—Prof. Winchell, in this Report, treats of the Geology of Freeborn and Mower Counties. Of Freeborn County Mr. Winchell remarks, "There is not a natural exposure of the underlying rock" and "hence the details of its geological structure are wholly unknown." The field is

still not a barren one; for the surface is covered with the drift, and the material of which it consists affords some evidence of the rocks below; and, besides, a shaft has struck the Cretaceous in the northwestern portion of the county. It is inferred that the Cretaceous extends north and south across the west end of the county. The average thickness of the drift in Freeborn County is stated to be not far from 100 feet. A figure is given illustrating the flow-and-plunge structure of the sand and gravel at Albert-Lea. Clay for brick-making is obtained near this place about five feet below the surface. Clay, from the drift formation, is also found at Geneva.

Mower County also is buried in drift, but not to the concealing of all rock exposures. As elsewhere in the northwest, the drift-made soils are of the very best kind. The drift material is a "hard-pan clay" light in color for 10 to 15 feet, and below this usually bluish. It contains gravel and boulders, some of them of granite and very large. At Windom and LeRoy this drift overlies a peat bed.

The Lower Cretaceous underlies the western portion of the county; how far east it extends is unknown. The Cretaceous beds at Austin have afforded fossil leaves—one kind related to *Ficus primordialis* Hr., and another a *Sequoia*. Explorations for coal in these counties have obtained nothing that promises success. Lignite is found to encourage the search, but, Mr. Winchell observes, "after the tests that have already been made it can be pretty confidently stated that the lignites are at present of no economical value."

13. *Note on Lignite in the Cretaceous of Minnesota;* by N. H. WINCHELL. (From a letter to J. D. Dana, dated Minneapolis, Minn., Sept. 2, 1875).—An interesting discovery of lignite in the extreme northern portion of Minnesota has just been made. Explorations have been carried on during the summer by private parties in search of coal, and a hundred pounds of what was taken for coal were brought to Duluth from a point between Vermilion and Rainy lakes.

Similar exploration has revealed the lignites of the Cretaceous at a number of other points in Minnesota, extending from near the Iowa state line to the central portion of the state. Mr. Kloos has given an account of a "Cretaceous basin in the Sauk Valley" in this Journal, and Mr. Meek identifies it as the *Fort Benton* by the few fossils that were gathered. Inferentially the Cretaceous has been extended over the most of Minnesota, and even into the State of Michigan (First Annual Report of the Geological Survey of Minnesota, 1872), but this discovery not only shows that the Sauk Valley is probably not an isolated "basin," but also that the Cretaceous beds did extend, prior to the drift, at least, if they do not now, over the entire state from north to south. I have an account also of a probable Cretaceous outcrop in the state of Wisconsin from Prof. Frank H. Bradley, but it has not been identified authentically.

14. *Note on the Age of the Cretaceous of Vancouver Island and Oregon;* by W. M. GABB. (From a letter to J. D. Dana, dated Philadelphia, Sept. 5, 1875.)—In the April Number of this Journal, p. 318, there is a note by Mr. Selwyn with reference to the age of the Nanaimo coal mines. While fully endorsing the opinion there expressed, it may be worth while to call attention to the fact that the same opinions were put on record in the Reports of the California Geological Survey, both by Prof. Whitney and myself, as much as eight years before the date of the volume of the Canadian Report quoted.

In this connection, I wish to note in the new edition of the "Manual of Geology," p. 456, top of page: The Cretaceous formation is said to occur "in Oregon, east of the Cascade Range (Marsh)," thereby giving to Prof. Marsh the credit of the first announcement.

Without wishing to disparage the excellent work done by that paleontologist, you have inadvertently done me an injustice, to which I call your attention. See Palæontology of California, vol. 2, p. 181, foot-note.

15. *Sub-Wealden Exploration.*—The Sub-Wealden boring has reached a depth of 1,672 feet. It was the opinion of British geologists that Paleozoic rocks would be found at a depth varying from 700 to 1,700 feet. So far, however, the strata are Mesozoic; but the latest fossils give some indications of a Paleozoic rock.—*Nature*, July 29.

16. *Das Elbthalgebirge in Sachsen.* I Theil, 8 Lief., and II Theil, 6 Lief. (Cassel, Th. Fischer.)—This great work, by Dr. GEINITZ, has been brought to a close by the publication of the two numbers here announced—the first (pp. 277 to 320) closing his descriptions of the fossils of the Lower Quader (Upper Green Sand of the Middle Cretaceous), and the second (pp. 199 to 246) of those of the Middle and Upper Quader (Upper Cretaceous). The lithographic illustrations, drawn partly by E. Fischer, and in part by Miss Elise Geinitz, daughter of the author, extend to 67 4to plates for the former and 46 for the latter, and are excellent; they include figures of species of Sponges (10½ plates) Foraminifera, Radiates (18½ plates), Bryozoans, Brachiopods, Conchifers, Gasteropods, Cephalopods, Worms, Crustaceans, Fishes, Reptiles and Plants. The remains of the Vertebrates of the Lower Quader, described and illustrated in Part I, include species of the genera of Sharks, *Oxyrhina*, *Otodus*, *Lamna*, *Scylliodus*, *Corax*, *Acrodus*, *Ptychodus;* of Ganoids, *Pycnodus;* and of Reptiles, of the genera, *Plesiosaurus* and *Leiodon*. The remains of Fishes, illustrated in Part II, include teeth of Sharks of the genera, *Oxyrhina*, *Lamna*, *Otodus*, *Notidanus*, *Corax*, *Ptychodus* and *Spinax;* remains of *Chimæra Agassizi* Buckl. and *C. Mantelli* Buckl.; under Ganoids, species of *Pycnodus*, *Hemicyclus*, etc.; under Ctenoids, *Beryx ornatus* Ag., *Hypsodon Lewesiensis* Ag., etc.; under Cycloids, *Cladocyclus*, *Strehlensis* Gein., *Saurocephalus lanciformis* Harlan, *S.? dispar* Hébert, *Enchodus halocyon* Ag., *Osmeroides Lewesiensis* Mant., *O. divaricatus* Gein., and *Cyclo-*

lepis Agassizi Gein.; and the Reptiles comprise the species *Plesiosaurus Bernardi* Owen, *Polyptychodon interruptus* Owen, *Chelonia Carusiana* Gein.

The work is full in its descriptions and synonymy, and many of the species have been first made known by Dr. Geinitz.

17. *Paleontological Cabinet of Prof. James Hall.*—Professor Hall's extensive collection of fossils has been purchased for the Central Park Museum, New York. The price reported to have been stipulated is $65,000.

18. *On two new varieties of Vermiculites;* by Prof. J. P. Cooke and Mr. F. A. Gooch. (Proc. Amer. Acad. Sci., of May 11, 1875).—In this paper, which may be regarded as supplementary to Prof. Cooke's elaborate memoir published early in 1874, the author describes two new varieties of vermiculites, one from Lerni, Delaware Co., Pa., and another from Pelham, Massachusetts. By means of careful experiments, the loss of water was ascertained after air-drying, and at 100° C., 300° C. and red heat. The analyses obtained and those previously made of other vermiculites, are discussed, and the following important conclusions reached.

1. That all the vermiculites are unisilicates.

2. That these minerals combine with water in several definite proportions—sustaining Prof. Cooke's former statement that the water in the vermiculites is water of crystallization.

3. That they may be reduced to the condition expressed by the atomic ratio (oxygen-ratio) for the silica, oxides and water, 2 : 2 : 1, which is therefore taken as the normal ratio.

4. That the only essential difference between the different varieties of vermiculites is in the ratio between the sesquioxide and protoxide bases.

The atomic ratios obtained for the air-dried, dried at 100°, and dried at 300°, specimens, are respectively, for

Hallite	4 : 4 : 3,	4 : 4 : 2½,	4 . 4 : 2
Pelhamite	4 : 4 : 4,	4 : 4 : 2,	4 : 4 : 1
Jefferisite	4 : 4 . 4,	4 : 4 : 2,	4 : 4 : 1

The Lerni mineral gave, dried at 100° C., the ratio 2 : 2 : 1 (=4 : 4 : 2); and the analysis afforded (mean of results)

$\ddot{\mathrm{Si}}$ 38·03, $\mathrm{\not{A}l}$ 12·93, $\mathrm{\not{F}e}$ 7·02, $\dot{\mathrm{F}}\mathrm{e}$ 0·50, $\dot{\mathrm{M}}\mathrm{g}$ 29·64, $\dot{\mathrm{H}}$ 11·68=99·80.

19. *Dana's System of Mineralogy.*—A new "subedition" of this work—the sixth—has been brought out by the publishers, Messrs. Wiley & Son of New York; in other words, copies have been struck off from the plates *after making a number of corrections* as in the case of the other "subeditions." Moreover the copies of this subedition contain the two Appendixes to the work—the first by Professor Brush, and the second by Mr. E. S. Dana—adding thus 98 pages to the 828 of the original volume.

20. *Hematococcus lacustris, &c.*—Some account of researches upon this microscopic plant, and upon the foundations of a natural classification of the Chlorosporous Algæ, are just published by Rostafinski—lately a pupil of DeBary—in the Memoirs of the Academy of Sciences of Cherbourg (tom. xix, pp. 137–154, 8vo, 1875). The identity of *Protococcus*, *Hæmatococcus*, or *Chlamydi-*

coccus nivalis and *pluvialis* is made out; at least it is shown that the latter can live upon snow and ice, and that the development is identical. For the generic name of the *Red-snow* plant, &c. Agardh's name of *Hæmatococcus* is preferred, on good grounds: the specific name adopted is *lacustris*, Girod-Chantrans having well investigated the plant and figured and described it, under the name of *Volvox lacustris*, so long ago as the end of the last and the beginning of the present century (1797, 1802). *Hæmatococcus* propagates by two kinds of zoospores; i. e. sometimes by large and ordinary ones, resulting from the division of the contents of the cell or plant into four daughter-cells, each of which is transformed into a zoospore of somewhat complicated structure; while other individuals transform their contents into about 32 microzoospores. The development of both kinds of zoospores into the plant has been observed by Rostafinski. The development is non-sexual. Velter's supposed discovery of the copulation of the large zoospores is discredited and explained away. Rostafinski concludes that *Hæmatococcus* is devoid of sexual reproduction. Following up Decaisne's early hint that the reproductive organs of *Algæ* should furnish the characters for their natural arrangement, he indicates the principal groups or tribes of the *Chlorosporeæ* which have thus far been made out, by DeBary and others, with some re-organization. Thus, after the *Conjugatæ*, in which fecundation takes place by the conjunction of two immobile cells of the same value (i. e. with no distinction of male and female), he proposes to place a parallel tribe, *Isosporeæ*, in which there is a copulation of zoospores, the sex of which is equally indeterminate (*Hydrodictyon*, *Botrydium*, &c.). The third is *Oophoreæ* of DeBary (*Sphæroplea*, *Vaucheria*, *Œdogonium*, &c., to which Rostafinski adds *Volvox* and *Eudorina*); here the fecundation is by antheroroids and oospores. And he is disposed to take *Hæmatococcus* as the type of a fourth tribe, *Agameæ*, propagating non-sexually by spores. A. G.

21. *Catalogue of Plants growing without cultivation within thirty miles of Amherst College;* by EDWARD TUCKERMAN, M.A. and CHARLES FROST, M.A. Amherst, 1875. pp. 98, 8vo.—The Lower Cryptogamia, exclusive of the *Lichenes*, are by Mr. Frost. They fill half the Catalogue. Prof. Tuckerman contributes an interesting preface. The Catalogue bears marks of the scrupulous accuracy and neatness of finish which characterize all of Prof. Tuckerman's work, the simplest, no less than the more elaborate; and the arrangement, typography, and paper, are of corresponding excellence. We venture to correct a correction. We are told at the close to "read *Calystegia Sepium*"; but we prefer to read as it stands in its place "*Calystegia sepium.*" Such substantive genitive plurals, not being proper names, surely need no capital initial, nor are they accustomed to have them. A few plants have been detected in the district which would not have been expected and are not before recorded. *Blephilia ciliata* is the most noteworthy of these. We are curious to know how many species this model catalogue contains, and count it an oversight that the number is

not reckoned. A rough and rapid enumeration gives 1066 Phænogamous species, 59 Acrogenous Cryptogamia, 216 Anophytes, 1361 Thallophytes, of which over eleven hundred are Fungi; so that the Cryptogamia amount to 1839.

We may hope that the time is not far distant when we may have Manuals of all our Lower Cryptogamia, beginning with the *Lichenes* by Prof. Tuckerman. A. G.

22. *Serjania Sapindacearum Genus monographice descriptum. Monographie der Sapindaceen-Gattung Serjania*, Von L. RADLKOFER. Munich, 1875. pp. 392, 4to. Published by the Royal Bavarian Academy.—To this memoir the quinquennial prize founded by the will of the elder DeCandolle has been awarded, and it well deserves it. It is one of the most elaborate, painstaking, and apparently thorough pieces of monographical work in systematic botany that we have ever seen. Undertaken as it is by one who has proved, at the commencement of his career, his ability for microscopical investigation (when Radlkofer gave the *quietus* to the theory of the origination of the embryo from the pollen-tube), we may hope that an investigation of this kind will not be detrimental to the author's prospects, in a country where it has been the fashion to undervalue everything not microscopical, and even that it may betoken a rising appreciation of systematic botany in Germany. The subject is too technical to call for a detailed review in this Journal. We merely note that 145 species of Serjania are characterized, described, and their synonymy completely exhibited. Nearly all extant materials appear to have been under the author's hands. A. G.

23. *Zur Keimungsgeschichte der Charen, von* A. DE BARY.—In the present paper, extracted from the Botanische Zeitung, Prof. Be Bary gives a detailed account of the manner in which the prothallus is produced from the spore in the Charæ. That the new *Chara*-plant does not spring directly from the spore was first shown by Pringsheim, who noticed that the plant is a lateral outgrowth from an intermediary filamentous structure, the *vorkeim* (prothallus). De Bary finds that a lenticular portion of the spore projects beyond the mass of the spore from which it is soon separated by a wall. The lenticular portion is then divided into portions, one of which develops into the prothallus proper, while the other becomes what is known as the primary root in *Chara*, although it does not correspond to the structure of the same name in phanerogams. In passing, reference is made to parthenogenesis in *Chara crinita*, which fact is confirmed by De Bary, who finds that female plants isolated in closed glass vessels fruit abundantly.

W. G. F.

24. *Description of a new Crustacean from the Water-lime group at Buffalo;* by AUG. R. GROTE and W. H. PITT, (Bulletin of the Buffalo Society of Natural Science, vol. iii, July, 1875.)—The specimen shown exhibits an impression of the ventral surface of a new form of Crustacean allied to *Eurypterus Pterygotus*, for which the name *Eusarcus scorpionis* is proposed. The cephalothoracic portion appears to be separate from the body,

and to be considerably narrower in proportion than in allied forms. The legs are in the same number as in *Eurypterus*. The swimming feet appear to differ by the straighter, less rounded outer margins. In the specimen the rhomboidal plates are not given. From the impressions of the joints of the swimming feet their relative dimension does not seem to accord with *Eurypterus*. The four pair of anterior feet proceed from two elongate oral plates of which the impression is very distinct. The spines of the anterior feet appear to be long, curved, and to have an anterior direction. The absence of chelate appendages to the posterior margin of the feet is particularly noticeable. The first seven broad segments of the abdomen form a large ellipse. There is an evident and remarkable narrowing of the succeeding caudal segments. Of these six appear to be made out on the specimen. The surface of the cast is punctate with scattered triangular impressions. The cast shows a widening of the terminal segment and no traces of a spiniform process are exhibited. This portion of the fossil was imbedded in the matrix, and seems to have been bent downward and outward in the specimen. The greater flexibility of the caudal segments may be inferred from their shape and position. The specimen from which the above description is drawn was found in the Water-lime group at Buffalo, N. Y., in the same bed which has furnished specimens of *Eurypterus lacustris*. The length of the entire specimen is two hundred and fifty millimeters; the greatest width of the body is one hundred and ten millimeters.

"The interest which attaches to this remarkable Crustacean arises from the discovery of a form which may be allowed to be higher than *Eurypterus* and *Pterygotus* from the peculiar differentiation of the body expressed by the narrowness of its cephalo-thoracic portion, and the sudden constriction of the terminal segments." The article is accompanied by a fine plate.

25. *Bathybius*.—Prof. Huxley in connection with the publication of a letter to him from Prof. Wyville Thomson in *Nature* of August 19th, remarks as follows:—Prof. Wyville Thomson further informs me that the best efforts of the *Challenger's* staff have failed to discover *Bathybius* in a fresh state, and that it is seriously suspected that the thing to which I gave the name is little more than sulphate of lime, precipitated in a flocculent state from the sea-water by the strong alcohol in which the specimens of the deep-sea soundings which I examined were preserved.

"The strange thing is, that this inorganic precipitate is scarcely to be distinguished from precipitated albumen, and it resembles, perhaps even more closely, the proligerous pellicle on the surface of a putrescent infusion (except in the absence of all moving particles), coloring irregularly but very fully with carmine, running into patches with defined edges, and in every way comporting itself like an organic thing."

Prof. Thomson speaks very guardedly, and does not consider the fate of *Bathybius* to be as yet absolutely decided. But since I am mainly responsible for the mistake, if it be one, of introducing this singular substance into the list of living things, I think I shall err on the right side in attaching even greater weight than he does to the view which he suggests.—*Nature.*

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *American Association.*—The meeting of the Association at Detroit was well attended, and, as the list below shows, many papers of interest were presented. The address of the retiring President, Dr. John L. LeConte, is republished in pages 241–256. An excellent address was delivered by Prof. H. A. Newton, Vice President of the Physical section, on the importance of the Study of Mathematics for the student in all branches of science, and another, by Dr. J. W. Dawson, before the Geological and Natural History section, presenting some of the difficulties which geology appears to place in the way of Darwinism. The Chemical section, organized at the preceding meeting, at Hartford, brought together many chemists.

The next meeting of the Association is to be held at Buffalo. The following officers were appointed for that meeting: President, Prof. WM. B. ROGERS; General Secretary, F. C. MENDENHALL; Vice President of Section A, CHARLES A. YOUNG, of Hanover, N. H.; of Section B, EDWARD S. MORSE, of Salem, Mass. F. W. PUTNAM, of Salem, Mass., is Permanent Secretary.

The following is a list of the papers read either in full or by abstract.

I. *Mathematics, Physics, Astronomy.*

The Solar Atmosphere; S. P. LANGLEY.

Account of the Measurement of a Primary Base Line in the U. S. Coast Survey, near Atlanta, Ga.; J. E. HILGARD.

Problems in Watson's Coordinates; T. HILL.

Distribution of the Asteroids; D. KIRKWOOD.

Descriptive Geometry; S. E. WARREN.

An Acoustic Method of Measuring the Velocity of Electricty; J. LOVERING.

A Systematic Method for indicating the Localities of a County, and rapidly discovering Places in a Map; J. M. TONER.

Tides of New York Harbor; W. FERREL.

Algebraic curves expressed in trigonometric equations; H. A. NEWTON.

Transformation of curves from Algebraic to Transcendental; H. A. NEWTON.

Geometrical representation of certain Diophantine Problems; H. A. NEWTON.

Path of Iowa County Meteorite, with exhibition of fragments of the same; N. R. LEONARD.

A new Meteorological Instrument; J. W. OSBORNE.

The Relative Intensity of the broken lines of Metallic Spectra; G. F. BARKER.

A New Vertical Lantern Galvanometer; G. F. BARKER.

On some Inequalities of Long Period in the Moon's Motion; J. N. STOCKWELL.

Adjustment of the Gregorian Calendar; E. B. ELLIOTT.

The Subsidiary Principle as applied to Coinage and Money account; E. B. ELLIOTT.

Description of an apparatus for the automatic determination of Atmospheric resistance; G. W. HOUGH.

Method of exhibiting by Projection the difference in rapidity of transmission of Sound through various Gases; T. C. MENDENHALL.

Temperature of two Deep Borings in Indiana; E. T. COX.

II. *Chemistry.*

On an improvement of Bunsen's method for specific gravities of gases, etc.; T. C. **Mendenhall.**

Chemistry of three dimensions; F. W. **Clarke.**

On certain new Tungsten compounds; A. R. **Leeds.**

On the omission of Dynamical Quantities from Chemical Formulæ; H. F. **Walling.**

Carbon determinations in Iron and Steel; J. W. **Langley.**

Comparative determinations of the solubilities of Alkaloids in Crystalline, Amorphous, and Nascent conditions; A. B. **Prescott.**

Note on Colorado Tellurium; F. W. **Clarke.**

On the requisite amount of simple friction of soft iron against cold steel to melt it; B. S. **Hedrick.**

On Otto's method of estimating Phosphoric Acid in presence of Iron and Aluminium; S. W. **Johnson.**

Apparatus for fat extraction; S. W. **Johnson.**

Composition of corn fodder and yield per acre; S. W. **Johnson.**

Composition of the Sweet Potato; S. W. **Johnson.**

On Thorpe's method of estimating Nitric Acid; S. W. **Johnson.**

A Convenient Instrument for showing the Absorption of Hydrogen Gas by Palladium; J. L. **Smith.**

On Graphitic Oxide as prepared from the Graphites of the Sevier Co. and Dekalb Co. Meteorites; J. L. **Smith.**

Notice of an undescribed Meteorite that fell in Wisconsin in 1865; J. L. **Smith.**

Exhibition of a large quantity of Cæsium Alum, with some remarks on its Preparation; J. L. **Smith.**

On certain precipitates containing Chloride of Silver; F. W. **Clarke.**

III. *Geology and Paleontology.*

Geology of the Southern Counties of New York, and particularly of the Catskill Mountain Region; J. **Hall.**

A comparison between the Northern (or Ohio) and the Southern (or W. Va.) side of the Alleghany Coal Field; E. B. **Andrews.**

New and remarkable Coal-plants from the lower Coal Measures of Ohio, and their typical relations; E. B. **Andrews.**

New specimens of Fossils from Canada; J. W. **Dawson.**

Physical Geology of the trap formation of Lake Superior; C. **Whittlesey.**

Physical Geology of the Ohio coal-field; C. **Whittlesey.**

On Ancient Glaciation at Kelley's Island, Ohio; C. **Whittlesey.**

Ancient Peat in the Drift deposits of Minnesota; N. H. **Winchell.**

Rectification of the Geological Map of Michigan; A. **Winchell.**

Evidence of Glacial Action upon the summit of Mt. Washington, N. H.; C. H. **Hitchcock.**

On the Galisteo Sandstone of New Mexico; E. D. **Cope.**

Indianaite, a new form of Kaolin found in Indiana; E. T. **Cox.**

Kaoline Beds of Southern Illinois: H. C. **Freeman.**

On some new Fossil Fishes and their Zoological Relations; J. S. **Newberry.**

On some of the Results of the Geological Survey of Ohio; J. S. **Newberry.**

The Ice Period in the Mississippi Valley; its phenomena and causes; J. S. **Newberry.**

Parallelism of Devonian outcrops in Michigan and Ohio; N. H. **Winchell.**

New and Interesting Insects from the Carboniferous of Cape Breton; S. H. **Scudder.**

On the supposed Ancient Outlet of Great Salt Lake; A. S. **Packard,** Jr.

Formation of Geyserite Pebbles in pools adjacent to the Geysers of the Yellowstone Park; T. B. **Comstock.**

Remarks on the Hot Springs and Geysers, and other topics illustrating the scientific value of the Yellowstone Park; T. B. **Comstock.**

Exhibition of some Curious disks of Silica in the structure of Coal from Tennessee; J. L. **Smith.**

IV. *Zoology and Botany.*

The effect of the Glacial Epoch upon the Distribution of Insects in North America; A. R. GROTE.

Are Potato Bugs poisonous? A. R. GROTE and A. KAYSER.

Are Insects any material aid to plants in fertilization? T. MEEHAN.

Locusts as Food for Man; C. V. RILEY.

The Locust Plague in America: how to avert it; C. V. RILEY.

On the method of subduing Insects Injurious to Agriculture; J. L. LECONTE.

Symmetry and want of Symmetry of Leaves; W. J. BEAL.

Venation of Leaves of Dragon-Root and Martynia compared; W. J. BEAL.

Additional Observations on the Tarsus and Carpus of Birds; E. S. MORSE.

Distribution of Batrachia and Reptilia of North America; E. D. COPE.

Indications of Descent exhibited by North American Tertiary Mammalia; E. D. COPE.

Protozoan Studies; W. S. BARNARD.

On leading divisions in Recent and Fossil Chitonidæ; P. P. CARPENTER.

A method of Bleaching Wings of Lepidoptera to facilitate the study of their venation; G. DIMMOCK.

Observations on the Development of *Didelphys Virginiana;* W. S. BARNARD.

Membral Myology of *Simia Satyrus* as compared with Man and the higher Apes; W. S. BARNARD.

Demonstration of Locomotion in the Larvæ of Oestridæ; O. S. WESTCOTT.

Periodicity in Vegetation; J. HYATT.

Embryology of the Fresh Water Mussels; W. K. BROOKS.

Some Observations on the Structure and Habits of *Utricularia vulgaris* (Carnivorous? plant); T. B. COMSTOCK.

On hybernation as exhibited in the Striped Gopher; P. R. HOY.

Gestation of Bats; B. G. WILDER.

Affinities and Ancestry of the Existing Sirenia; B. G. WILDER.

Notes on Menobranchus and Menopoma; B. G. WILDER.

Notes on the American Ganoids (*Amia*, *Lepidosteus*, *Acipenser* and *Polyodon*; B. G. WILDER.

Structure and Economy of Lamprey Eels (*Petromyzon*); B. G. WILDER.

The rectal pouch of Sharks and Skates, as perhaps representing the allantois of Air-breathing Vertebrates; B. G. WILDER.

V. *Ethnography and Archæology.*

Fac similes from Nature of Ancient Rock Sculptures in Ohio, with verbal explanations; C. WHITTLESEY.

Recent Mound Explorations at Davenport, Iowa; R. J. FARQUHARSON.

Indian Mounds and Shell-heaps near Pensacola, Florida; G. M. STERNBERG.

Notes on Aboriginal Money of California; L. G. YATES.

The Ancient Men of the Great Lakes; H. GILLMAN.

Ancient Earthworks near Anderson, Ind.; E. T. COX.

Rates of Human Progress; L. H. MORGAN.

Arts of Subsistence; L. H. MORGAN.

Ethnical Periods; L. H. MORGAN.

Mound Explorations in Kent County, Michigan; E. A. STRONG.

Archæological Notes from Wyoming; T. B. COMSTOCK.

2. *Foreign Scientific Associations.*—The meeting of the British Association at Bristol opened on Wednesday, the 25th of August. *Nature*, in the number for the 26th, and several following numbers, contains an extended report of the Proceedings.

The French Association began its session at Nantes on the 19th of August; and the Italian, at Palermo, on the 29th.

3. *The British Scientific Expedition.*—A third Report of the proceedings of H. M. S. Challenger has been issued by the Ad-

miralty, giving an outline of the work done, in connection with oceanic geography and its physical condition, between March and November of last year.

Having arrived in safety at Melbourne from her Antarctic cruise, the Challenger left Port Philip on the 1st of April for Sydney, at which place the ship was docked, refitted and completed with coal and provisions. During the stay in that harbor, Prof. Wyville Thomson made a journey to Queensland for the purpose of studying the natural history of tropical Australia.

On the 12th of June the ship left Port Jackson, on her voyage to New Zealand meeting with stormy weather, so stormy indeed as greatly to impede the work of sounding across, and it was unfortunate, as much interest was felt both in New South Wales and New Zealand in regard to the adaptability of the channel bed between them for telegraphic purposes. From the Australian shore the water deepened gradually until a depth was attained of 2600 fathoms, about one-third the distance across; the bottom at this depth consisted of the usual red clay, found in the deepest soundings. A depth of 275 fathoms, with rocky bottom, was found about 280 miles from New Zealand, with slightly deeper water within it. On the 25th, Port Hardy was reached, and Port Nichelson on the 27th. A seaman was unfortunately washed overboard and drowned in Cook Strait. The weather continued wild during the Challenger's stay at Wellington. On the 6th of of July she proceeded on her voyage, passing northward along the east coast of the north island, and on toward the Kermedec group; the stormy weather experienced precluded sounding operations, and the depth between New Zealand and those islands has still to be decided. The results of the dredging near Kermedec proved that animal life was much the same as at the same depths off the coast of Portugal, but several species new to science were obtained.

On the 17th of July, in lat. 25° 5′ S. and long. 172° 56′ E., the deepest cast since quitting the Atlantic was obtained—2,900 fathoms, the bottom temperature being 32°·9, by which temperature it is conclusive that there is a continuous and deep channel extending from the southward into these seas. The Expedition remained two days at Tongatábu, and then left for the Fiji Islands. The ship left the Fiji islands on the 10th of August, and when about thirty miles from the land, a depth was sounded of 1350 fathoms and the red ooze found, which had previously been obtained at only much greater depths. On the 15th, when about midway between Fiji and the New Hebrides group, a cast was had of 1450 fathoms, with the same nature of bottom.

Passing through between the islands of the last-named group, the Challenger proceeded on her way toward Raine Island. A sounding of 2650 fathoms was had, and then four others, slightly shoaling. The serial temperatures of these soundings proved one physical fact, viz: that the sea was cut off by a surrounding ridge, over which the greatest depth of water of any channel through it is 1,300 fathoms, the temperature at that depth being 35°, and then continuing the same to the bottom. Below the 1,300 fathoms in the hollow between New Hebrides and Torres Strait the water is comparatively stagnant, as in the Mediterranean and other cut-off seas.

When 170 miles from Raine Island, the depth was 1,700 fathoms, and at seventy-four miles 1400 fathoms, showing that the inclination of the bottom of the sea was gradual up to the barrier reef. After anchoring for one night near Raine Island, the Expedition reached Somerset, Cape York, on the 1st of September, where they remained a week, and then proceeded through the Banda Sea, carrying a continuous line of soundings, and touching at Dobbo, Ki Doulan, and Banda. They arrived at Amboina on the 6th of October, where they obtained coal, and then, passing through the Molucca passage, reached Ilo Ilo on the 28th, Manila on the 4th of November, and Hong Kong on the 19th.

From the temperatures obtained in the Banda, Celebes, Sula, and China Seas, it is evident that their waters are cut off from the general oceanic circulation by ridges conecting the islands which surround them.

In each of the seas soundings of over 2,000 fathoms were obtained, but in no instance did the temperature decrease regularly from the surface to the bottom, as is usual in the ocean. In every case, after attaining a certain depth, the temperature below that depth remained the same; thus, in the Banda and China seas, the temperature remained the same from 900 fathoms to the bottom; in the Celebes Sea from 700 fathoms to the bottom; and in the Sula Sea from 400 fathoms to the bottom.—*Athenæum*, Aug. 7.

3. *Images produced by Lightning.*—A letter of Professor W. L. Brown, of the University of Georgia, to one of the editors, calls attention to a remarkable instance of the formation of impressions upon the human body by a lightning stroke, and encloses letters of Messrs. E. G. Simmons and T. N. Hawkes, of Americus, Ga., where the occurrence took place, describing the circumstances and some of the details of the phenomenon.

On the 12th of July, 1875, at about 4 P. M., as Mr. Simmons states, a stroke of lightning fell upon a house in Americus, rendering insensible for a time four persons who were seated in one of the rooms. The two outer sides of this room, which was at a corner of the house, had each one window, and nearly opposite these on the two other sides were the chimney and the door respectively. Outside, a tree stood in front of each of the windows, and about twelve feet from the house. A third tree, a locust, stood opposite the outer corner of the room and about the same distance from the house as the others. This tree was severed by the lightning, but the other two were not affected. A young child was sitting near the center of the room, while the mother and a young lady were seated not far from the chimney, near which, and close to the wall, was another child.

All these persons were rendered insensible for a time by the stroke which severed the tree, and on their recovery there were found impressed upon the bodies of them all more or less distinct images of this tree. It was most distinct in the case of the child near the center of the room. Mr. Simmons says, "the child is impressed upon its back and exactly opposite upon its stomach. The entire tree is plain, and perfect in *toto;* every limb, branch, and leaf, and even the severed part, is plainly perceptible. It is a perfect photograph of the tree, and could not have been made more perfect by a painter's brush. It impressed the young lady upon the left hip and right leg," the mark being quite as perfect as that upon the body of the child. The mother and other child also bore less distinct impressions upon the leg. Mr. Hawkes says, "I saw the youngest child and the marks upon it. The marks on the lower part of the spine are exactly of the shape of a branch of the locust tree. Where the leaves are, on the branch, the mark does not take the shape of the entire leaf, but only of the skeleton of a leaf. The mark was about twelve inches long, and had a fork, exactly like the locust limb. It looked as though the blood was drawn to the skin along the marked part." The marks were not permament, as Prof. Brown, in his letter dated Aug. 7, adds, "a recent letter informs me that the impressions are no longer distinct."

Although many similar cases have been reported, the phenomenon described is rare enough to make a well-marked instance like this worthy of record. The formation of the images in all such cases is readily explained by a peculiarity in the mode of the electric discharge under certain conditions, by imitating which it is possible to produce similar figures artificially with an electrical machine, such as the Holtz machine, capable of giving electricity of very high tension. When the poles of the latter are strongly charged and are separated to the distance of a few inches, the discharge, instead of producing a spark or brush, sometimes consists of a very small jet upon the negative, and a sort of phosphorescent glow upon the positive. The space between them, though not luminous, is the seat of a discharging action which appears to take place along definite lines, like a stream or current, and is sometimes called the dark discharge. An object placed between the poles, and in the path of the discharge, interrupts this, and destroys the glow upon the positive pole in points corresponding to the lines thus broken; and in this way there is produced an image or shadow of the interposed object, which is often strikingly distinct and perfect.

In the case above described the phenomena are readily accounted for, if we suppose the thunder-cloud to have been negatively charged, and the tree to have stood in the path of the dark discharge which preceded or accompanied the lightning stroke, the action having been sufficiently intense, and the quantity of electricity great enough, to produce a visible impression upon the delicate tissues of the skin.

A more particular account of the subject may be found in two articles published in this Journal, May, 1870, page 381, and June, 1871, page 437. A. W. W.

5. *Annual Report of the Argentine Meteorological Office*, for the year 1874. 18 pp. 8vo. Buenos Aires, 1875.—This Report, by the Director of the office, Prof. B. A. Gould, gives details as to the arrangements for making meteorological observations which have been introduced into the Argentine Republic, and the present needs in order that the objects of the meteorological office may be efficiently carried out and rendered of permanent value to science. Tables of the mean temperature and barometric pressure, of principal points, made out from the observations thus far obtained, are given. Mr. Gould states that while at Cordoba the humidity of the air has its maximum in summer, the amount of moisture in February averaging 80 per cent of that requisite for complete saturation of the atmosphere, and its minimum, 50 per cent in September, at Buenos Aires, on the contrary, the maximum is usually in June, 83 per cent, and the minimum in December, 60 per cent. During the summer of 1874 the temperature of the solar rays was found at Cordoba to be on December 7th not less than 74·5° C., and it reached 77·8° on the following January 8th. The minimum under a clear sky, 52·5° was observed on the 19th of June.

Mr. Gould is making his comprehensive and thorough knowledge of terrestrial physics as well as astronomy tell in various ways on the progress of science in South America.

6. *Appletons' American Cyclopædia*, Vol. XI and XII, 1875. —The opening article of volume XI, Magnetism, is by Prof. Joseph Henry. It discusses magnetism and diamagnetism, terrestrial magnetism, magneto-electricity, induced currents from discharges of ordinary electricity, and induction in masses of metal in motion. Prof. Cleveland Abbe of the U. S. Signal Service contributes the article Meteorology, which is illustrated by numerous diagrams many his own or from original American sources. It is an admirable digest of the present state of this science. Molecule, by Prof. J. P. Cooke of Cambridge, is an article worthy of its author, setting forth the latest physico-chemical deductions, and the author's own views, in a clear and concise manner. The articles Metal and Meteorites, Ore dressing, and extraction of metals from their ores by chemical means, are furnished by Dr. Thos. M. Drown of Easton and Dr. R. W. Raymond; Mineral deposits, by Dr. J. S. Newberry; Microscope, by Prof. A. M. Mayer; Mediterranean Sea, by Count Pourtales; Meteor, Mars, Mercury, Moon, by Richard A. Proctor, London; and the metal Mercury, and Mine, by Dr. Raymond.

In volume XII, some of the leading scientific articles are—Paleontology, by Prof. James Hall; Mountain, by Dr. T. S. Hunt; the Theory of Music, by Prof. Mayer; Nebular hypothesis and other astronomical articles, by R. A. Proctor; Chemical Nomenclature, by Prof. Frank H. Storer of Harvard College. The botanical articles are by Prof. Geo. Thurber; those on Zoology by Dr. S. Kneeland of Boston; on Materia Medica by Dr. E. H. Clark of Harvard University; the medical and physiological by Dr. John C. Dalton; while Drs. Joy and Hogeboom furnish various chemical articles. From this list of well-known authors and investigators, it is evident that the scientific character of the new edition of Appletons' Cyclopedia is worthy, as a whole, of commendation. We miss occasionally a well-known name among the biographical sketches; for example, Dr. Charles G. Page, whose contributions to dynamical electricity are second only, in this country, to those of Prof. Henry; and the well-known paleontologist F. B. Meek. Other like omissions could easily be mentioned, while some names have a prominence greater than history will award them. B. S.

7. *Boston Society of Natural History.*—The Paleontological collections of the Boston Society of Natural History have been recently much enlarged by the purchase of the collection of Herr Eser of Stuttgart, which is particularly rich in fossils of the Tertiary and Mesozoic formations, including the Triassic. Its specimens of Tertiary plants are of such delicacy that they are mounted, like botanical specimens, on paper. It was secured for the society by the efforts of Mr. A. Hyatt, during his visit in 1874 to Europe, and through the generosity of Mr. John Cummings. In addition several specimens of Ichthyosaurs and Teleosaurs were procured.

8. *Geographical Congress at Paris.*—The recent Geographical Congress at Paris awarded *Letters of Distinction* (highest award) to the Secretary of the Navy for the work of the Coast Survey: and to General Myer, of the Signal Service Bureau; *Medals of the First Class* to Francis A. Walker, for his Physical Atlas of the United States; Dr. F. V. Hayden, for his Rocky Mountain Geological Exploration; and to the Bureau of Statistics, of the Interior Department;—*Medals of the Second Class* to the Committee of Emigration, and the Members of the Tuscarora Expedition.

OBITUARY.

Dr. I. A. LAPHAM, of Milwaukee, Wisconsin, lately at the head of the Geological Survey of that State, died on the 14th of September. Dr. Lapham was by profession a surveyor, but he was also a good worker in geology, meteorology, and American archæology. Several years since he published a geological map of Wisconsin, and also a map illustrating the influence of the Great Lakes on the climate of Wisconsin. He early drew attention to the Indian mounds of Wisconsin, and a work by him, illustrated by numerous plates, entitled "Antiquities of Wisconsin," was published, in 1855, in the Smithsonian Contributions to Knowledge.

CARL JOHANN AUGUST THEODOR SCHEERER, the mineralogist, metallurgist, and chemist, died at Freiberg, on the 20th of July last, having been born in Berlin on the 28th of August, 1813. He was the author of various works and memoirs of high merit; among the former his *Lehrbuch der Eisenhüttenkunde* and his well known *Löthrhorbuch* (a treatise on the blowpipe), and among the latter a paper on Paramorphism in its relations to chemistry, mineralogy, and geology.

THE
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[THIRD SERIES.]

ART. XLI.—*On the Variation in the Strength of a Muscle;* by FRANCIS E. NIPHER, Professor of Physics in Washington University, St. Louis, Mo. With plate VIII.

THE strength of a muscle is the maximum weight which it is capable of lifting at a single impulse. It is practically impossible to measure this strength directly, as it necessitates several trials, during which the muscle becomes fatigued. It may, however, be determined indirectly by a method which I shall make the subject of a future paper.

The evidence which we shall now present of variation in muscular strength from day to day, is seen in the variation in the amount of work done by the muscle, before exhaustion. We present here three series of experiments upon the right arms of myself and a former pupil, Mr. D. A. Myers. The first series consisted in finding the maximum pull, which the muscles could exert upon the spring of a dynamometer. The arm was stretched horizontally, grasping the hook of the dynamometer, the palm of the hand being downward, the pull was exerted upward. The arm moves in the vertical plane which makes an angle of 45° with the transverse plane.*

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* Prof. Haughton has criticised this position (Nature, vol. xi, p. 488), his point being that some muscles are called into play irregularly. He suggests that the arm should move in the transverse plane, with the palm of the hand upward. This position is a very unnatural one, and the arm will be wholly fatigued in two or three minutes by simply holding the arm in this position when its weight is supported by an external force. Although Prof. Haughton's opinion on this subject is worthy of grave consideration, yet I am not prepared to adopt it in this case. The manner of experiment is determined to some extent by the end to be accomplished, and I am still of the opinion that I have reduced the errors of experiment to a minimum. This point, however, deserves further attention.

In this case, part of the work of the muscles consists in moving the hook of the dynamometer over a certain space. x, under a constantly increasing strain P. The rest of the work done consists in sustaining the strain P, and the fatigue caused is due wholly to molecular motions in the muscle itself. Prof Haughton has proposed to call the former work "dynamical," the latter "statical" work.

The dynamical work D is then

$$D=\int P.dx, \text{ or as } P=c.x$$

(where c is the tension of the dynamometer spring when $x=$ unity.)

$$D=\int cxdx=\frac{cx^2}{2}+c''$$

or between the limits $x=0$ and $x=\frac{P}{c}$

$$D=\frac{P^2}{2c}. \qquad (1)$$

In like manner the statical work S is

$$S=\int Pdt \quad \text{or as} \quad P=c't$$

where c' is the value of P when t is unity—

$$S=\int c'tdt=\frac{c't^2}{2}+c'''.$$

or between the limits $t=0$ and $t=\frac{P}{c^1}$

$$S=\frac{P^2}{2c'} \qquad (2)$$

The value of D is measured in kilogram-meters; the value of S is measured in kilogram-seconds. It will be admitted that the statical work of holding a weight of w. kilos upon the out-stretched arm, is equivalent to the dynamical work of lifting w. kilos through a certain space. Calling the total work W, we shall have—

$$W=D+k.S. \qquad (3)$$

I propose to call k the dynamical equivalent of statical work. No value has yet been obtained for it which can be deemed more than approximate.

In a series of experiments still in my possession, in which the object was to find the relation of the strength (s) to the dynamical work of exhaustion, P was taken provisionally as a measure of the strength. Its variation from day to day is shown graphically on the chart. My experiments are represented by curve F, F; those of Mr. Myers by curve E, E. The strength is laid off on the ordinates. Time is in days laid off on the axis of abscissa. The values of (s) determined on the same day are represented by the small circles which lie in the same vertical.

The second series consisted in finding the dynamical work done before exhaustion, in lifting a weight of 5·0 kgr. through a height of 0·7 meters in 1·25 seconds. The manner of experiment has already been described in former papers.* The results of these experiments are shown on the chart. The broken line A B is the series of constant experiments given in the first table, on page 133 of this Journal, for Feb., 1875. The dates of the individual experiments are not known, but no two experiments were separated from each other by an interval of more than two days. Soon after this series was finished, the series represented by the line B C was begun, and these experiments are accurately represented in time. The ordinates represent the number (n) of times the weight was lifted before exhaustion, and are laid off on the scale to the left of the chart. D D represents the experiments of Mr. Myers with the same weight, laid off on the scale to the right of the chart.

The third series was made with a view of finding the relation of strength to statical work of exhaustion. The statical work consisted in holding a weight of 5·0 kgr. upon the horizontal arm, the position of the arm being the same as before. The time (t) of exhaustion from day to day (in seconds) is shown in the broken lines G, G and K, K the former being my own experiments, the latter those of Mr. Myers.

During the time of these experiments the muscles were kept in gentle training by daily exercises on the swinging rings and parallel bars. This exercise was taken every day, whether experiments were made or not,† and was found very beneficial in reducing the daily variations in strength. On stopping the experiment for a few days the decrease in the power of the muscle is very apparent, and the influence appears most marked upon the observations of the dynamometer. This is due to a difference in training, and it is one of the most difficult points in the whole subject. Two different muscles may have equal strength when measured with the dynamometer, and yet one may be capable of doing twice as much dynamical work as the other with a moderately light weight. This makes it necessary that such experiments should be made on trained muscles.

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* This Journal, Feb., 1875, pp. 130–137. Nature, vol. xi, p. 276.

† The time of the summer vacation excepted.

It will be seen that Mr. Myers is considerably stronger than I, and that variations in the amount of work done are greater for him than for me. This means that variations in strength cause greater variations in the work done by him than in my own case. This is entirely in harmony with the equation given at the close of my paper in this Journal, February, 1875.

During the whole of this series of experiments the size of the muscles did not perceptibly change, while in Myers' experiments the number of lifts with a 5 kgr. weight varied, in two successive experiments (Dec. 20 and 23) from 420 to 1,300.

On the 29th of April, 1873, after a month of exceedingly severe mental work, I was present at a terrible accident, occasioned by the burning of a building. For a quarter of an hour I was under severe mental strain and was for a week completely prostrated. The urinary deposits were enormous, and the condition of the nerves may be inferred from a specimen signature executed, May 6th, after having run violently up a short flight of stairs. Out of about 30 signatures made at that time this was the best. My ordinary penmanship is shown in the signature below, executed on recovery. While in this condition a few experiments were made with the dynamometer, and in lifting the 5·00 kgr. weight, they lie between the dates, April 29 and May 8, inclusive, and are limited by the points marked *a–b* on diagrams F, F and B, C. It will be observed that here, also, the observations with the dynamometer are most affected, while the dynamical work is not perceptibly affected.

Two important results follow from this investigation:

1. After the relation of the strength of a muscle to the dynamical work of exhaustion has been determined, the strength at any time will be most accurately determined, by measuring the dynamical work of exhaustion. On days when from any cause the muscles are temporarily weak, the strength as determined by the dynamometer, and the work of exhaustion with very heavy weights, is less. In exhaustion with lighter weights, however, (5· kgr.) the exercise of the first part of the experiment appears to invigorate the muscle, and the influence of temporary weakness, due to errors in diet, or lack of exercise, or the oppressive atmosphere of the room, is eliminated.

2. The coefficient of muscular power per square centimeter of section of the muscle, is a quantity which varies greatly with different muscles, and with the same muscle at different times; or in other words, the work which a muscle can perform, depends not only upon its size, but also upon its quality.

This helps to explain the different results arrived at in the determination of the coefficient of contraction of different muscles. Thus, the following values in kgrs. per sq. cm. have been found by various experimenters.*

1.	Flexors of arm,	8·99	(Henke.)
2.	" " "	8·19	"
3.	" " "	6·67	(Haughton.)
4.	Extensors of foot,	5·90	(Henke.)
5.	" " "	11·60	(Koster.)
6.	Flexors of leg,	7·78	(Haughton.)

Haughton has pointed out other reasons for such differences, but one of the most important reasons is found in the amount of training which the muscle has received. Hence, muscles which are seldom called into action, have not the same contracting power as those which are daily used.

The experiments here described, as well as those before given in this Journal, were performed while I was an assistant in the laboratory of Prof. Gustavus Hinrichs, to whose kindness I am under many obligations. I also take pleasure in acknowledging experimental aid from Prof. W. C. Preston and Mr. D. A. Myers.

St. Louis, June 15th, 1875.

ART. XLII.—*Studies on Magnetic Distribution;* by HENRY A. ROWLAND, of the Johns Hopkins University, Baltimore.†

PART I.—*Linear Distribution.*

CONTENTS.—I. Preliminary Remarks; II. Mathematical theory; III. Experimental methods for linear distribution; IV. Iron rods magnetized by induction; V. Straight electromagnets and permanent steel magnets; VI. Miscellaneous applications.

I.

In a paper of mine published about two years ago, I alluded to some investigations which I had made in 1870 and 1871 on the distribution of magnetism. It is with diffidence that I approach this subject, being aware of the great mathematical difficulties with which it is surrounded. But as the facts are still in advance of what is known on the subject, and as I see that other investigators‡ are following hard upon my footsteps, I thought it would be well to publish them, particularly as it is no fault of mine that they did not appear some years ago.§

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* Haughton's "Animal Mechanics." London, 1873, p. 70.

† Communicated by the author.

‡ Particularly M. Jamin.

§ All the experiments referred to in this paper were made in the winter of 1870–71.

The mathematical theory which I give, although not particularly elegant, will at least be found to present the matter in a new and more simple light, and may be considered simply as a development of Faraday's idea of the analogy between a magnet and a voltaic battery immersed in water. I shall throughout speak of the conduction of, and resistance to, lines of magnetic force, and shall otherwise treat them as similar to lines of conducted electricity or heat, it now being well established from the researches of Professor Maxwell and others that this method gives exactly the same results as the other method of considering the action to take place at a distance.

In arranging this paper I have thought best to give the theory of the distribution first, and then afterward to see how the results agree with experiment; in this way we can find out the defects of the theory, and what changes should be made in it to adapt it to experiment.

At present I am acquainted with two formulæ giving the distribution of magnetism on bar magnets: the first was given by Biot, in his *Traité de Physique Expérimentale et Mathématique*, vol. iii, p. 77, and was obtained by him from the analogy of the magnet to a dry electric pile, or to a crystal of tourmaline electrified by heat. He compared his formula with Coulomb's observations, and showed it to represent the distribution with considerable accuracy. Green, in his "Essay," has obtained a formula which gives the same distribution, but he obtains it by a series of mathematical approximations which it is almost impossible to interpret physically. M. Jamin has recently used a formula of the same form, but I have as yet been unable to find how he obtained it. My own formulæ are also quite similar to these, but have the advantage of being obtained in a more simple manner than Green's, and what is of more consequence, all the limitations are made at once, after which the solution is exact; so that although they are only approximate, yet we know just where they should differ from experiment.

II.

If we take an iron bar and magnetize one end of it either by a magnet or helix, we cause lines of magnetic induction* to enter that end of the bar, and after passing down it to a certain distance to pass out into the air and so around to the bar again to complete their circuit. At every part of their circuit they encounter some resistance, and always tend to pass in that direction where it is the least; throughout their whole course they obey a law similar to Ohm's law, and the number of lines passing in any direction between two points is equal to the difference of magnetic potential of those points divided by the resistance to the lines.

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* For difference between lines of magnetic force and lines of magnetic induction See Maxwell's "Treatise on Electricity and Magnetism," arts. 400, 592, and 604.

The complete solution of the problem before us being impossible, let us limit it by two hypotheses. First, let us assume that the permeability of the bar is a constant quantity; and secondly, that the resistance of the lines of induction is composed of two parts, the first being that of the bar, and the second that of escaping from the bar into the medium, and that the latter is the same at every part of the bar. The first of these assumptions is the one usually made in the mathematical theory of magnetic induction; but, as has been shown by the experiments of Müller, and more recently by those of Dr. Stoletow and myself, this is not true, and we shall see this when we come to compare the formula with experiment. The second assumption is more exact than the first for all portions of the bar except the ends.

Let us first take the case of a rod of iron with a short helix placed on any portion of it, through which a current of electricity is sent. The lines of magnetic induction stream down the bar on either side; at every point of the bar two paths are open to them, either to pass further down the rod, or to pass out into the air. We can then apply the ordinary equations for a derived circuit in electricity to this case.

Let μ be the magnetic permeability of the iron,

R be the resistance of unit of length of the rod,

R′ be the resistance of medium along unit of length of rod,

ρ be the resistance at a given point to passing down the rod,

s be the resistance at the end of the rod,

Q′* be the number of lines of induction passing along the rod at a given point,

Q'_e*† be the number of lines of induction passing from the rod into the medium along a small length of the rod ΔL,

L be the distance from the end of the rod to a given point,

$$r=\sqrt{\frac{R}{R'}},$$

$$A=\frac{\sqrt{RR'}+s}{\sqrt{RR'}-s}.$$

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* These are the surface-integrals of magnetic induction (see Maxwell's "Electricity," art. 402); the first across the section of the bar, and the second along a length ΔL of the surface of the bar.

† It is to be noted that Q'_e, when ΔL is constant, is nearly proportional to the so-called surface-density of magnetism at the given point.

To find ρ, the ordinary equation for the resistance of a derived circuit gives

$$\rho+d\rho=\frac{(\rho+\mathrm{R}d\mathrm{L})\frac{\mathrm{R}'}{d\mathrm{L}}}{\rho+\mathrm{R}d\mathrm{L}+\frac{\mathrm{R}'}{d\mathrm{L}}},$$

whence $$\frac{d\rho}{d\mathrm{L}}=\frac{1}{\mathrm{R}'}(\mathrm{RR}'-\rho^2),$$

and $$\rho=\sqrt{\mathrm{RR}'}\frac{\mathrm{A}\varepsilon^{2r\mathrm{L}}-1}{\mathrm{A}\varepsilon^{2r\mathrm{L}}+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

To find Q′, we have $$d\mathrm{Q}'=\frac{\mathrm{Q}'\rho}{\mathrm{R}'}d\mathrm{L},$$

whence $${}^*\mathrm{Q}'=\frac{\mathrm{C}}{\mathrm{A}+1}(\mathrm{A}\varepsilon^{r\mathrm{L}}+\varepsilon^{-r\mathrm{L}}), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

and $$\mathrm{Q}'\varepsilon=\frac{\mathrm{Q}'\rho\Delta\mathrm{L}}{\mathrm{R}'}=r\frac{\mathrm{C}\Delta\mathrm{L}}{\mathrm{A}+1}(\mathrm{A}\varepsilon^{r\mathrm{L}}-\varepsilon^{-r\mathrm{L}}). \quad . \quad . \quad . \qquad (3)$$

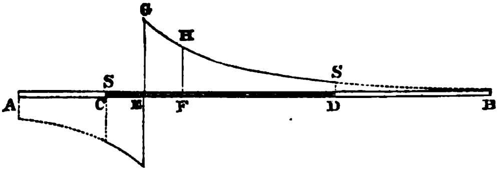
When L is very large, or $s=\sqrt{\mathrm{RR}'}$, we have

$$\mathrm{Q}'=\mathrm{C}_{,}\varepsilon^{r\mathrm{L}_{,}}, \text{ and } \mathrm{Q}'_{\varepsilon}=\mathrm{C}_{,}r\Delta\mathrm{L}\varepsilon^{r\mathrm{L}_{,}},$$

in which L, is reckoned from an origin at any point of the rod.

These equations give the distribution on the part ouside the helix, and we have now to consider the part covered by the helix. Let us limit ourselves to the case where the helix is long and thin, so that the field in its interior is nearly uniform.

1



As we pass along the helix, the change of magnetic potential due to the helix is equal to the product of the intensity of the field multiplied by the distance passed over; so that in passing over an elementary distance dy the difference of potential will be $\mathfrak{H}dy$. The number of lines of force which this difference of potential causes in the rod will be equal to $\mathfrak{H}dy$ divided by the sum of the resistances of the rod in both directions from the given point. These lines of force stream down the rod on either side of the point, creating everywhere a magnetic potential which can be calculated by equation (2), and which is represented by the curves in fig. 1. In that figure AB is the rod, CD the helix, and E the element of length dy. Now if we take all the elements of the rod in the same way and consider the effect at HF, the total magnetic potential at this point will, by hypothesis No. 1, be equal to the sum of the potentials due to all the elements dy.

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* This could have been obtained directly from the equation $\frac{d^2\mathrm{Q}}{d\mathrm{L}^2}=\mathrm{Q}'r^2$, and $\mathrm{Q}'_{\varepsilon}$ from the equation $\mathrm{Q}'_{\varepsilon}=\frac{d\mathrm{Q}'}{d\mathrm{L}}\Delta\mathrm{L}$.

Let $\Delta Q'$ be the number of lines of force produced in the bar at the point E due to the elementary difference of potential at that point, $\mathfrak{H}dy$,

$\Delta Q''$ be the number of lines of force arriving at the point F due to the same element,

Q''_ε be the number of lines passing from bar along length ΔL,

ρ_y be the sum of the resistances of the bar in both directions from E,

ρ_x be resistance at F in direction of D,

y be the distance DE,

x be the distance DF,

b be the distance CD,

s'' and s' be the resistance of the bar, &c., respectively at C in the direction of A, and at D in direction of B.

$\mathfrak{H}$ be the magnetizing-force of helix in its interior,

Let

$$A'=\frac{\sqrt{RR'}+s'}{\sqrt{RR'}-s'};\ A''=\frac{\sqrt{RR'}+s''}{\sqrt{RR'}-s''};\ r=\sqrt{\frac{R}{R'}};$$

$$\rho_y=\frac{2(A'A''\varepsilon^{2rb}-1)}{(A''\varepsilon^{2ry}+1)\ (A'\varepsilon^{2r(b-y)}+1)}\sqrt{RR'},$$

$$\Delta Q'=\frac{\mathfrak{H}dy}{\rho_y},$$

$$\Delta Q''=\Delta Q'(A''\varepsilon^{rx}+\varepsilon^{-rx})\frac{\varepsilon^{ry}}{A''\varepsilon^{2ry}+1},$$

$$\Delta Q''=\frac{\mathfrak{H}}{2\sqrt{RR'}}\ \frac{A''\varepsilon^{rx}+\varepsilon^{-rx}}{A'A''\varepsilon^{2rb}-1}(A'\varepsilon^{2r(b-y)}+1)\varepsilon^{ry}dy,$$

$$Q''_\varepsilon=\frac{\Delta L}{R'}\int\Delta Q''\rho_x=\frac{\mathfrak{H}\Delta L}{2R'}\ \frac{A''\varepsilon^{rx}-\varepsilon^{-rx}}{A'A''\varepsilon^{2rb}-1}\int_x^b(A'\varepsilon^{2rb}\varepsilon^{-ry}+\varepsilon^{ry})dy,$$

$$Q''_\varepsilon=\frac{\mathfrak{H}\Delta L}{2R'r}\ \frac{A''\varepsilon^{rx}-\varepsilon^{-rx}}{A'A''\varepsilon^{2rb}-1}\ [A'\varepsilon^{2rb}(\varepsilon^{-rx}-\varepsilon^{-rb})+\varepsilon^{rb}-\varepsilon^{rx}].\quad .\quad .\quad (4)$$

This gives the positive part of Q''_ε. To find the negative part, change x into $b-x$, A' into A'' and A'' into A', and then change the sign of the whole.

When the helix is symmetrically placed on the bar, we have $s'=s''$, $A'=A''$; whence, adding the positive and negative parts together, we have

$$Q''_\varepsilon=\frac{\mathfrak{H}\Delta L}{2\sqrt{RR'}}\,\frac{1-A'}{A'\varepsilon^{rb}-1}\left(\varepsilon^{r(b-x)}-\varepsilon^{rx}\right), \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

which gives the number of lines of induction passing out from the rod along the length ΔL when the helix is symmetrically placed on the rod.

To get the number of lines of induction passing along the rod at a given point, we have

$$Q''=\int_0^x Q''_\varepsilon\,dx=\frac{\mathfrak{H}}{2R}\,\frac{1-A'}{A'\varepsilon^{rb}-1}\left(\varepsilon^{rb}+1-\varepsilon^{rx}-\varepsilon^{r(b-x)}\right)+C''', \qquad (6)$$

where $$C'''=-\frac{\mathfrak{H}}{r}\,\frac{\varepsilon^{rb}-1}{(A'\varepsilon^{rb}-1)(\sqrt{RR'}-s')}.$$

When the bar extends a distance L' out of both ends of the helix, so that

$$s'=\sqrt{RR'}\,\frac{\varepsilon^{2rL'}+1}{\varepsilon^{2rL'}-1} \text{ and } A'=-\varepsilon^{2rL'},$$

we have $$C'''=-\frac{\mathfrak{H}}{r}\,\frac{(\varepsilon^{rb}-1)(\varepsilon^{2rL'}-1)}{(\varepsilon^{rb}\varepsilon^{2rL'}+1)2\sqrt{RR'}}.$$

It may be well, before proceeding, to define what is meant by magnetic resistance, and the units in which it is measured. If μ is the magnetic permeability of the rod, we can get an idea of the meaning of magnetic resistance in the following manner. Suppose we have a rod infinitely long placed in a magnetic field of intensity $\mathfrak{H}$ parallel to the lines of force. Let Q' be the number of lines of inductive force passing through the rod or the surface-integral of the magnetic induction across its section; also let a be the area of the rod. Then by definition $\mu=\frac{Q'}{a\mathfrak{H}}$. If L is the length of the rod, the difference of potential at the ends will be $L\mathfrak{H}$; hence

$$Q'=\frac{L\mathfrak{H}}{R_{,}} \text{ and } R_{,}=\frac{L\mathfrak{H}}{Q'}=\frac{L}{a\mu},$$

and R in the formulæ becomes

$$R=\frac{R_{,}}{L}=\frac{1}{a\mu}.$$

It is almost impossible to estimate R′ theoretically, seeing that it will vary with the circumstances. We can get some idea of its nature, however, by considering that the principal part of it is due to the cylindric envelope of medium immediately surrounding the rod. The resistance of such an envelope per unit of length of rod is

$$\frac{1}{2\pi\mu_{\prime}} \text{ hyp. log } \frac{\mathrm{D}}{d},$$

where D is the diameter of the envelope, d of the rod, and $\mu_{\prime}$ the permeability of the medium. We are not, however, able to estimate D. If, however, we have two magnetic systems similar in all their parts, it is evident that beyond a certain point similarly situated in each system we may neglect the resistance of the medium, and $\frac{\mathrm{D}}{d}$ will be the same for the two systems. Hence R′ is approximately constant for rods of all diameters in the same medium, and r takes the form

$$r = \frac{2}{d}\sqrt{\frac{1}{\pi\mu\mathrm{R}'}}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

It is evident that the reasoning would apply to rods of any section as well as circular.

In Green's splendid essay (reprint, p. 111, or Maxwell's 'Treatise on Electricity and Magnetism,' art. 439) we find a formula similar to equation (5), but obtained in an entirely different manner, and applying only to rods not extending beyond the helix. In the 'Reprint' β corresponds to my r, and its value, using my notation, is obtained from the equation

$$\cdot 231863 - 2 \text{ hyp. log } p + 2p = \frac{4}{(\mu-1)p^2}, \quad . \quad . \qquad (8)$$

where $p = \frac{rd}{2}$.

If we make μ a constant in this formula, we must have $p = \frac{rd}{2} = \text{constant}$, hence $r \propto \frac{1}{d}$, which is the same result for this case as from equation (7).

When μ in the two formulæ is made to vary, the results are not exactly the same; but still they give approximately the same results for the cases we shall consider; and since the formula is at the best only approximate, we shall not spend time in discussing the merits of the two.

III.

Among the various methods of measuring linear magnetic distribution, we find few up to the present time that are satisfactory. Coulomb used the method of counting the number of vibrations made by a magnetic needle when near various points of the magnet. Thus in the curve of distribution most often reproduced from his work, he used a magnetized steel bar 27

French inches long and two lines in diameter placed vertically; opposite to it, and at a distance of eight lines, he hung a magnetic needle 3 lines in diameter and 6 lines long, tempered very hard, and the number of oscillations made by it was determined. The square of this number is proportional to the magnetic field at that point, supposing the magnetism of the needle to be unchanged; and this, corrected for the magnetism of the earth, gives the magnetic field due to the magnet alone. This for points near the magnet and distant from the ends is nearly proportional to the so-called magnetic surface-density opposite the point. At the end Coulomb doubled the quantity thus found, seeing that the bar extended only on one side of the needle.

It will be seen that this method is only approximate, and almost incapable of giving results in absolute measure. The effect on the needle depends not only on that part of the bar opposite the needle, but on portions to either side, and gives, as it were, the average value for some distance; in the next place, the correction at the end, by multiplying by two, seems to be inadequate, and gives too small a result compared with other parts. For at points distant from the end the average surface-density at any point will nearly be equal to the average for a short distance on both sides, while at the end it will be greater than the average of a short distance measured back from the end. To these errors must be added those due to the mutual induction of the two magnets.

The next method we come to is that which has been recently used by M. Jamin, and consists in measuring the attraction of a piece of soft iron applied at different points of the magnet. In this case it does not seem to have been considered that the attraction depends not only on the magnetic density at the given point, but also on that around it, and that a piece of soft iron applied to a magnet changes the distribution immediately at all points, but especially at that where the iron is applied. The change is of course less when the magnet is of very hard steel and the piece of soft iron small. Where, however, we wish to get the distribution of soft iron, it becomes a quite serious difficulty. Another source of error arises from the fact that the coefficient of magnetization of soft iron is a function of the magnetization: this source of error is greatest where the contact-piece is long and thin, and is a minimum when it is short and thick and not in contact with the magnet. Hence this method will give the best results when the contact-piece is small and in the shape of a sphere and not in contact with the magnet, and when the method is applied to steel magnets. But after taking all these precautions, the question next arises as to how to obtain the magnetic surface-density from the experiments. Theory indicates, and M. Jamin has assumed, that the attractive force is nearly proportional to the square of the surface-density. But experiment does not seem to confirm this, except where there is some distance between the two bodies, at least in the case of a sphere and a plane surface, as in Tyndall's experiments (Phil. Mag., April, 1851). It is not necessary at present to consider the cause of this *apparent* discrepancy between theory and experiment; suffice it to say that the explanation of the phenomenon is without doubt to be sought for in the variable character of the magnetizing function of iron. All I wish to show is that the attraction of iron to a magnet, especially when the two are in contact, is a very complicated phenomenon, whose laws in general are unknown, and hence is entirely unsuitable for experiments on magnetic distribution.

A third method is that used in determining the correction for the distribution on the magnets in finding the intensity of the earth's magnetism. Usually the distribution is not explicitly found in this case; but it is easy to see how it might be. Thus one way would be as follows: Take the origin of coördinates at the center of the magnet. Develop the distribution in an ascending series of powers of x with unknown constant coefficients. Calculate the magnetic force due to this distribution for any points along the axis, or else on a line perpendicular to the magnet at its center. Determine the force at a series of points extending through as great a range and as near the magnet as possible. These experiments gave a series of equations from which the coefficients in the expansion can be determined. Other and better methods of expansion might be found except for short magnets, where the method suggested is very good.

The similarity of this method to that used by Gauss in determining the distribution on the earth is apparent.

A fourth method is similar to the above, except that the direction of the lines of force around the magnet are measured and calculated instead of the force.

The last two methods are very exact, but are also very laborious, and therefore only adapted to special investigations. Thus, by the change in direction of the lines of force around the magnet, we have a delicate means of showing the change in distribution, as, for instance, when the current around an electro-magnet varies.

The fifth method is that used lately in some experiments of Mr. Sears (this Journal, July, 1874), but only adapted to temporary magnetization. At a given point on the bar a small coil of wire is placed, and the current induced in it measured by the swing of the galvanometer-needle when the bar is demagnetized. It does not seem to have been noticed that what we ordinarily consider as the magnetic distribution is not directly measured in this way; and indeed, to get correct results, the magnetization should have been reversed, seeing that a *large portion* of the magnetization will not disappear on taking away the magnetizing-force where the bar is long. The quantity which is directly measured is the surface-integral of the temporary magnetic induction *across the section* of the bar, while the magnetic surface-density is proportional to the surface-integral of magnetic induction *along a given portion of the bar.* In other words, the quantity measured is Q instead of $\frac{Q_\varepsilon}{\Delta L}$. We can, however, derive one from the other very easily.

The sixth and last method is that which I used first in 1870, and by which most of my experiments have been performed. This consists in sliding a small coil of wire, *which just fits the bar and is also very narrow*, along the bar inch by inch, and noting the induced current over each inch by the deflection of a galvanometer-needle. This measures Q_ε, except for some corrections which I now wish to note. In the first case, to give exact results, the lines of force should pass out perpendicular to the bar, or the coil must be very small. But even when the last condition is fulfilled errors will be introduced at certain portions of the bar. The error is vanishingly small in most cases, except near the ends, and even there it is not large except in special cases; for at this part the lines of force pass forward toward the end of the bar, and so the observation next to the end may be too small, while that at the end is too large. The correction can be made by finding where the lines of force through the center of the section of the coil in its two positions meet the bar. The error from this source is not large, and may be avoided to a great extent.

One very great advantage in the method of induced currents is the facility with which the results can be reduced to absolute measure by including an earth-inductor in the circuit as I have before described (Phil. Mag., August, 1873.) There is also no reaction (except a temporary one) between the magnet and current, so that the distribution remains unchanged. Hence it seems to me that this method is the only one capable of giving exact results directly.

The coils of wire which I used consisted of from twenty to one hundred turns of fine wire wound on thin paper tubes which just fitted the bar and extended considerably beyond the coils. The width of the coils was mostly from ·1 to ·25 of an inch wide and from ·1 to ·2 inch thick. A measure being laid by the side of the given bar under experiment, the coil was moved from one division of the rule to the next very quickly, and the deflection produced on an ordinary astatic galvanometer noted. After experience this could be done with great accuracy. It might be better in some cases to have the coil slide over a limited distance on the tube, though, for the use I intend to put the results to, the other is best

Up to 35° Q_ε is nearly proportional to the deflection; and when any larger value is put down on the Tables, it is the sum of two or more deflections. I have not the data in most cases to reduce my results to absolute measure, but took pains to insure that certain series of experiments should be comparable among themselves.

Having measured Q_ε at all points of a rod, we may find Q by adding up the values of Q_ε from the end of the rod.

The magnetizing-force to which the bar was subjected was in all cases a helix placed at some part of the bar. The iron bars were of course demagnetized thoroughly before use by placing them in the proper position with reference to the magnetic meridian and striking them.

In the Tables L is the distance in inches from the zero-point, Q_ε is the deflection of the galvanometer when the helix is passed between the points indicated in the first column. Thus in Table II, 34·7 is the deflection on the galvanometer when the helix was moved from the tenth to the eleventh inch from the zero-point, and so we may consider it as the value of Q_ε at 10½ inches; so that the values of Q_ε refer to the half inches, but Q to the even inches.

In all the calculations the constants in the formulæ were taken to represent Q most nearly, and then the corresponding formulæ for Q_ε taken with the same constants.

For ease in calculating by ordinary logarithmic Tables, we may put $\varepsilon^{rL}=10^{\cdot 4343L}$.

[To be continued.]

ART. XLIII.—*The Effect of the Glacial Epoch upon the Distribution of Insects in North America*; by AUG. R. GROTE, A.M.

(Read before the American Association for the Advancement of Science, at Detroit, Aug. 10th.)

FROM the condition of an hypothesis the Glacial period has been elevated into that of a theory by the explanations it has afforded of a certain class of geological phenomena. The present paper endeavors to show that certain zoological facts are consistent with the presence, during past time, of a vast progressive field of ice, which, in its movement from north to south, gradually extended over large portions of the North

American continent. These facts, in the present instance, are furnished by a study of our Lepidoptera, or certain kinds of butterflies and moths now inhabiting the United States and adjacent territories. Before proceeding with the subject, a brief statement of the phenomena assumed to have attended the advent of the Glacial period is necessary.

At the close of the Tertiary, the temperature of the earth's surface underwent a gradual change by a continuous loss of heat. The winters became longer, the summers shorter. The tops of granitic mountains in the east and west of the North American continent, now in summer time bare of snow and harboring a scanty flora and fauna, became, summer and winter, covered with congealed deposits. In time the mountain snows consolidated into glacial ice, which flowed down the ravines into the valleys. Meanwhile the northern regions of the continent, which may have inaugurated the conditions, submitted extendedly to the same phenomena. Glacial ice, first made on elevations, finally formed at, and poured over, lower levels. Glacial streams finally united to form an icy sea, whose frozen waters slowly plowed the surface of the rocks, and, in their movement from north to south, absorbed the local glacial streams in their course, and extended over all physical barriers. The Appalachians and Rocky Mountains are supposed to have had local glaciers. The animals must always have retreated before this frozen deluge. The existing insects of the Pliocene, in submitting to the change of climate which accompanied the advance of the glacier, must have quitted their haunts with reluctance, and undergone a severe struggle for existence, no matter how gradually they had been prepared for the encounter. We may expect that multitudes of specific formsultimately perished, of whose remains no traces have been perserved.

After this brief statement of the outlines of the opening of the Glacial period, we turn to some facts offered by a study of certain of our existing species of butterflies and moths.

The tops of the White Mountains and the ranges of mountain elevations in Colorado offer us particular kinds of insects, living in an isolated manner at the present day, and confined to their respective localities. In order to find insects like them we have to explore the plains of Labrador and the northern portion of the North American continent, in regions offering analogous conditions of climate to those existing on the summits of these mountains. The genera *Oeneis* and *Brenthis* among the Butterflies, and *Anarta* and *Agrotis* among the Moths, are represented by the same or similar species in all of the above mentioned localities. In the case of the White Mountain butterfly, *Oeneis semidea*, we have a form sustaining itself on a very limited Alpine area on the top of Mount Washington.* Although there is some doubt whether precisely the same form of *Oeneis* has been discovered in Colorado, the fact remains that *Oeneis*, butterflies exceedingly like it, though registered by us under different specific names, live in Labrador and Colorado. Whether the White Mountain butterfly, *Oeneis semidea*, be, as suspected by Lederer, a modification of some of the Labradorian forms of the genus, or not, the geographical distribution which its genus enjoys cannot be meaningless. The question comes up, with regard to the White Mountain butterfly, as to the manner in which this species of *Oeneis* attained its present restricted geographical area—How did the White Mountain butterfly get up the White Mountains? And it is this question that I am disposed to answer by the action attendant on the decline of the Glacial period.

I have before briefly outlined the phenomena attendant on the advance of the ice-sheet, and I now dwell for a moment on the action which must equally be presumed to have accompanied its retirement. Many of the features of its advance were repeated, in reverse order, on the subsidence of the main ice-sheet or glacial sea. The local glaciers appeared again, separate from the main body of ice, and filled the valleys and mountain ravines, thus running at variance with the main body of the glacier, being determined by local topography A reversal of the temperature shortened the winters and lengthened the summers. Ice-loving insects, such as our White Mountain butterfly, hung on the outskirts of the main ice-sheet, where they found their fitting conditions of temperature and food. The main ice-sheet had pushed them insensibly before it, and during the continuance of the Glacial period, the geographical distribution of the genus *Oeneis* had been changed from a high northern region to one which may well have included portions of the Southern States. And, on its decline, the ice-sheet drew them back again after itself by easy stages; yet not all of them. Some of these butterflies strayed by the way, detained by the physical nature of the country and destined to plant colonies apart from their companions. When the main ice-sheet left the foot of the White Mountains, on its long march back to the pole, where it now seems to rest, some of these wayward, flitting *Oeneis* butterflies were left behind. These had strayed up behind the local glaciers on Mount Washington and so became separate from the main body of their companions, which latter journeyed northward, following the course of the retirement of the main ice-sheet. They had found in elevation their congenial climate, and they have followed this gradually to the top of the mountain, which they have now attained and from which they cannot now retreat. Far off in Labrador the descendants of their ancestral companions fly over wide stretches of country, while they appear to be in prison on the top of a mountain. I conceive that in this way the mountains may generally have secured their alpine animals. The Glacial period cannot strictly be said to have expired. It exists even now for high levels above the sea, while the Esquimaux finds it yet enduring in the far north. Had other conditions been favorable, we might now find Arctic man living on snow-capped mountains within the Temperate zone.

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* See Mr. Scudder's article in the "Geology of New Hampshire," i. 342. Mr. Scudder first pointed out the existence of Alpine and sub-Alpine faunal belts on Mount Washington, and makes the interesting remark. "that if the summit of Mount Washington were somewhat less than two thousand feet higher, it would reach the limit of perpetual snow."

At a height of from 5,600 to 6,200 feet above the level of the sea, and a mean temperature of about 48 degrees during a short summer, the White Mountain butterflies (*Oeneis semidea*) yet enjoy a climate like that of Labrador within the limits of New Hampshire. And in the case of moths an analogous state of things exists. The species *Anarta melanopa* is found on Mount Washington, the Rocky Mountains and Labrador. *Agrotis Islandica* is found in Iceland, Labrador, the White Mountains, and, perhaps in Colorado. As on islands in the air, these insects have been left by the retiring ice-flood during the opening of the Quarternary.

On inferior elevations, as on Mount Katahdin, in Maine, where we now find no *Oeneis* butterflies, these may formerly have existed, succumbing to a climate gradually increasing in warmth from which they had no escape; while the original colonization. in the several instances, must have always greatly depended upon local topography.

I have briefly endeavored to show, that the present distribution of certain insects may have been brought about by the phenomena attendant on the Glacial period. The discussion of matters connected with this theoretical period of the earth's history thus brings out more and more clearly, as it now appears, the fact of its actuality. I hope that my present statements may draw the attention of our zoologists more to the matter, seeing that we have in our own country fields for its full exploration.

ART. XLIV.—*Æstivation and its Terminology;* by ASA GRAY.

THE term *æstivation*, to denote the arrangement of the parts of the calyx, corolla, &c., in the bud, as well as that of *vernation* for leaves in a leaf-bud, was introduced by Linnæus. He did not elaborate the former subject as he did the latter, and the few terms given to the modes he recognized are for the most part defined merely by a reference to their use in vernation. Æstivation as a botanical character is comparatively recent, and its terminology is not yet quite satisfactorily settled. I propose to consider, 1, what the leading modes are, and 2, how they are to be designated.

1. In the first place, the modes of æstivation may be conveniently divided into two classes, those in which the parts overlap, and those in which they do not.

Of overlapping æstivation, only two principal kinds need be primarily distinguished, viz: 1. where some pieces overlap and others are overlapped, i. e., some have both margins exterior and others both margins interior or covered; 2. where each piece of a circle is overlapped by its neighbor on one side while it overlaps its neighbor on the other. There are mixtures and subordinate modifications of these two, but no third mode.

In æstivation without overlapping, there is first, the rare case in which the parts of the whorl or cycle never come into contact in the bud; and secondly, that in which they impinge by their edges only. There is also the case in which both margins of each piece are rolled or bent inward, and the rarer one in which they are turned outward; and the apex of each piece may comport itself in any of these ways. But these dispositions are those of the pieces or leaves taken separately, and the terms applied to them are the same as in vernation or prefoliation, are used in the same sense, and so are not at all peculiar to æstivation or prefloration. The like may be said of a remaining mode, which belongs, however, to a different category, that in which the parts being united into a tube or cup, this is bodily plaited into folds, or otherwise disposed. In which case the margin of the tube or cup, or such lobes as it may have, may exhibit any of the modes of æstivation above indicated.

Without further notice, then, of this last, the *plicate* or *plaited* æstivation, and of analogous conformations of the tube or cup of a calyx or corolla, or of the disposition of each piece individually (whether *revolute, involute, reflexed, inflexed,* and the like)—about the terminology of which there is no question,—omitting, likewise, for the latter reason, the case of *open* æstivation, there are left three types to deal with:

I. With some pieces of the set wholly exterior in the bud to others.

II. With each piece covered at one margin, and covering by the other.

III. With each piece squarely abutting against its neighbors on either side, without overlapping.

In modes II and III, the pieces are all on the same level and are to be viewed as members of a whorl. In mode I, although they may sometimes be members of a whorl, some parts of which have become external to others in the course of growth, they may, and in many cases must belong either to two or more successive whorls (as in the corolla of *Papaveraceæ*, and even the calyx of *Cruciferæ*, the upper or inner of course covered by the lower or outer), or to the spiral phyllotaxy of alternate leaves.

The type of the latter, and the common disposition when the parts are five, is with two pieces exterior, the third exterior by one edge and interior by the other, and two wholly interior. This is simply a cycle in $\frac{2}{5}$ phyllotaxy, the third piece being necessarily within and covered at one margin by the first, while it is exterior to and with its other margin covers the fifth, this and the fourth being of course wholly interior. So, likewise, when the parts are three, one exterior, one half exterior, and one interior or overlapped, the æstivation accords with $\frac{1}{3}$ phyllotaxy. When of eight or higher numbers the spiral order is usually all the more manifest. When of four or six, the case is one of whorls (opposite leaves representing the simplest whorl), either of a pair of whorls (as in *Epimedium*, *Berberis*, &c.), or a single whorl, the parts of which have overlapped in cyclic order.

2. As to the terminology. Linnæus in the Philosophia Botanica treats only of Vernation, there termed *Foliatio.* For this the former term was substituted, and that of *æstivation* for the disposition of petals in a flower-bud, introduced, as I suppose (not having the volume to consult) in the Termini Botanici, published in the sixth volume of the Amœnitates Academicæ, 1762. I refer to it only through Giseke's edition, 1781. Here the terms are *convoluta*, *imbricata*, *conduplicata*, defined only by reference to the section *vernatio*, and *valvata*, unhappily explained by a reference to the glumes of Grasses, also "*inæquivalvis*; si magnitudine discrepant." *Imbricata* is the only term besides *valvata* which directly relates to the arrangement of petals, &c., *inter se;* and the reference takes us back to something "tectus, ut nudus non appareat," covered as with tiles we may infer. In the Philosophia Botanica, under the section *Foliatio*, the definition of *imbricata* is "quando parallele, superficie recta, sibi invicem incumbunt." This would apply either to mode I, or mode II, according as *invicem* is

understood; but the diagram, tab. x, 6, shows that case I is intended. *Convoluta* refers to the rolling of a petal or leaf by itself, as does *conduplicata* to its folding; but Linnæus gives two figures, one of a single rolled-up leaf, the other of one leaf rolled up within another.

Finally, among the modes of vernation indicated by Linnæus, there is one which it is important here to notice, relating as it does to the arrangement of a pair of leaves in the bud, and evidently quite as applicable to a whorl of a larger number of parts than two, i. e.—

"*Obvoluta*, quum margines alterni comprehendunt oppositi folii marginem rectum." Phil. Bot., 105. Or, in Term. Bot., "pagina superiore lateribus approximatis ita ut alterum latus distinguat alterum folium."

This, as the definition and the diagram in the Philosophia Botanica show, answers in æstivation to mode II. It was early taken up as such by Mirbel (Elem. Phys. Veg. et Bot., 1815, ii, 738, 739), where the polypetalous corolla of *Hermannia* and *Oxalis*, and the gamopetalous corolla of *Apocyneæ* are cited as examples.

Valvate æstivation, our mode III, is rightly defined by Mirbel in the same place, and still earlier by Brown.

Linnæus made no use of æstivation as a character. Nor did Jussieu, except merely that, in his Genera Plantarum, the petals of *Malvaviscus* are said to be *convolute*.

In DeCandolle's Théorié Élémentaire, 1813—a still unsurpassed treatise, upon which, next to the Philosophia Botanica, our botanical glossology rests—neither the word æstivation, nor its synonym, prefloration, is mentioned, and even vernation or prefoliation is equally omitted.

But the history of æstivation as a botanical character began in a work published three years earlier, viz., in R. Brown's Prodromus Floræ Nov. Holl., 1810. The Preface notes that it was first accurately observed by Grew. In it Brown *defines* only the *valvate* mode, "ubi margines foliolorum vel laciniarum integumenti invicem applicati sunt, capsulæ valvularum in modum." In the body of the work, wherever it is important, the æstivation is noted as *valvate*, *imbricate*, *plicate*, *induplicate*, &c.; and the open æstivation (*aperta*) is named by him in a subsequent paper.

Being the first to employ æstivation systematically, and to develop its value, Brown's terminology for its modes may well be considered authoritative. And so indeed it is, as far as it goes. But he did not make one important distinction viz., that between our I and II. Imbricate, in his use, comprises all kinds of overlapping, that of the corolla of *Apocyneæ* and of a Gentian, as well as that of a Primrose. He must have not only noticed the difference, but also appreciated its general importance, notwithstanding the occasional passage of the one into the other. He must have also observed that in many cases, as in *Asclepias* for instance, the mode II passes into mode III, the valvate, and may possibly have discerned that under a phyllotaxic view these are more nearly related than either is to mode I. I find, however, only one instance in which he has indicated the distinction, viz., in the character of *Burchellia*, furnished to the Botanical Register, t. 435, 1820. Of its corolla it is said: "æstivatione mutuo imbricata contorta." The phrase is interesting, as it seems to recognize the distinction between the mode of overlapping (which is that of our mode II) and the torsion, which only now and then accompanies it. Looking over the Plantæ Javanicæ Rariores to see if there is any later use, I find no instance in which Brown has occasion to speak of this mode II; but it occurs in the portion of his associate, Mr. Bennett, who (on p. 212) describes the petals of *Sonerila* as "æstivatione *convoluta*." Had this term been thus employed by Brown himself, and at an earlier date, I should regard the terminology of these three modes of æstivation as settled, viz.: I. *imbricata*, II. *convoluta*, III. *valvata*. The first and the third are established beyond question, although somewhat remains to be said about the first.

But meanwhile another use has prevailed as respects the second. In DeCandolle's Prodromus, the first general or considerable work after Brown in which terms of æstivation are employed, this mode is almost uniformly characterized as *contorta*. I cannot at this moment trace the term to its origin. It was probably suggested by the name *Contortæ*, said to have been given by Linnæus to the Apocyneous natural order; and it seemed appropriate to the instances in which the strong convolution of rounded petals, as in *Oxalis*, or their lobes, as in *Phlox*, give an appearance like that of twisting, although there is no twist or torsion. But it is to just such cases, in which there is most of seeming twisting on account of the strong convolution, that the term *convolute* is now and then assigned in the Prodromus; as in the character of *Byttneriaceæ*, and that of *Malvaviscus*. The latter may perhaps be explained by the peculiarity that the petals do not uncoil in anthesis. But in *Apocynaceæ*, in the Prodromus, the terms *convoluta* and *contorta* are seemingly employed synonymously, or nearly so (the latter most frequently); at least I see no difference between the æstivation of *Allamanda*, said to be contorted, and that of *Vinca* (*rosea*), said to be convolute. Endlicher in this regard follows the Prodromus. In the new Genera Plantarum by Bentham and Hooker this mode is most commonly designated as *contorta*, sometimes as *contorto-imbricata*, rarely (*Philadelphus*, &c.) *convoluta*. I have myself, from a period as early as 1840, employed the term convolute, thinking it unadvisable to have two names for the same thing, and wishing to restrict, if it might be, the term contorted to cases of torsion. Adrien de Jussieu, on the other hand, used convolute (with strict Linnæan propriety) for regular imbrication with a high degree of overlapping, thus giving two names to different degrees of the same thing.

It being conceded, I presume, that the mode II should be specifically distinguished, what name, on the whole, ought it to bear? If we follow prevalent usage, *contorta* will be the term. But this term was unknown in this sense to the founders of æstivation, Linnæus and Brown; it correctly expresses the real state of things in only a few cases; and where there is torsion, it leads to a most awkward way of expressing it. We have to write: "lobes of the corolla contorted and twisted: *corollæ lobi contorti et torti*," introducing *dextrorsum* or *sinistrorsum*,* to express the direction of the overlapping and of the torsion, which are not always the same. So that the most current name is the least appropriate. *Convoluta* is as good a name as can be, and its use in the present sense is not unconformable with the Linnæan use in vernation. When well carried out, three or five or more petals, as the case may be, are simply rolled up together. When the overlapping is slight, there is simply the tendency to convolution. But if, as in other nomenclature, priority gives a paramount claim, *obvoluta* will be the proper term, beginning as it did with Linnæus for vernation, and taken up, as it was very early, by Mirbel for æstivation. The only objections to it are, first, that it has never come into systematic use, and second, that *ob* in the composition of botanical terms, commonly stands for obversely or inversely. But *obvoluta* is not burdened with this signification: it is classical for "wrapped round," as is *convoluta* for rolled together. I conclude that one or the other of these two terms ought to be used.

Finally, although there is little, if any, practical misuse, there is some mis-definition, of the term *imbricate* as applied to æstivation. Adrien de Jussieu defines it well (in Cours Élémentaire, 308) in the phrase "La préfloraison spirale est aussi nommé *imbriquée;*" and in noting that when the number stops at five, the pieces fall into two exterior, two interior, and one (the third in the spiral) intermediate, this making what is called *æstivatio quincuncialis*.† This is clear and to the point. But other authors have had a fancy for distinguishing between quincuncial and imbricate (as if the former were not the typical case of the latter when the parts are five), and so have had to devise something else to answer to imbricate. Alphonse De Candolle (in his Introd. Bot., i, 154, written before phyllotaxy was well understood), after relegating *imbricative* to the category of a crowd of verticils, and remarking that the quincuncial is sometimes confounded with the imbricate, adds: some confound also under this latter name the case in which there is one exterior piece, one interior, and three covered at one margin but free at the other. I know not where this began; but its latest reproduction is in Le Maout and Decaisne's Traité Général, and in the English translation of it. In the diagram the pieces are numbered directly round the circle from 1 to 5, the fifth coming next the first: "so they thus complete one turn of a spiral."—which shows that Le Maout had vague ideas of phyllotaxy, of which he seems to have invented a new ($\frac{1}{5}$) order. Moreover this is essentially identical with the *cochlear* æstivation of the same work (not of Lindley); and Eichler, in his Blüthendiagramme, adopts this name (unsuitable though it be), for this particuliar arrangement, whatever be the position of the enclosed or enclosing petal. A glance shows that this supposed "true imbricate æstivation" is a slight and not very uncommon deviation (by the displacement of what should be the interior margin of one of the petals during growth) of the mode II, variously termed obvolute, convolute, or contorted æstivation. But it is so intermediate between this and the quincuncially imbricate as perhaps to justify Brown in applying the name imbricate generically to all the overlapping modes. I see, since the above was written, that Eichler, in his Blüthendiagramme, in effect does this. I find also, that Eichler uniformly employs the term *convolute*, or *convolutive*, as I have done, instead of contorted. I should hope, rather than immediately expect, that this use would become general.

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* I note with satisfaction that Bentham and Hooker use these terms to signify from left to right, or from right to left, of a person, supposed to stand outside of the closed bud, which is surely the natural position of the observer.

† The name *quincuncial* answers the purpose after definition, and has long been in use; but this arrangement in diagram is wholly unlike the *quincunx*, with its four pieces or stars in the periphery, or at the angles of a square, and one in the center.

ART. XLV.—*Abstract of a Memoir on the "Biological Relations of the Jurassic Ammonites;"* by Professor A. HYATT.*

THE speaker traced the history of the evolution of the order of Ammonoids, showing that the characteristics of the first three stages of the embryo were inherited from a very early period. These were first, the sac-like shell of the embryo containing the equally sac-like beginning of the siphon,—prosiphon as it has since been called by M. Munier-Chalmas; second, the beginning of the true shell or apex, with its nautilus-like septum, and peculiar nautilus-like umbilicus; third, the depressed and goniatite-like continuation of the form fo the shell with its accompanying goniatitic septa.

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* From the Proceedings of the Boston Society of Natural History, vol. xvii, December 16, 1874.

These of course represent only their most advanced stage in the Ammonites proper of the Jura and Trias; they are, when first observed in the Silurian and Devonian, exceedingly variable in the length of the periods and other important characteristics even between the varieties of different species. They become invariable in the young as embryonic characteristics only after the lapse of time represented by the Silurian, Devonian, and Carboniferous periods. This variability in the same species in the Silurian shows how recently they were inherited, and their invariability in every individual of the Jurassic show the result of the long ages of inheritance through which the group has passed between that period and the Silurian epoch.

He then showed that in each subordinate group there were certain invariably occurring forms precisely similar to those found in other groups often widely removed in time and very distinct in the structure of the parts. These are apt to occur with a certain fixity of succession which enables the observer to predict with considerable certainty the general characteristics of the succeeding forms of any given group after he has thoroughly studied the development and succession of a few of the lowest. They correspond to what naturalists are in the habit of calling paralled forms, often also representative forms. These forms begin in every group with which I am acquainted with a certain low or open-whorled form and evolve, in course of time and by inheritance, more and more involved whorls, or else the whorls are modified in the characteristics which usually accompany the normal increase of the involution, namely, by the increasing thinness of the shell laterally, flattening of the sides which becomes more and more convergent outwardly, and the tendency of the abdomen to become narrower. This and the origin of most of the groups from certain single ancestral species of the discoidal or open-whorl forms show conclusively that these forms rise independently in each group.

But it must be noticed that they can be only thus limited in each group or series of groups which are genetically connected. The range of forms comprehends every imaginable modification of the original inherited or stock form of the third stage among Ammonites. This is tubular or coniform and has an inherited tendency to grow by increasing the abdominal more than the dorsal side, thus revolving upon itself. Therefore, while the choice or selection of the original forms by which a series starts into being is practically unlimited except by the possibilities of the typical discoidal form of the embryo and young, the subsequent development in each series becomes more and more limited according to the size of the group. The same law of inheritance which renders the embryonic form of the third stage fixed or invariable in each individual of the true Ammonites of the Jura, subsequently accomplishes the same purpose, to a less degree and with greater fluctuation, for the later developed forms and characteristics of each separate series or group, obliging them to evolve, if they progress at all, a certain succession of forms which have been described above.

It will be noticed that I use the word progress in a special sense as applicable to a certain class of paralled forms and not to those with which we shall presently deal, the old-age forms, which though equally perfect in the phenomena of parallelism, cannot be attributable to growth. The former are the mechanical results of the growth or increase in size of the shell of the common embryonic form of the third and succeeding stages of the young, while the latter result from the natural but inevitable loss of growth-force in the adult shell and its parts.

This growth seems to me to be due to the favorable nature of the physical surroundings, primarily producing characteristic changes which become perpetuated and increased by inheritance within the group. We can recognize this in the constantly increasing size of the shell, complication and development of the new parts, as has been shown by Prof. Cope in his "Method of Creation of Organic Types." Though he does not attribute so much to the influence of the physical surroundings as has been done here, the result of my investigations are, as they have been heretofore, very similar to his.

The law or general expression for the mode of inheritance by which this is accomplished is the same for all characteristics, whether of form or structure; namely, that of acceleration. By this I mean the constant tendency of every individual to inherit the characteristics of its parents at earlier periods than those in which they have appeared in the parents themselves. I know of no exception to this law, whether the characteristics are due to a healthy adult condition or to old age; whether they precede or succeed the supposed period of reproduction. This I have already treated of fully in previous publications, and need only refer to the old parallel forms, presently to be treated of, in order to make it clear to every zoologist that senile characteristics must be inherited or these series of senile parallel forms could have no existence.

This constant tendency to reproduce the ancestral characteristics at earlier and earlier stages accounts for the reduction of the principal characteristics of the Nautiloids and Goniatites to an embryonic condition in the young of the Jurassic Ammonites.

It also accounts for the inheritance of the more and more involved form in each of the subordinate series. This becomes apparent when the parallel forms of any series are traced from the primary discoidal or open umbilicated through the intermediate forms to the most completely involved.

We find in all cases the more discoidal or primary with all its characteristics, whatever they may be, repeated at earlier stages in each species, until at last in some of the most involved, all perceptible traces of its existence are lost. Then and only then can the series be said to die a natural death. When this form appears I have never found another. The reason for this is that in all cases the disappearance of the primary or ancestral form and characteristics of the series is due to the encroachments of the inherited old age characteristics. When these, which are essentially degradational, begin to be inherited in a race, the adult characteristics begin to be confined to younger periods of growth and finally disappear altogether; the shell showing certain old age or inherited senile characteristics from the beginning of the fourth stage. Everywhere this mode of inheritance by acceleration occurs, everywhere it seems to govern the succession of the forms. I have not, however, been able yet to trace the precise connection between all the roots of the secondary series. If this could be completely done, which I fear is impossible at present, no doubt some similar relations would be found.

Besides those parallel forms which may be called progressive, there are others in the same groups which may be shown to be due to the inheritance of the old age of these same parallel forms, and, by comparison with similar forms prematurely produced in different species by disease or local influences, they may be attributed to similar causes, namely, the action of unfavorable surroundings. It is no exaggeration to say that in many instances the small, dwarfed forms produced by disease are very similar to the normal and large old age forms of the same series. This resemblance extends sometimes even to the mode of development. Disease thus produces directly an effect similar to the normal action of the laws of inheritance through a greater or less period of time under the influence of physical surroundings.

The word surroundings is now used instead of environment, for the reason that environment covers the whole ground of physical causes which may have either a remote or immediate effect upon the life of the species.

The environment, or the sum of the physical influences, however favorable it may seem to be, is, as is well known to all phyiologists, perpetually inimical to the prolonged existence of life, and brings about in the individual the retrograde metamorphoses known as old age, and leads to death by disuse, atrophy and decay of the functions and organs.

These changes in the individual are in precise correspondence with those taking place in a group, and, as has been shown, these characteristics are acted upon in their transmission from individual to individual, during the decline of the group, by the same law of inheritance as are the progressive characteristics during its rise. Thus it becomes possible to compare the life of the individual with the life of the group to which it belongs, the period of growth and development to the period of the progressive evolution of new forms, and the period of old age with its retrograde metamorphoses to the period of decline during which retrogressive forms are evolved. This comparison and the facts noted above enable us to attribute the parallel modifications of forms, whether occurring during the progressive or declining period in the existence of a group, to the direct influence of environment.

Besides these characteristic forms and structural parts which are parallel, there are many others in each group not classified under the head of similiarities but under that of differences, in so far as they distinguish the groups from each other. These may be often followed back to varieties of one species, showing that certain varieties have given rise to the groups. These varieties are few as compared with the whole number of varieties traceable in these original ancestral species.

Thus it seems clear, that these varieties must have had certain advantageous peculiarities enabling them to survive the climatic or geological changes, which destroyed the weaker descendants of the same stock, and that these peculiarities rendered them capable of perpetuating their race until they arose into a group or series of genetically connected forms.

Unless the Darwinian law of natural selection, or the survival of the fittest, does apply to the perpetuation of these structural differences which distinguish groups from each other, I am entirely at loss in my attempts to account for them. I here carefully guard against attributing the origin of these differences to the law of natural selection, but limit its action strictly to the modification of the structural differences which tend to appear first in the varieties and then by inheritance in larger and larger groups and at earlier and earlier stages in the life of the individual.

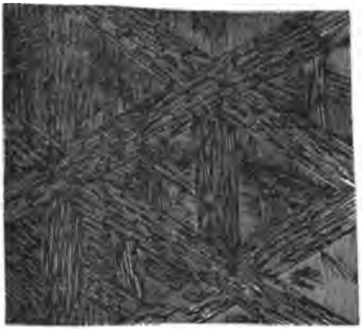
It may also be shown by Cope's law of the origination of differences by growth that the origin of these differences probably lies in some law of growth under the influence of physical surroundings, supply and kind of food, climate, etc. Thus they may be said to be due to growth modified and directed by the Darwinian law of natural selection, both of these being directly subject to the influence of environment, or the sum of all the physical influences brought to bear upon the organization.

This conclusion, it will be noticed, is strictly in accordance with the general tendency of zoological opinions at the present time and almost identical with the results taught by Herbert Spencer in his works on biology, although I was not aware of this until after they were written. Many of the facts supporting the position assumed have already been published in various scattered papers, but those will be united and accompanied by others since discovered in the partially completed memoir of which this is the abstract.

ART. XLVI.—*A Note in relation to the mass of Meteoric Iron that fell in Dickson County, Tenn., in* 1835; by J. LAWRENCE SMITH, Louisville, Ky.

EVERY metallic particle in the interior of a meteoric stone is a complete miniature type of the large masses of meteoric iron which have been discovered in different parts of the world, but not seen to have fallen, leading to the natural conclusion that they must have fallen at periods anterior to the date of their discovery. And it is an interesting fact in celestial meteorology, that the stony meteorites, with their little particles of metal, fall with comparative frequency. Yet the fall of iron masses free from earthy matter is so rare that we have but four authenticated cases: that of Agram in Croatia, in May, 1751, that of Braunau, Bohemia, in July, 1847, that of Victoria, Africa, in 1862, and the one which now forms the subject of this communication, which fell on the 1st of August, 1835, near Charlotte, Dickson County, Tenn., U. S.; lat. 36° 15′, long. 87° 22′. A short description was given by Professor Troost of Nashville, and published in this Journal in 1845. Prof. Troost dying very shortly after that period, his cabinet of minerals and other objects of natural history were placed in boxes by his executors, and have remained thus until within the past few months, when they passed under my control. The scientific world knowing so little of this meteoric iron, I at once proceeded to its examination; and, as only a small part of one end, weighing two or three hundred grams, had been cut off, it was easy to restore that from a drawing, and obtain a perfect cast of the mass, which has been done. My reason for making the present communication is to call attention to the remarkable features of this most interesting meteorite, which, although it is forty years since it fell, has not been seen by a half dozen scientific men.

This meteorite fell during the day-time, in a field where several persons were at work, frightening a horse attached to a plough, who ran wildly about the field dragging the plough after him. It struck the ground at the root of a large oak, descending at rather an acute angle, and burying itself in the roots of the tree. The sky was cloudless and a noise was heard preceded by a vivid light. Other particulars connected with its fall, as well as a description of its size and form, have been already published by Prof. Troost. It is of an elongated kidney shape and remarkably symmetrical form, the metal being bright and almost polished on many parts of the surface, and it has remained in this condition ever since it was discovered, although exposed to such atmospheric conditions as usually rust and tarnish iron; it is in this respect unique among meteoric irons, as well as in another particular first noted by Prof. Troost. Although to the naked eye the surface has the appearance of smooth cast iron, the smoothness of the surface in many parts disappears when examined through a lens: "it is then seen to have a reticulated surface, formed by the edges of thin laminæ of metal, separated from each other by an apparently semi-fused or slaggy matter. These laminæ running in an inclined position into the mass, intersect one another at angles of 60°, and forming equilateral triangles, would divide the mass into regular octahedrons. The accompanying cut will better exhibit these lines very much magnified.



Another noteworthy fact in connection with this iron (which is soft and tough) is that when cut and polished, it will resist the tarnishing effects of the ordinary vapors of the laboratory, as I have pieces which have been thus exposed for several months.

By the agency of heat or acid the Widmannstättian figures are developed with exquisite beauty, not equalled except by three or four known meteoric irons. In connection with these figures I will call attention to the delicate parallel lines inside of these figures, which I pointed out several years ago as being peculiar to certain of the irons, they being not contained in all Widmannstättian figures, and which I designate by the term „ Laphamite markings."

This iron is not absolutely compact, for one can trace, even with the eye, minute cavities which are distinctly visible with a lens; but I have not yet been able to detect any schreibersite either on the surface or in the interior of the mass.

Its specific gravity is 7·717.

On analysis it was found to consist of

Iron	91·15
Nickel	8·01
Cobalt	·72
Copper	·06

No trace of sulphur was detected, and so minute a trace of phosphorus, that only a few exceedingly small crystals of phosphate of magnesia and ammonia could be discovered in the test made with a gram of the iron, representing only a small fraction of a milligram of phosphorus. In fact, I have never yet analyzed a meteoric iron containing so little phosphorus. In regard to the gaseous contents of this iron, the following were the results obtained by Prof. W. Wright, who made an examination of them at my request.

"The iron being exposed to a red heat gave a little more than twice its volume of gas. It can be estimated as 2·2, without an appreciable error. It did not appear to be given off readily, and doubtless a larger portion would have been obtained if the iron had been in a more thoroughly divided state. An analysis of the gas gave

H	71·04
CO	15·03
CO_2	13·03

There did not appear to be any appreciable quantity of nitrogen."

It is a question of no small interest, in connection with the fall of meteoric irons, whether or not they are heated to a sufficient degree of intensity to fuse the surface of the metal. The present meteorite would appear to solve this question in the negative; for if the surface had been melted the delicate reticulated structure, which is discoverable by the glass, would have disappeared, and it would have had an irregular melted exterior. In the present case this oxide exists on the edges and between the striæ; which serves to show that the surface of the iron, although not melted, was nevertheless intensely heated, and had been preserved from fusion only by the rapid conduction of the heat from the circumference to the center. And this should be the case with nearly all, if not all, the masses of iron which have fallen.

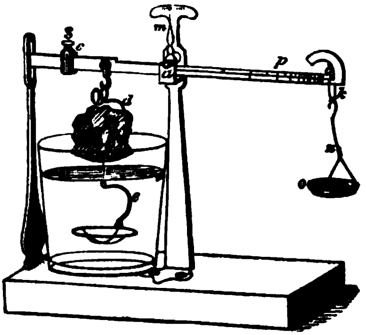
The Braunau iron was not near the point of fusion; otherwise it would have set fire to the rafters of the house in which a part of it was imbedded at the time of its fall, and the surface of that iron precludes the idea of its having been fused. If this generalization of iron be correct, it has an important bearing upon the hypothesis of the manner in which the Ovifak iron (supposing it to be meteoric) penetrated the basalt in scattered particles just at the time of the outflow of the basalt in a plastic state; for if the iron was not melted in its passage through the air, it could not have penetrated the basalt in such a manner that the particles are completely surrounded by terrestrial basalt. This fact in connection with many others lead me more and more strongly to the conviction, in common with some others, that the Ovifak iron is terrestrial.

On the whole, the iron just described is the most interesting specimen of meteoric iron yet known.

ART. XLVII.—*Specific Gravity Balance*; by ROSWELL PARISH.

THE specific gravity balance described below, is intended for the determination of the specific gravities of minerals, and of other solids heavier than water, without the use of exact weights and without mathematical computation.

The construction of this instrument is shown in the following figure. For use, the fine wire loop *k* with its attachments is removed, and the index *m* is adjusted to the middle line by means of the sliding cylinder with screw *c*.



The mineral (or other solid) is now placed in the wire basket *d*, which is suspended from the knife-edge *s* near *c*, and is counterpoised by the wire pan-holder *n*, one or more shallow metallic pans *o*, and a sufficient quantity of sand (or fine copper punchings), suspended by the wire loop *k* at the notch *b*, as shown in the figure.

The mineral is now transferred to the lower basket *e* (suspended from the upper by fine wire, and immersed in water), and the counterpoise *kno* is moved toward *a* until at some point as *p* it restores the beam to a horizontal position.

The specific gravity is then read off by means of the graduation upon the arm *ab*. This arm is graduated in accordance with the following considerations:

If the exact weight of the counterpoise *kno* be represented by x, then

$$\frac{x.ab}{as}=\text{weight of mineral in air.} \qquad (1)$$

$$\text{Also } \frac{x.ap}{as}=\text{weight of mineral in water.} \qquad (2)$$

$$(1)-(2)=\frac{x.ab}{as}-\frac{x.ap}{as}=\frac{x.bp}{as}=\text{weight of water displaced.} \qquad (3)$$

$$(1)\div(3)=\frac{x.ab}{as}\div\frac{x.bp}{as}=\frac{ab}{bp}=\text{specific gravity of mineral.}$$

Hence the specific gravity is known if the ratio $\frac{ab}{bp}$ is known. This ratio is indicated upon the arm *ab* for as many points as possible.

An arm *ab* 12·6 inches long may be graduated to tenths as high as seven, while low specific gravities may be indicated with much greater exactness.

It will be seen that this balance does not determine weight, and that it renders mathematical computation unnecessary.

Worcester, Mass., Aug. 30, 1875.

ART. XLVIII.—*On Southern New England during the Melting of the Great Glacier*; by JAMES D. DANA. No. III.

III.—REINDEERS IN SOUTHERN NEW ENGLAND.

THE beds in the vicinity of New Haven pertaining to the Champlain or Fluvial period of the Quaternary have recently afforded remains of mammals. I am unable to prove positively that the species belonged to the earlier or "Diluvian" part of the period rather than the later or "Alluvian," and yet I deem this so far probable that I make this paper No. III, in the series on "Southern New England during the melting of the great glacier."

In a memoir on "The Geology of the New Haven region,"* I have described the clay deposits of the Quinnipiac valley between New Haven and North Haven; and on page 176 of this volume I have mentioned facts that appear to prove that they are of glacial origin, and were laid down before the glacial flood had reached its climax. As this clay has afforded the bones recently discovered I here briefly repeat the facts respecting its position and relations.

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* Transactions of the Connecticut Academy, vol. ii, 1870, p. 84.

The Quinnipiac valley, south of North Haven village, is to a great extent a region of wet meadows and marshes, nearly five miles long and one broad, deep in peat, and mostly under water at high tide. The clay deposits occur for three and a half miles on both the east and west margins, sometimes extending laterally to the peat region there to stop suddenly, and sometimes graduating into sand-beds before reaching the peat. They are evidently, as I have elsewhere stated, local deposits in what were once still-water areas toward the sides of the wide Quinnipiac basin. The clay-bed seldom reaches more than six feet above high-water level, and is usually covered by four to five or more feet of sand or gravel of the stratified drift. The thickness is over 35 feet at Crafts' clay pit near the Quinnipiac station, two miles south of North Haven, but near the northern limit of the basin, a quarter of a mile south of North Haven, at the Stiles clay pit, only 15 to 18 feet.* The underlying bed, when examined, is of fine sand, called by the workmen "quicksand." The clay is regularly laminated, with the laminæ half an inch to an inch and a half thick; but each lamina really consists of two—one, of the finest "fatty" clay, and the other, lighter in color, of a more or less sandy clay—evidence of alternations of quiet and flowing waters during the progress of the deposition.

Mr. S. P. Crafts, the proprietor of a clay-pit at the village of Quinnipiac†—whose intelligent interest in the geology of the region has kept him on the lookout for fossil leaves and bones, glacial bowlders, and whatever would illustrate the origin of the deposits—has recently brought me from his pit two bones, the second within the month past. One of the bones is a humerus and the other a tibia. The humerus was taken from a depth of 11 feet in the clay bed, and the tibia, subsequently, at a depth of 7 feet. Professor Marsh has given me the following note on these bones for this place:

"The two bones from the clay pit of North Haven are the right humerus and left tibia of a species of Reindeer, about the size of *Rangifer tarandus*. The two bones did not belong to the same individual, the humerus indicating a younger and somewhat larger animal. The tibia is the more characteristic specimen, and agrees so closely with the corresponding bone of the European Reindeer (*Rangifer tarandus*) that it must have belonged to the same or a very nearly related species. Its resemblance to the tibia of the American Caribou (*Rangifer caribou*) is much less marked. The humerus also evidently belonged to a Reindeer, but the proportions are somewhat similar to those of the Caribou."

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*Beneath North Haven village the bed thins to 4 or 5 feet and the clay is rather sandy—or what is called "weak clay." In sinking a well about 80 rods east of the North Haven depot, as I was informed by Mr. D. H. Pierpont, 7½ feet of sand were first passed through, next 4 feet of "weak clay," and then 8½ feet of fine quicksand, before reaching tide level. At another place, nearer the river, the bed of clay was 5 feet thick and the sand between it and tide level but 3 feet. At the south end of the Quinnipiac basin, four and a half miles south of North Haven, where the terrace has a height of 42 feet above high-tide level, there are layers of quicksand in the formation; but no clay has yet been found. It may, however, exist there below the sands.

† Four miles from New Haven and two from North Haven.

Both bones when found were without a trace of wear or fracture. The tibia had lost much of its gelatine and has hence become somewhat cracked from drying since it was exhumed. The humerus has almost the freshness and firmness of a recent bone. Both are of a light brownish color from their long burial. The difference in the conditions of the two bones Mr. Crafts explains by saying that the humerus was imbedded in the laminated clay, and the tibia in a portion of the clay-bed containing sandy seams an inch or so thick, and open therefore to percolating waters. Besides relics of mammals, the clay beds have afforded no organic forms either vegetable or animal. The microscope reveals nothing.

The bones give us an insight into the life of New England in the early part of the Quaternary, proving that Reindeers of the Arctic type were living here. From the facts we gather that:—

1. The bones are probably not of pre-Glacial age. They are certainly not bones which the glacier had taken up from the soil underneath it along with moraine material for transportation and deposition: this being evinced by their freedom from all wear and fracture. The clay was produced through the glacial abrasion of hard stones; and surely if the bones had been in the same mill they would have been ground up too, or at least would have been in some parts worn or broken. It is possible that they were lying in the soil near by when the ice-period began, and that subglacial waters washed them into the basin with the depositing clay; but this is far from probable.

2. They were placed where they were found during the ormation of the clay bed, and were deposited at widely different times, four feet in thickness of the clay intervening beween them. They therefore belonged to different individuals.

3. The clay bed is of glacial origin. For it contains an occaional bowlder of large size,* and is overlaid by sands and ravel of the stratified drift.

4. If the reindeer bones were not pre-Glacial they must have come from reindeers living in the Quinnipiac valley after the lacier had retreated to the north of the valley. And since the lay-bed is of glacial origin, its bowlders must have come rom ice-floes that floated down stream from the retreated lacier.

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* This volume, page 177; the bowlder there referred to is of trap.

5. The clay beds of the Quinnipiac valley antedate the great flood due to the final dissolution of the glacier; for at the village of Quinnipiac the bed is overlaid on the west side by beds of coarse gravel, and just south of North Haven the sand and gravel overlying the clay bed has the flow-and-plunge structure so common in the stratified drift.

In my Memoir on the Geology of the New Haven region, I referred the clay-beds to the later or "Alluvian" part of the Champlain or Fluvial period, supposing them to be the mud-deposits made along the borders of the great Quinnipiac basin or inner New Haven harbor, during the more quiet portion of that era of submergence; and the sand-beds overlying and underlying the clay beds at North Haven I referred to the same time. But the discovery since then of the large bowlders in the clay beds,* and of evidence that the deposit of coarse stratified gravel situated along side of the clay pits actually overlies the clay in places, as proved by boring, appears to force us to the conclusion that the clay beds were (1) completed after the glacier had retreated from the valley, and (2) before the final flood.

6. Whether this retreat of the glacier was a *temporary* retreat, produced by a warm interval in the era of ice, or whether it was the *final* retreat, marking the progress of its final dissolution over New England, it is not easy positively to decide. The formation of stratified drift over the New Haven region has no break in the succession of its beds to mark such a retreat and return of the ice; and the clayey stratum contains no layer of vegetable or animal debris as testimony to a warm interval. All that the few facts suggest as to climate is that it was such as reindeers liked, which means that it was cold, and that the ice was still not far off to the north.

Remains of the Reindeer have been found in Northern New York on Racket River, and in Southern, at Sing Sing; in New Jersey, at Vincentown, Burlington County; and in Kentucky at Big Bone Lick. The specimen from Vincentown described by Dr. Leidy† and that from Sing Sing, mentioned by Mr. R Fisher‡ were antlers. Dr. Leidy says that the Vincentown "antler bears a nearer resemblance to those of the Barren-Ground [or Arctic] Reindeer than it does to those of the Woodland Reindeer [or Caribou], but differs in some respect from both." In my Geological Manual I have suggested tha these remains may indicate the occurrence on the American Continent of the *second* glacial era so well marked in Europ by the reindeer remains in Southern France. But they may, on the contrary, have belonged to the same time with the Reindeers of the Quinnipiac valley—the era of melting of the great glacier of the *first* glacial era.

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* Geology of the New Haven region, p. 84.

† Proc. Acad. Nat. Sci. Philadelphia, 1858, p. 179, and Journ. Acad. Nat. S Philadelphia, vol. vii, 2d series, 1869, on the Extinct Mammalian Fauna, &c., 377.

‡ Ibid., 1859, 194.

The Stiles clay-pit, near North Haven, is reputed to have afforded many years since (as I state on page 86 of my Memoir on the New Haven Region) the antlers of a buck at a depth of 10 or 15 feet. Mr. J. Lorenzo Stiles informed me in 1870 that the antlers were those of the common species of deer. The specimen went to the New Haven City "Museum." The old museum long since disappeared, so that the fact with regard to the species cannot now be ascertained. The antlers may have been those of another Quinnipiac Reindeer.

ART. XLIX.—*Iowa County Meteor and its Meteorites;* by N. R. LEONARD.

ON the evening of February 12th, 1875, at about half past ten o'clock, a very large meteor was seen passing from S.W. toward the N.E., over Northern Missouri and Southern Iowa, and coming to the earth in the form of a shower of stones, in Iowa County, Iowa, a few miles east of Marengo.

At this hour the sky seems to have been quite clear over the greater part of the States named; though light clouds and a sort of haze are spoken of by observers in the counties adjoining the place where the stones fell, so that the meteor was seen throughout a region extending at least 400 miles in length from S.W. to N.E., and 250 miles in breadth.

In their descriptions of the course it pursued the accounts of observers varied with their positions with reference to the place where it fell. Those east of this region thought the course to be toward the west or northwest, those north described it as moving toward the south or the southeast, and in a few cases the statements of different observers in the same town are contradictory as to the direction of its motion.

The brilliancy of its light, and the concussion which accompanied its fall, were such as to attract very general notice, considering the lateness of the hour, and we believe that the observations herewith presented will be found to determine the path it pursued with a fair degree of accuracy.

I will give, first, those descriptions which relate to its general appearance, as nearly as possible in the language of the observers

At Keokuk, Iowa, it is described as "Oblong in figure, with a train ten to twelve times the length of the body, giving an intensely brilliant light, of crystalline whiteness at the center, fire red on the border, and throwing out red sparks and purplish jets of flame; train less luminous than body, exploded like a rocket. Opinions were divided as to whether any detonation accompanied the explosion." These observations were collected for me by L. C. Ingersoll, M.D., from a number of persons in that city who witnessed the flight.

At Washington, Iowa, Rev. E. B. Taggart in a letter to the Free Press of that city, describes it as of a "Horse-shoe shape, greatly elongated. The outer edge very bright, then a narrow dark space, with a core of intense brilliancy, so vivid as to blind the eyes for a moment. It had not a comet-like train, but a sort of flowing jacket of flame. Detonations heard, so violent as to shake the earth, and to jar the windows like the shock of an earthquake."

At Iowa Agricultural College, Prof. Macomber writes: "In form it was like an immense rocket with streamers flowing from the hinder part, the front being smooth and curved like a sabre. Its color was at first brilliant white, illuminating the sky like a flash of lightning; then fading gradually into yellow, then a deep rich orange, almost scarlet when it burst."

At Sigourney, almost directly under the path of the meteor, Mr. J. A. Donnell, writing to the "Sigourney News," speaks of it as "A globe of fire with pale lines of light radiating from it. The light of the globe very vivid. It appeared to be falling toward the earth from about 10° west of the zenith." He says he could see it dropping through a succession of clouds until it came inside the dome above him, where it apparently stood still for a moment and then passed over toward the northeast. The detonation was compared to the discharge of a 40 gun battery which he had heard in the army.

At Amana, about five miles northeast of the middle of the region where the meteor fell, Mr. F. Christen writes that, "Its light was at first dazzling white, then changed to red. Threw distinct shadows of objects on the street. Fragments seemed to separate, not with violence, but simply as if falling apart; the separation was speedily followed by a disappearance of the meteor."

Mr. G. Holm of *Marengo*, gives an account similar to the last, and in addition says that the descending path was *wave-like* and not a uniform curve. Mr. Frank McClintock of *West Union*, says that "at about the middle of its course it appeared to give a slight dart or bound toward the east." Probably the same wave-like motion spoken of by Mr. Holm, when viewed from a station not far removed from the direction in which the meteor was moving.

At Mt. Pleasant, Iowa, Prof. Mansfield writes that some of the observers thought that the meteor attained its maximum brightness when about due west of that place. Some of the students who were familiar with the color of the flames of different substances with which they had experimented in the Chemical Laboratory called out at the time that the color of the meteor showed iron and copper.

In computing the path pursued by the meteor I have relied almost entirely upon observations which could be verified afterward, by reason of its having passed near to, or behind some recognized point on a building or other object whose altitude and bearing from the station of the observer have since been ascertained by instrumental measurement.

In giving the data I will denote the location of each observer by mentioning the section, township, and range numbers, which, as our lands are laid out on the rectangular method, will be quite definite.

The following is a list of observations upon which I have relied.

1. At Amana (northwest corner of 26–81–9). Mr. F. Christen first saw the meteor when at an altitude of 10 or 11 degrees and at a bearing of S. 19° W. Soon after he saw it passing near the top of a chimney whose bearing and altitude were respectively S. 26° W., and 17½° and finally saw it separate and disappear at an altitude of 29° bearing S. 65° W.

2. At Mt. Pleasant (4–71–6), there is not a perfect agreement as to the altitude of the meteor when due west of that place. Some thought that it passed very near the moon, others thought that it passed above, and one at least gives its altitude as less than that of the moon. One observer spoke of seeing it when at a bearing of about S. 14° W.

3. At Albia, Monroe County (15–72–17), Mr. Pascal T. Lambert saw it when due east at an altitude of 40° to 45°, and pointed out the place of its disappearance which I found to have a bearing N. 41° 30′ E., or almost exactly in the direction of South Amana, in whose vicinity it fell.

4. Mr. E. H. Warrall of the U. S. Corps of Engineers at Keokuk, Iowa (24–65–5), gave its altitude when at a bearing of about N. 60° W., at between 10° and 12°.

Another observer gave the altitude when near the same place at 10° 30′. Both observers saw the meteor disappear behind a church steeple.

5. Rev. E. B. Taggart, of Washington, Iowa (17–75–7), thought that it passed 10° or 15° west of the moon.

6. Prof. J. K. Macomber of the Iowa State Agricultural College (4–83–24), first saw the meteor when at an altitude of 7° or 7° 30′ and bearing S. 55° E. This observation is almost exactly accordant with one taken independently at the same

place by a student who was engaged at the time in taking meteorological observations.

7. Mr. J. A. Donnell, of Sigourney (2–75–12), thought that the meteor passed about 10° west of the zenith of his place; no means of verifying his observation, and no measures taken.

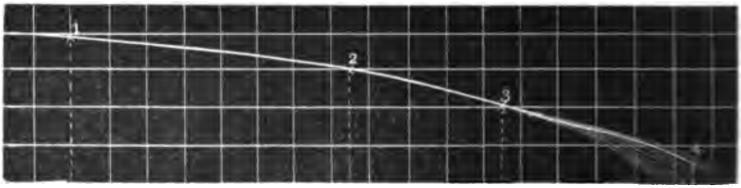
8. Mr. Ream, telegraph operator at Oskaloosa (13–75–17), gave the zenith distance of the meteor when due east as about 35°. No measures taken.

9. C. D. Leggett, Esq., of Fairfield (25–72–10), estimated its zenith distance, when northwest of him, at 25°.

From a comparison of observations 2 and 3, I conclude that the meteor passed over a point a few miles east of the town of Ottumwa. The course can be approximately marked on a map of the state by a line drawn through Agency City and South Amana, the latter being a little west of the center of the district over which the meteorites were found. This would give a bearing of about N. 18° E., and would accord very well with the estimated zenith distances Nos. 8 and 9. It would, however, be at variance with No. 7, and the only explanation I can offer is by saying that it is very difficult for a man to fix, at a glance, the point directly over head, so that we may suspect this observation to be in error. No. 4 corresponds to a point about sixty eight miles from the place where the largest fragment was found, and gives for the altitude fifteen miles. No. 6 indicates a point thirty-eight miles from the same place, and gives for the altitude twelve miles. No. 1 indicates three points in the meteor path, the first nearly accordant with that given by No. 4, and gives about the same result as to height; the second point denoted is at a distance of about twenty-two miles from the end of the path, and at an altitude of eight miles; and the third point is two miles from the end of the path, and at an altitude of two miles.

Figure 1 shows these results to the eye. The sides of the squares are four miles. The points 1, 2, 3, and 4 are sixty-eight, thirty-eight, twenty-two, and two miles from where the largest stone fell. The heights at 1 and 4 of the figure I consider the most reliable; those at 2 and 3 are somewhat uncertain, owing to some uncertainty in the azimuth given by the observations.

1.

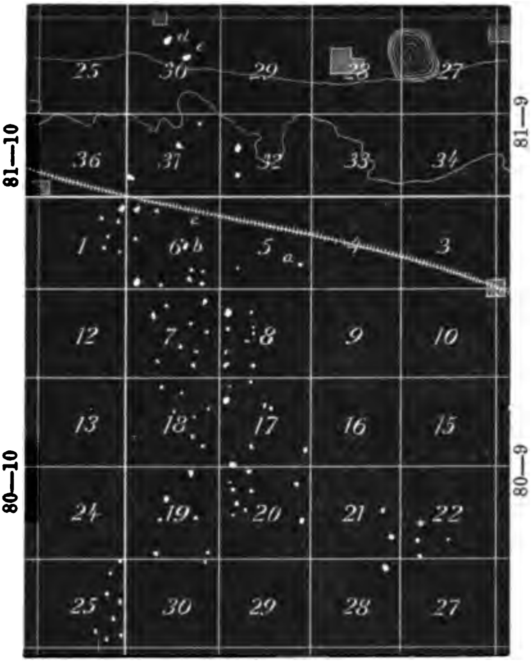


Showing the path of the meteor through the atmosphere.

The product of this meteor-fall was a large number of irregularly shaped stones, varying in weight from a few ounces up to 74 pounds, and aggregating, so far as found, 500 pounds weight, of which I have secured more than 300.

Figure 2 shows the localities where the various fragments were found. The region represented is part of four townships near the northeast corner of Iowa County. The railroad is the Chicago, Rock Island & Pacific Railroad; the village in 36–81–10 is South Amana; that at northeast corner of 10–80–9 is Homestead; that in east line of 27–81–9 is Amana; and that in 28–81–9 is Middle Amana.

2.



Showing the distribution of the stones that have been found.

The dots represent places where fragments have been picked up. We specially note—

a. in 5–80–9, where the first was found,	weight,	7 lb. 6 oz.
b. " 6–80–9, broken in striking the ground,	"	43 lb. 8 oz.
c. " "	"	15 lb.
d. " 30–81–9, lately found,	"	74 lb.
e. " " "	"	48 lb.

The largest quantity was probably found in the vicinity of 6–80–9, many pieces in that region weighed 8 to 14 pounds; not many small stones in that locality. Below sections 7 and 8 they were all small. The length of the field is about 7 miles, and its extreme breadth at the southern end about 4 miles.

It is worthy of remark that the regions bordering close upon the river are timbered land, and that especially in sections north of the river and below the two larger stones found, the lands are low and now overflowed, so that larger pieces, hidden by forest or water, may yet be found.

These meteoric stones are many of them entirely covered with the ordinary black coating, and they all present the "pitted" appearance common to such bodies. In several instances there is plain evidence of a fracture having taken place while the stone was as yet some distance from the earth.

These surfaces of fracture are for the most part covered with a secondary coating which sometimes appears to have been partially formed by the pouring over of the melted surface matter from other parts of the surface. In some cases, however, the overflow is only traceable to a short distance from the edge of the fracture, and the remainder is merely discolored as if by smoke.

The want of homogeneity in the structure of the ærolites is shown in several cases, by a sort of beaded circlet surrounding the stone. These circlets are composed of molten drops of iron and seem to lie nearly in a plane, and where this plane has been broken across in the fall, it may be traced over the fresh surface by the presence of unusually large particles of nickeliferous iron. For the following chemical analysis of a fragment of the meteorite I am indebted to Prof. J. Lawrence Smith.

Analysis of Prof. J. Lawrence Smith.—The Iowa County meteorite is of the more common variety, with a dull black coating, and having a rather light gray color in the interior. It has numerous particles of nickeliferous iron disseminated through it, also particles of troilite. The specimen analyzed had a vein running through it which was much richer in iron than the mass to which it belonged.

This meteorite has a hardness rather above the average of its class. I have found it to be composed of

Stony Matter,	81·64
Troilite,	5·82
Nickeliferous iron,	12·54

Of the stony part there was

Soluble in acid,	54·15
Insoluble,	45·85

Separate analyses of these gave for the

	Soluble.	Insoluble.
Silica,	35·61	55·02
Protoxide of iron,	27·20	27·41
Magnesia,	33·45	13·12
Soda with traces of potass. and lithia,	1·45	2·01
Alumina,	·71	·84

This plainly shows that the principal constituent of the soluble portion is an olivine, rich in oxide of iron, approaching hyalosiderite in composition, and that the insoluble part is a pyroxene.

The nickeliferous iron contained, besides traces of phosphorus, sulphur, and copper,

Iron,	89·04
Nickel,	10·34
Cobalt,	·58

From an examination of an entire stone sent me after the completion of the above analysis, I have found the specific gravity to be 3·57. J. LAWRENCE SMITH.

Louisville, Ky., April 15th, 1875.

The first stone from the meteor that was found was discovered lying on the snow, on the afternoon of February 15th, and was adherent to snow and ice underneath. As the weather had been very cold from the time of the meteor-fall to the time of finding this fragment it must have been warm enough when it fell to melt slightly the underlying snow, to which it was afterwards frozen.

I visited the spot shortly afterward and found that it had first struck the ground more than 30 feet to the southwest of the place where it was found, making a slight indentation and bounding thence to the place where it finally came to rest. It was a fragment and showed a secondary coating of rather more than average thickness.

The other meteoric stones were not found until after the melting of the snow in the latter part of March. It is doubtless owing to the frozen condition of the ground and the low angle of descent that only a few of the larger pieces made any indentation in the earth, and we may therefore suppose that a much larger proportion of this meteor-fall has been secured than is usual.

The velocity with which the meteor moved cannot be satisfactorily stated. The maximum velocity, according to the data in my possession, would be about ten miles per second; the minimum about three miles. The most probable value for the last sixty or seventy miles of its course is from six to seven miles per second. This estimate was furnished by Mr. Christie, of Amana, who happened to be walking rapidly at the time the meteor appeared, and continued for a distance of fourteen paces, when having passed the corner of a building that threatened to obstruct his view, he stopped, and watched the meteor until it disappeared, and gave his estimate of the whole time at ten to twelve seconds; and as he saw it through sixty or seventy miles of its path, the resulting velocity would be as above stated.

Iowa State University, Aug. 4th, 1875.

ART. L.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXXVI.—*On the Post-pliocene fossils of Sankoty Head, Nantucket Island;* by A. E. VERRILL; *with a note on the Geology;* by S. H. SCUDDER.

AN account of the beds at Sankoty Head, with a list of the fossils obtained, was published by Messrs. Desor and Cabot in 1849,* and most of our knowledge of the locality has hitherto been derived from their description.

During the past summer the headquarters of the U. S. Fish Commission were at Wood's Hole, Mass., where an excellent and permanent biological station has been established under its auspices. In connection with the investigation of the marine invertebrates it was considered desirable to examine the fossiliferous deposits to ascertain the facts in regard to the former distribution of the species, and the physical changes that have occurred on our coast. Accordingly one of our dredging parties, consisting of Prof. A. Hyatt, Mr. Sanderson Smith, Mr. C. H. Merriam, and others, visited the locality, while on one of the excursions to Nantucket Shoals, and made a collection of the fossils. Later Mr. S. H. Scudder, of Cambridge, Mass., with the kind coöperation of Mr. W. J. Flagg, made an excavation into the cliff, so as to expose the fossiliferous beds more fully, and Mr. Richard Rathburn visited the locality, and made for the Commission, a large and very valuable collection of the fossils, in which the specimens from the lower and upper beds were kept distinct. Some of our most interesting results have been due wholly to the care with which he made and labelled this collection. From it we learn that the two layers, though only a few inches apart, contain very different assemblages of fossils and were deposited under quite different circumstances; the lower bed contains only such southern species as now inhabit the warm quiet waters of sheltered bays on the southern coasts of New England and farther south; while the upper bed contains many northern species, many of them fragmentary and beach-worn, and all of them, with one unimportant exception, the same species that are now found cast upon the outer beaches of Nantucket and Cape Cod by storms, and living in the colder outer waters, off the same coasts.

In the list published by Desor and Cabot seventeen species were enumerated, and those from the different beds were not kept separate. In our collections there are about sixty species, nearly all of which can now be assigned to their actual positions in the strata.

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* Quarterly Journal of the Geological Society of London, vol. v, p. 340, Feb., 1849. An abstract of the article has been printed by Dr. Packard in the Memoirs of the Boston Soc. of Nat. History, vol. i, p. 252, 1866. See also Proc. Boston Soc. Nat. Hist., vol. iii, p. 79, and this Journal, II, xiv, 50.

Mr. Scudder made a study of the stratification, and as his observations do not agree perfectly with the account given by Desor and Cabot, he has kindly furnished the following description of the locality. The most important point from which it differs from the former one is his conclusion that the fossiliferous beds are conformable to the strata of sandy clay, forming the base of the cliff. Mr. Desor stated that they are unconformable, and referred the clays to the Miocene Tertiary, like those of Martha's Vineyard.

Note on the Post-pliocene Strata of Sankoty Head; by S. H. Scudder.

"The sands and gravels forming the bluff at Sankoty Head, Nantucket, rest at base upon a thick bed of light brown sandy clay of uncertain thickness, but extending upward to about twenty feet above the sea-level. As the beds which rest upon it dip to the southwest, and as the anchor brings up clay from Sankoty Head eastward for half a mile, this clay bed is probably of great thickness.

The brown clay is overlaid by four feet of gravel and coarse sand, the coarser parts mostly confined to three or four inches of the uppermost levels; the upper bed is more or less ferruginous and hardens on exposure into a rather compact conglomerate. To this stratum must doubtless be referred a single specimen of a bivalve (probably a Mactra), with valves half open, picked up on the bluff, imbedded in a gravel conglomerate, and like it strongly impregnated with iron. The gravel is followed by about four feet of sands, subdivisible into separate beds, viz: at base, an inch or two of a very fine loose white sand, followed by nearly two feet and a half of a little less fine, closely packed, white sand, with irregular ferruginous streaks through its mass; this is covered by nine inches of a coarse beach sand, with a still coarser sand in pockets; and this again by nine inches of a very fine white sand. Above this comes a foot of ferruginous sand closely packed with masses of tough blue clay, much exceeding the sand in bulk, and forming the floor of the fossiliferous beds.

These consist first, at base, of twenty-two inches of coarse sand in which the oyster, quohog, and common clam are the prevailing forms, the first predominating to such a degree as to make the name of oyster-bed the most appropriate. This merges into a serpula-bed, about twenty-eight inches in thickness, made up almost altogether of large masses of serpula, packed in sand and almost wholly devoid of other fossils. The bed of worn shells superimposed on this is about twenty-two inches in thickness and closely resembles *coquina*, except in the entire want of adhesion between the fragments.

This bed is followed by about ten feet of fine white thinly bedded sand, and this by the stratified drift of the island, to a depth, as estimated by Desor and Cabot, of forty-two feet; the foot of peat mentioned by them is wanting at this exact locality, (though present a few hundred feet farther south,) leaving the drift covered by five or six feet of dune-sand, more or less intermixed with loam below.

On following the bed of broken shells along the face of the cliff it was found to thin out to about a foot in thickness twenty-five feet on either side of the most prominent point, where the section was made,* and which has doubtless been longer protected than the other parts of the bluff by the former presence of a great mass of clay next the water's edge, called "Antony's Nose"; beyond these twenty-five feet, the bed of broken shells becomes more or less obscured by an admixture of sand, gravel and serpula, and is entirely lost at forty feet distance on either side.

The strata, from the lowermost clay to the bed of worn shells, all dip to the southwest. The uppermost beds incline along the face of the cliff three (3) degrees to the south, while the inclination to the west (along the section dug out of the cliff) is eleven (11) degrees, making a dip of nine (9) degrees to the southwest. All the beds below this also incline eleven (11) degrees to the west, but the inclination of their face toward the south increased gradually in passing downward, until that of the upper edge of the lower clay reaches eleven (11) degrees, making a southwesterly dip of this bed seventeen (17) degrees to the southwest. There is no evidence of any thinning out of the gravel-bed, as stated by Desor and Cabot, nor of any unconformability between this bed and the underlying clays; but, on the contrary, every appearance that the latter belong to the same continuous series as the former.

It is worthy of note that the fossils of this locality lie above the clays, instead of in the clays, as in most of the New England localities of post-pliocene marine shells."

Mr. Rathburn informs me that in the lower shell-bed the shells are extremely abundant and mostly entire, but generally break in pieces when the matrix is removed, and that when first taken out they appear to be soft, but harden on exposure to the air. The matrix is a coarse yellowish ferruginous sand with small pebbles, and the shells have a rusty stain. None of the specimens give any evidence of having been worn by the waves, and many of the most delicate, like *Cumingia tellinoides*, *Angulus tener*, etc., are entire, and sometimes have the valves still united. This is the case, also, with some of the oysters. A large proportion of the quohog-clams (*Venus mercenaria*) are broken into angular fragments with sharp edges and angles. I have ascertained by an examination of large numbers of specimens, both entire and broken, that the breaking is due wholly to lines of fracture developed in the shell by drying or weathering, just as similar shells often crack into angular fragments in the dry heated air of our museums. Many of the entire specimens of the fossil quohogs, etc., show such fractures extending in different directions across the shell, so that they are ready to break up into several angular fragments under the least strain, or even by a change in the moisture or temperature. This condition of fossil shells is a very frequent one in other localities, and will account for very many cases where the shells are found broken into angular fragments in rocks of other periods. It is evident, both from the condition of the shells in the lower bed and from the peculiar assemblage of southern species, that it was deposited in the very quiet waters of a sandy sheltered bay, entirely protected from the action of the oceanic waves. The assemblage of species is similar to that now living in the protected bays of Southern New England at the depth of 3 to 5 fathoms.* The quohog-clam, oyster, *Modiola hamatus*, *Cumingia tellinoides*, *Arca transversa*, *Urosalpinx cinerea*, and the three species of *Crepidula* are the most abundant and characteristic shells.

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* I must here express my indebtedness to William J. Flagg, Esq., of New York, who freely gave me the unlimited assistance of his workmen, and enabled me to make a much more thorough exploration than could otherwise have been possible.

The Serpula bed consists mainly of convoluted masses of the tubes of *Serpula dianthus* V., mixed more or less with sand, but without many other fossils. This *Serpula* is still abundant all along the coasts of Southern New England, and southward to the Carolinas, in all sheltered bays and harbors where the water is not brackish, from low-water to 8 fathoms or more. It is often particularly abundant on oyster-beds, and in such localities often completely overgrows the shells, if neglected for a year or two.

The upper shell-bed, according to Mr. Rathburn, consists almost wholly of broken shells, with a little quartzose sand, which is light colored, and not at all ferruginous, so that the shells from this bed are not stained rusty yellow by the oxide of iron, like those of the lower one. Many of the fragments are distinctly water-worn and rounded, and most of them have the appearance of those shells thrown on the outer beaches by the surf, or of the dead shells often dredged up in large quantities on sandy bottoms in shallow waters near the shore, or in the vicinity of sand-shoals, where the waves break during storms. The abundance of northern forms, such as *Buccinum undatum*, *Ceronia arctata*, *Astarte castanea*, *Cyclocardia borealis*, *Mya truncata*, *Balanus porcatus*, etc., shows that the bed was deposited by the cold waters of the outer coast, and their water-worn condition proves that the deposit was made in very shallow water near the shore, or near sand-shoals, swept by the waves. Such deposits may be made at any depth less than about 12 fathoms, but are more commouly made in 2 to 8 fathoms, on our coast.

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* That the depth could not have been less than 3 fathoms is probable because those species that abundantly inhabit the eel-grass (*Zostera*), which grows in sheltered localities at all depths down to about 2½ fathoms, are either rare or entirely absent, viz. *Bittium nigrum*, *Astyris lunata*, *Triforis nigrocincta*, *Lacuna vincta*, *Littorina rudis*, *Pecten irradians*, etc. That the depth was probably not above 5 or 6 fathoms, I infer because the quohog and oyster, when adult, are seldom found in any abundance below 5 fathoms.

This locality shows, therefore, that although important changes in the distribution of the land and water, as well as in the level of the land, must have occurred in this region during the time when these strata were being deposited, and subseqently, the temperature of the waters must have been nearly the same then as it is now, and that there must have been, at that time, the same contrast that now exists* between the coldness of the waters on the outer shores and the heat of the sheltered bays and harbors. The fossils of the lower bed indicate, for the water, a summer temperature of 70° to 75° F., while those of the upper bed correspond to a temparature of 55° to 60°, thus showing plainly the influence of the Arctic current along the coast. All the species still inhabit the waters of Southern New England, except *Diodora Noachina* found in the upper bed, but this occurs in Massachusetts Bay, and will probably be found hereafter in the deeper channels among the Nantucket Shoals, where we found this year many northern species that had not previously been found south of Cape Cod. *Modiola hamatus*, which occurs in considerable numbers in the lower bed, is a southern shell, common in the Gulf of Mexico, and on the southern coast as far north as Virginia, but occurring on the southern coast of New England, only locally, in harbors, etc., and generally on beds of oysters that have been transplanted from farther south. For this reason it has been generally supposed that it is not indigenous but has in all cases been introduced with the oysters. However this may be now, it is evident that it was a native species in Post-pliocene times.

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* The nature of the changes that caused the alteration of the temperature and difference of the life indicated by these two beds of fossils will be discussed elsewhere more fully. It may be well, however, to state that my conclusion is that when the lower shell-bed and serpula-bed were forming, a shallow bay existed at this place, from which the outer waters were excluded, either by an island to the eastward of the site of Nantucket, or else by a southward prolongation of Cape Cod. The extensive submerged shoals south of Cape Cod and east and southeast of Nantucket may be the remnants of such lands. Some of these shoals are now covered with stones and rocks (probably drift boulders). Before the deposition of the upper bed some portion of the dry land eastward of the locality must have been submerged by subsidence, or else washed away by the encroachment of the sea (the latter most probably, to judge from the nature of the fossils), thus allowing the cold outer waters to occupy the bay, and the Atlantic surf to fill it with broken shells and beach sand. The protecting land must have been at least 50 feet higher, in its lowest parts, than the present level of the sea. As the shoals east of Nantucket have now several fathoms of water over them a vast amount of denudation must have taken place since that time. This is also shown by the great thickness of the strata of sand and gravel resulting from that denudation, and still remaining above the fossiliferous beds, and doubtless these strata have themselves also suffered great denudation during the period of their emergence from the sea, and subsequently. The partial destruction of islands formerly existing in the region of Nantucket Shoals would also result in partially filling the channels and deeper depressions between them, the final result of surf-action being a levelling one, and this would have been the case both during the period of submergence, and subsequently during that of emergence, but during the period of greatest depression (100 feet or more below the present level) there would have been comparatively little denudation of the deeply submerged islands, but there may have been islands high enough to have been out of water even then, which have since disappeared by denudation. The same reasoning will also apply to St. George's Bank and the great shoals adjacent, all of which are probably islands which have been worn down by the waves below the level of the sea.

The only species that shows any noteworthy variation, when compared with the modern shells from the same region, is the quohog (*Venus mercenaria*). Most of the specimens of this species differ considerably, in the rounder form, thicker shell, and in the greater development of the concentric ridges, from the ordinary quohog-clams seen in our markets. In a lot of large shells of this species obtained from a fisherman at Nantucket, this season, there are many that are equally massive and have the same rounded form, and although the concentric sculpture is not so strongly developed as in many of the fossil shells, there is a decided approach toward them in this respect also. These living Nantucket quohogs certainly resemble the fossil ones more closely than do those from any other locality known to me. But among the fossil specimens (as also among the recent ones from Nantucket) there are some specimens that are as smooth, thin and elongated as the ordinary variety found in our harbors; and intermediate specimens also occur, so there is no reason to suppose that the variation in the fossil shells was anything more than a local variation, such as often occurs in many species at the present time. Nevertheless it may be convenient to designate such special conditions of a species by a particular variety-name. The general absence of such variations in the Post-pliocene shells of the New England coast, as compared with those now living upon it, is certainly very remarkable, considering their great antiquity and the many important changes that have since taken place in the physical conditions of the land and water. Such instances give us the best evidence in regard to the constancy and stability of true specific characters, even when apparently of little or no importance in the economy of the animal itself.

List of Species from the lower Shell-bed.

The names of the species are those used in the Report on the Invertebrata of Southern New England, by the author and Prof. S. I. Smith, in 1st Report of the United States Commissioner of Fish and Fisheries, 1874.

In each case the relative abundance is indicated. I have also added the present distribution on the American coast, in a general way, as well as a few notes on the geological distribution of each species. Fuller information on the distribution of the species has been given in the Report referred to.

MOLLUSCA.

Tritia trivittata Adams. Common.

Florida to Gulf of St. Lawrence; low-water to 40 fathoms. Fossil in the Post-pliocene of Point Shirley, Gardiner's I., Virginia and southward; in the Miocene of Maryland, South Carolina, etc.

Ilyanassa obsoleta Stimpson. Common.

Florida and Gulf of Mexico to Southern Maine; local in the southern part of the Gulf of St. Lawrence; littoral to 2 fathoms. Fossil in the Post-pliocene of Point Shirley, Virginia, South Carolina, etc.

Urosalpinx cinerea Stimpson. Common.

Tampa Bay and Eastern Florida to Massachusetts Bay, and local farther north to Gulf of St. Lawrence; littoral to 10 fathoms. Post-pliocene fossil at Point Shirley, Gardiner's I., Virginia and southward; in the Miocene of Maryland.

Eupleura caudata Adams. One fine specimen.

Gulf of Mexico and Eastern Florida to Cape Cod; low-water to 8 fathoms. In the Post-pliocene from Virginia to Florida; Miocene of Maryland and South Carolina.

Astyris lunata Dall. One specimen.

Alabama and Florida to Massachusetts Bay; low-water to 14 fathoms. Fossil in the Post-pliocene of Gardiner's I. and South Carolina.

Cerithiopsis Greenii Verrill. Four specimens.

South Carolina to Massachusetts Bay; two to 10 fathoms.

Crepidula fornicata Lamarck. Abundant.

Gulf of Mexico to Southern Maine, and local in the southern part of the Gulf of St. Lawrence; low-water to 15 fathoms. Post-pliocene fossil at Gardiner's I., South Carolina, etc.; and Miocene in Maryland and South Carolina.

Crepidula plana Say. Common.

Distribution same as the preceding, from which, however, it is very distinct.

Crepidula convexa Say. Not common.

Distribution like the two preceding, from both of which it is perfectly distinct (although confounded with *C. fornicata* by Tryon and others). Fossil in the Post-pliocene of Virginia and South Carolina.

Odostomia trifida Gould. Common.

New Jersey to Massachusetts Bay; low-water to 5 fathoms.

Odostomia impressa Stimpson. Common.

South Carolina to Vineyard Sound; low-water to 5 fathoms.

Turbonilla interrupta Adams. Several specimens.

South Carolina to Cape Cod; local farther north to the southern part of the Gulf of St. Lawrence, near Prince Edwards' Island; low-water to 15 fathoms. Post-pliocene fossil at Gardiner's I. and in S. Carolina.

Saxicava arctica Deshayes. Rare and small.

Arctic Ocean to Georgia, local and rare south of Long Island; low-water to 50 fathoms. Fossil in the Post-pliocene of Maine and everywhere northward.

Mya arenaria Linné. Abundant.

Arctic Ocean (lat. 78° N.) to South Carolina; low-water to 40 fathoms. Post-pliocene fossil from South Carolina and Virginia to Greenland and Northern Europe.

Corbula contracta Say. One valve.

Florida to Cape Cod.

Ensatella Americana Verrill. Common.

Labrador to Florida; low-water to 25 fathoms. Fossil in the Post-pliocene of Portland, Me., Point Shirley, Gardiner's I., Virginia and South Carolina; Miocene of Maryland, etc.

Angulus tener Adams. Several specimens.

Florida to Gulf of St. Lawrence; low-water to 12 fathoms.

Cumingia tellinoides Conrad. Common.

Florida to Cape Cod; three to 12 fathoms. In the Post-pliocene and Miocene of South Carolina, etc.

Petricola pholadiformis Lamarck. Rare.

Gulf of Mexico to Massachusetts Bay; local in Casco Bay and southern part of the Gulf of St. Lawrence; low-water to 4 fathoms. In the Post-pliocene and Pliocene from Florida to Virginia.

Venus mercenaria Linné. The variety abundant; a nearly typical form not uncommon.

Florida to Massachusetts Bay; local on the southwestern coast of Maine and southern shores of the Gulf of St. Lawrence and Bay of Chaleur; low-water to 8 fathoms. In the Post-pliocene of Point Shirley, Gardiner's I., Virginia, Florida; Miocene from Maryland to South Carolina.

Var. antiqua Verrill. By this name I propose to designate the unusually massive and strongly sculptured variety to which most of the fossil shells belong, and which has been already discussed on a previous page.

The shell is rather obtusely rounded posteriorly, and is thickly covered with prominent concentric lamelliform ridges, which mostly extend entirely across the shell, but are often reflexed, appressed and more or less confluent over the middle region, where the ordinary variety is nearly smooth (except when young). The violet color can still be traced in some specimens entirely around the inner margin, as in many recent Nantucket examples.

Tottenia gemma Perkins. Few specimens obtained.

South Carolina to Labrador; low-water to 4 fathoms.

Gouldia mactracea Gould. One specimen.

Florida to Cape Cod; 3 to 15 fathoms.

Scapharca transversa Adams. Very abundant and large.

Florida to Cape Cod; low-water to 15 fathoms. In the Post-pliocene of Cape Cod, Gardiner's I., Virginia, South Carolina; Miocene of Virginia and North Carolina.

Mytilus edulis Linné. Common.

Circumpolar; Arctic Ocean to North Carolina; littoral to 50 fathoms. In the Post-pliocene from Florida to Greenland and Northern Europe.

Modiola hamatus Verrill. Common.

Gulf of Mexico to Long Island Sound and Naragansett Bay; littoral.

Crenella glandula Adams. Few specimens obtained.

Labrador to Long Island; 5 to 60 fathoms. In the Post-pliocene of Montreal.

Anomia glabra Verrill.

Florida to Cape Cod; and locally farther north to Nova Scotia; littoral to 20 fathoms. In the Post-pliocene and Pliocene of South Carolina.

Ostrea Virginiana Lister. Abundant and well-grown.

Gulf of Mexico to Cape Cod, and locally farther north, at Damariscotta, Me., and in the southern part of the Gulf of St. Lawrence; low-water to 5 fathoms. In the Post-pliocene of Point Shirley, Gardiner's I., South Carolina, etc. Both the short, rounded specimens and the much elongated and narrow forms occur, as well as all the intermediate states, just as in many modern oyster-beds, showing that in Post-pliocene times the same kind of individual variations prevailed that have perplexed many modern systematists, but they have not yet become specific, nor even definite varietal characters.

BRYOZOA.

Hippothoa variabilis V. (*Escharella variabilis* Verrill, in Report on Invert.) Common on *Serpula*, etc.

Florida to Massachusetts Bay; low-water to twenty fathoms.

Biflustra tenuis V. (*Membranipora tenuis* (Desor); Verrill, in Report on Invert.) Common on shells.

Delaware to Massachusetts Bay; low-water to 15 fathoms.

Membranipora catenularia Smitt. Common on shells.

Arctic Ocean to Long Island Sound; low-water to 50 fathoms; northern Europe.

CRUSTACEA; ANNELIDA; PORIFERA.

Panopeus, sp. Several claws were found.

Eupagurus pollicaris Stimpson. A claw, probably from this bed, is recorded by Desor.

Florida to Massachusetts; 2 to 15 fathoms.

Balanus eburneus Gould. Common.

West Indies to Massachusetts Bay; littoral to 3 fathoms.

Balanus crenatus Bruguiére. Common.

West Indies to the Arctic Ocean; 3 to 15 fathoms.

Serpula dianthus Verrill. Very abundant.

North Carolina to Cape Cod; low-water to 15 fathoms.

Cliona sulphurea Verrill. The excavations are abundant in oyster shells.

Florida to Massachusetts Bay; 1 to 15 fathoms.

*List of Species found only in the upper Shell-bed.**

Buccinum undatum Linné. Common.

Arctic Ocean to New Jersey; northern coasts of Europe; low-water to 100 fathoms. In the Post-pliocene, Maine to Labrador and Northern Europe.

Neptunea curta Verrill. Several specimens, one of large size.

Labrador to Massachusetts Bay, and in deep water farther south, to Long Island.

Lunatia heros Adams. Common, but badly broken.

Georgia to Gulf of St. Lawrence; low-water to 40 fathoms. Post-pliocene, Canada, S. Carolina; Pliocene, S. Carolina; Miocene, Maryland to S. Carolina.

Var. triseriata. One specimen occurred.

Neverita duplicata Stimpson. One broken specimen.

Yucatan to Massachusetts Bay; low-water to 10 fathoms. Post-pliocene, Virginia to Florida; Pliocene, S. Carolina; Miocene, Maryland to S. Carolina.

Crucibulum striatum Adams. One large specimen.

New Jersey to Bay of Fundy; low-water to 40 fathoms.

Scalaria Grœnlandica Perry. Recorded by Desor, and doubtless from this bed.

Arctic Ocean to Block Island; 10 to 109 fathoms; Northern Europe. Fossil in Post-pliocene of Northern Europe.

Diodora noachina Gray. Two good specimens.

Arctic Ocean to Cape Cod; 8 to 70 fathoms; northern Europe.

Mya truncata Linné. Several large specimens.

Arctic Ocean to Nantucket Shoals; low-water to 10 fathoms; northern Europe. In the Post-pliocene, Maine to Labrador.

Thracia truncata Mighels and Adams. A few valves, special bed not indicated.

Greenland to Long Island; 10 to 60 fathoms.

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* A valve of *Saxicava Norvegica* was found after this list was put in typ Arctic Ocean to Massachusetts Bay.

Clidiophora trilineata Carpenter. One valve.

Florida to Gulf of St. Lawrence; low-water to 30 fathoms.

Macoma fragilis Adams, *var. fusca* (Say). A few valves.

Greenland to Georgia; littoral to 6 fathoms. In Post-pliocene from S. Carolina to Greenland.

Ceronia arctata Adams. Abundant and large.

Gulf of St. Lawrence to Long Island.

Mactra solidissima Chemnitz. Common, fragmentary.

Florida to Labrador; low-water to 12 fathoms. Post-pliocene of Point Shirley, Mass.

Cyclocardia borealis Conrad. Common.

Labrador to New Jersey; 3 to 80 fathoms. In the Post-pliocene of Gardiner's I.; Point Shirley; Labrador.

Cyclocardia Novangliæ Morse. A few valves.

Gulf of St. Lawrence to Long Island Sound; 3 to 40 fathoms.

Astarte undata Gould. One worn valve.

Long Island Sound to Northumberland Straits in Gulf of St. Lawrence; low-water to 100 fathoms. In the Post-pliocene of Gardiner's Island and Point Shirley.

Astarte castanea Say. Abundant.

New Jersey to Nova Scotia; 5 to 40 fathoms. In the Post-pliocene at Point Shirley.

Modiola modiolus Turton. Many worn valves.

Circumpolar; Greenland to New Jersey; low-water to 80 fathoms. In the Post-pliocene of Point Shirley, Canada, and northern Europe.

Anomia aculeata Gmelin. Common.

Arctic Ocean to Long Island Sound; low-water to 100 fathoms.

BRYOZOA.

Eschara verrucosa Esper. On shells of Ceronia.

Arctic Ocean to Nantucket Shoals; 3 to 45 fathoms. Northern Europe. In the fossil examples the surface of the cells is covered with radiating ridges, often rising into an eminence in the middle, and is perforated with numerous pores in the grooves. Orifice somewhat semicircular with a median avicularium in front of the proximal edge.

Celleporaria incrassata Smitt (?). Several.

Off Martha's Vineyard to Spitzbergen; 10 to 160 fathoms. The specimens are thick, irregular masses. The apertures are small, rounded, oblong, constricted on the sides by small points projecting inward near the middle, and sometimes with a small proximal spine.

CRUSTACEA AND ECHINODERMATA.

Balanus porcatus. Very abundant and large, but broken.

Arctic Ocean to Long Island Sound; 2 to 90 fathoms.

Strongylocentrotus Dröbachiensis A. Agassiz. Spines only. Arctic Ocean to New Jersey; circumpolar; low-water to 430 fathoms. Post-pliocene of Maine and northward.

List of Species found in both Shell-beds.

	Lower Bed.	Upper Bed.
Urosalpinx cinerea	Common.	One.
Tritia trivittata	Few.	Common.
Ilyanassa obsoleta	Few.	Few.
Crepidula fornicata	Abundant.	Few.
Crepidula plana	Common.	Few.
Ensatella Americana	Abundant.	Few.
Mya arenaria	Abundant.	Few.
Saxicava arctica	Few.	Common.
Venus mercenaria	Abundant.	Few.
Scapharca transversa	Abundant.	Few.
Mytilus edulis	Few	Common.
Crenella glandula	Few.	Several, large.
Ostrea Virginiana	Abundant.	Few.

Most of these belong to the fauna of the warm sheltered bays, and their occurrence in the upper bed may have been accidental in many cases. The few worn shells of such species, though found in the upper bed, may have been originally deposited in the lower one and afterward washed out by the waves and redeposited in the upper one.

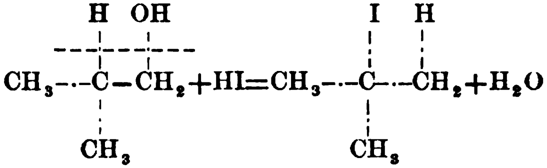
The absence of many species ordinarily abundant in the clay-beds of our New England Post-pliocene, (e. g., the species of *Nucula*, *Yoldia*, *Leda*, etc.), is doubtless due to the fact that such species lived only on muddy bottoms, while both of these deposits were made on sandy bottoms. They are, in fact, characterized by the nearly complete absence of all true muddy-bottom species.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Trimethyl-carbinol, and its supposed occurrence among the products of Fermentation.*—In examining a crude fusel oil, obtained from a manufactory of potato-spirit, which contained only a small quantity of amyl alcohol, Freund converted the isobutyl alcohol, boiling between 107° and 110°, which was principal product, into isobutyl chloride by heating with fuming hydrochloric acid in sealed tubes. Beside this substance, which boiled between 66·5° and 69°, 120 grams of a chloride boiling at 50°–53° was separated; and this on being heated with water in sealed tubes for ten hours gave trimethyl-carbinol, distilling from 82° to 85°, and solidifying on cooling. It was at first supposed, that as Butlerow has stated, this tertiary alcohol was contained in the crude product, being a product of the fermentation;

but the improbability of the existence of at least 12 per cent of this substance in a most carefully fractionated product such as was used in preparing the chloride, when it had a boiling point lower by 25°, led to the supposition that the trimethyl-carbinol was produced from the isobutyl alcohol itself, by the action of the hydrochloric acid. Pure isobutyl alcohol was taken and heated as above with hydrochloric acid; the tertiary chloride was obtained and converted into the alcohol by the action of water. Further experiments showed that the quantity produced depends on the relative amount of hydrochloric acid; and that hydrobromic and hydriodic acids act similarly. The reaction is as follows:—



$$CH_3-\underset{CH_3}{\overset{H}{C}}-\overset{OH}{CH_2}+HI=CH_3-\underset{CH_3}{\overset{I}{C}}-\overset{H}{CH_2}+H_2O$$

The author recommends this reaction for the preparation of trimethyl-carbinol. Isobutyl alcohol is saturated with HCl, five parts fuming acid are added, and the whole is heated in a water-bath for 24 hours.—*J. pr. Ch.*, II, xii, 25, July, 1875. G. F. B.

2. *On the Preparation of Epichlorhydrin.*—Prevost recommends, instead of treating dichlorhydrin by potassium or sodium hydrate in the cold, as in the usual method for the preparation of epichlorhydrin, to warm the dichlorhydrin in a roomy retort with a receiver attached, to a temperature not exceeding 130°, and to add sodium hydrate gradually; 250 grams being taken for 550 c.c. of dichlorhydrin. At the given temperature the epichlorhydrin distills over with the vapor of water. The process is more rapid and the yield is much larger than in the one formerly employed.—*J. pr. Ch.*, II, xii, 160, July, 1875. G. F. B.

3. *On Three new Pinacolins.*—The name pinacolin was given by Fittig to a body obtained by dehydrating pinacone, and extended by Butlerow to a class of acetones which contained tertiary-alcohol radicals. Pinacolin itself according to Butlerow is methyl-trimethylcarbyl-acetone, $C(CH_3)_3$-.-CO-.-CH_3. Wischnegradsky, under this chemist's direction, has prepared three new bodies belonging to this class, two of which are isomeric and contain seven atoms of carbon while the third contains eight. If one molecule of trimethyl-acetyl chloride be added carefully to two of zinc-ethyl, a violent reaction takes place and a solid substance is obtained having an odor like camphor and like mint, and a specific gravity of 0·831 at 0°. Analysis showed it to be ethyl-butyl-pinacolin, having the rational formula $C(CH_3)_3$-.-CO-.-C_2H_5. The other bodies were obtained by the action of ethyl-dimethyl-acetyl chloride upon zinc-methyl and zinc-ethyl respectively. The first is methyl-amyl-pinacolin, $C(C_2H_5)(CH_2)_3$-.-CO-.-CH_3, isomeric or rather metameric with ethyl-butyl-pinacolin, and having a boiling point 6·5° higher. The second is ethyl-amyl-pinaco-

lin, $C(C_2H_5)(CH_3)_2$·—CO—·—C_2H_5, having a specific gravity of 0·845 at 0° and boiling at 150·5° to 157·5°. By the oxidation of these pinacolins, they are split up, the CO group remaining with the tertiary radical. The author is continuing the investigation.—*Liebig's Annalen*, clxxviii, 103, Aug., 1875. G. F. B.

4. *Salylic acid identical with Benzoic.*—In 1860, Kolbe and Lautemann reduced chlorsalylic acid with sodium amalgam and obtained an acid which they called salylic acid, and which, though the formula was identical with that of benzoic acid, the authors believed, on account of the behavior of the acid and its salts, to be only an isomer of this acid. This opinion having been called in question by Reichenbach and Beilstein in 1864, who obtained benzoic acid by distilling salylic acid with water, Kolbe has re-examined the question more thoroughly, using for the reduction a kilogram of chemically pure chlorsalylic acid. A heavy sandy crystalline powder without odor was obtained entirely different in appearance from benzoic acid, as was also its lime salt. But on distillation with water pure benzoic acid went over, leaving in the retort a minute quanity of a pale yellow greasy substance. That this was the cause of the differences observed was proved by again mixing it with the benzoic acid; on crystallization the form of salylic acid returned. It hence appears that only benzoic acid is produced in the reduction of chlorsalylic acid; and that a quantity of some fatty body, produced during this process, so minute as not to affect the analytical results, is capable of altering the entire external appearance of the product. Treatment with permanganate also removes the impurity and yields pure benzoic acid; possibly by oxidizing the reduction product to benzoic acid.—*J. pr. Ch.*, II, xii, 151, July, 1875. G. F. B.

5. *On the Reduction-product of Chlordracylic acid.*—Inasmuch as Kolbe had shown in the above paper that the reduction-product of chlorsalylic (metachlorbenzoic) acid was benzoic acid, it occurred to Hartmann that the same fact might be true of chlordracylic acid (parachlorbenzoic acid). The acid was prepared by the action of phosphoric chloride upon paraoxybenzoic acid. The product of its reduction was benzoic acid, disguised as before by the presence of some tarry substance.—*J. pr. Ch.*, II, xii, 204, Aug., 1875. G. F. B.

6. *Use of Salicylic acid in Titrition.*—Weiske proposes to replace the usual litmus solution in titrition by salicylic acid. The acid is dissolved in water, filtered, and to the solution a few drops of ferric chloride is added. To the intensely colored solution, very dilute soda solution is added carefully from a burette to exact neutralization, whereby the liquid becomes reddish-yellow. If now a few cubic centimeters be added to the liquid to be titered, the latter remains uncolored. But as the point of neutralization is reached by the addition of the soda solution, the liquid acquires a deeper and deeper violet tint, which suddenly and immediately disappears again when the soda is added in the slightest excess. The final reaction is sharp and the results are much better than with litmus.—*J. pr. Ch.*, II, xii, 157, July, 1875. G. F. B.

7. *On the Synthesis of Malonic acid.*—PINNER some time ago showed that when zinc and hydrochloric acid acted on trichlorlactic acid there was produced, not monochlorlactic acid, but, by the loss of a molecule of water, chloracrylic acid. He now finds that if ethyl chloracrylate be boiled with barium hydrate, there is formed in considerable quantity a difficultly soluble barium salt, which yields an acid crystallizing from ether in large prisms and identical with malonic acid. It fuses at 132° and yields salts identical with those of malonic acid. The author represents the reaction in three stages: 1st. The replacement of the chlorine by hydroxyl producing acryl-lactic acid:—

$$\mathrm{CHCl{=}{:}{=}CH{-}{\cdot}{-}COOH+HOH=CH(OH){=}{:}{=}CH{-}{\cdot}{-}COOH+HCl}$$

2d. The addition of a molecule of water and then by rearrangement forming malonic aldehyde:—

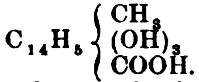
$$\mathrm{CH(OH){=}{:}{=}CH{-}{\cdot}{-}COOH+HOH=CH(OH)_2{=}{:}{=}CH_2{-}{.}{-}COOH}$$
$$\mathrm{CH(OH)_2{-}{\cdot}{-}CH_2{-}{.}{-}COOH=COH{-}{\cdot}{-}CH_2{-}{.}{-}COOH+HOH.}$$

3d. By oxidation giving $\mathrm{COOH{-}{\cdot}{-}CH_2{-}{\cdot}{-}COOH}$, malonic acid. —*Ber. Berl. Chem. Ges.*, viii, 963, July, 1875. G. F. B.

8. *On the Action of Chlorine on Pyrogallol.*—STENHOUSE and GROVES have investigated the action of chlorine on pyrogallol. When an acetic acid solution of this substance is saturated with chlorine in the cold, it becomes of a pale orange-red color. It is then placed in water at 70° for 20 minutes, the chlorine being continued, then rapidly cooled and a third of strong hydrochloric acid added, the chlorine continued for an hour, and the whole allowed to stand for 24 hours. Crystals are obtained which when purified appear as brilliant orthorhombic prisms insoluble in water, soluble freely in ether, and affording on analysis the empirical formula $C_{18}H_7Cl_{11}O_{10}$. To this substance the authors give the name mairogallol. If a solution of pyrogallol in acetic acid be saturated with chlorine in the cold as above, then concentrated hydrochloric acid added and a rapid current of chlorine passed through it, it effervesces strongly and in a few minutes solidifies to an orange-colored mass. After purification, crystalline crusts are obtained consisting of minute colorless radiating needles, easily soluble in water and alcohol and affording on analysis the formula $C_{18}H_6Cl_{12}O_{12}+(H_2O)_2$. This the authors call leucogallol. It is quite unstable.—*Jour. Chem. Soc.*, II, xiii, 704, Aug. 1875. G. F. B.

9. *On the Constitution of Emodin.*—Emodin is a constituent of rhubarb-root discovered by De la Rue and Müller in 1857. LIEBERMANN has succeeded in determining its rational constitution. The material operated on was obtained from Trommsdorff, in the preparation of chrysophanic acid. Heated with zinc dust, emodin yielded a hydrocarbon closely resembling anthracene but having a higher fusing point. Oxidation with chromic acid gave a white product soluble in concentrated sulphuric acid, which after precipitation passed only slowly into alizarin on fusion with potash. Treatment with acetic oxide gave two products: one in

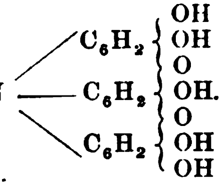
gold-yellow plates, the other in bright yellow needles. These bodies proved to be mono- and tri-acetyl-emodin and led to the formula $C_{15}H_{10}O_5$ for emodin itself. It differs from purpurin in having CH_2 more; or in having a methyl group in place of H. Hence the hydrocarbon which it yields is not anthracene but methyl-anthracene. This was proved by finding that it gave anthraquinone-carbonic acid on oxidation. From these data, Liebermann concludes that emodin is a tri-oxymethylanthraquinone of the following formula:



$$C_{14}H_5\begin{cases}CH_3\\(OH)_3\\COOH.\end{cases}$$

He has now undertaken the examination of chrysophanic acid, which appears to be similar, being dioxymethylanthraquinone. —*Ber. Berl. Chem. Ges.*, viii, 970. July, 1875. G. F. B.

10. *On the Coloring Matters Phlorein, Hæmatein and Brazilein.*—**Benedikt** has succeeded in producing from phloroglucin a coloring matter which shows a marked analogy with those of logwood and Brazil wood. This substance, which he calls phlorein, is prepared by the action of nitrous acid dissolved in nitric upon phloroglucin dissolved in ether. On distilling off the ether a magnificent violet semi-fluid residue remained, which, on adding water, was precipitated in red-brown light flocks. It was purified by reduction, and was obtained as a dark green powder with a strong metallic luster, nearly insoluble in boiling water, but soluble in alcohol, ether, and acetic acid with a deep brown, and in alkalies with an intense purple color. Owing to the difficulty of its combustion, its nitrogen was at first overlooked. The empirical formula $C_{18}H_{11}NO_7$ is assigned to it, the rational formula founded on this, deriving it from ammonia



$$N\begin{cases}C_6H_2\begin{cases}OH\\OH\\O\end{cases}\\C_6H_2\begin{cases}OH.\\O\end{cases}\\C_6H_2\begin{cases}OH\\OH\end{cases}\end{cases}$$

The great similarity of phlorein to hæmatein and brazilein led the author to prepare these bodies and to analyze them. He found that they both contained nitrogen, hitherto overlooked, and were otherwise similarly constituted. He writes phlorein as $3(C_6H_5O_3)N-(H_2O)_2$, hæmatein as $3(C_{16}H_{13}O_6)N$ and brazilein as $3(C_{22}H_{17}O_7)N$. Phlorein, however, yields a hydroderivative.—*Liebig's Annalen*, clxxviii, 92, Aug., 1875. G. F. B.

11. *Arithmetical relations between the Atomic weights.* Note by Prof. **Ira Remsen**, addressed to one of the editors, dated Williamstown, Mass., Oct. 11, 1875.—In the last October number of the American Journal of Science there is contained an article by M. D. C. Hodges, entitled, "On the Arithmetical Relations between the Atomic Weights," in regard to which I venture to offer a remark. The subject treated in the article is undoubtedly of in-

terest, and some credit is due Mr. Hodges for his apparently independent discovery of the relations described. I think, however, that a certain amount of credit should also be given to those who first called attention to these relations.

In the Annalen der Chemie und Pharmacie (VIII Supplement, S. 133) there is a masterly article by D. Mendelejeff in which the same subject is discussed that is discussed in Mr. Hodges's paper. Every relation referred to in the latter paper is made use of in the former paper, together with a great many beautiful relations which do not seem to have occurred to Mr. Hodges.

The subject of Mendelejeff's paper is: "The periodical law of the Chemical elements." Under this head he discusses the following subjects:

(1.) The nature of the periodical law.

(2.) On the application of the periodical law for the purpose of systematizing the elements.

(3.) Application of the periodical law for the purpose of determining the atomic weights of elements which have not been studied exhaustively.

(4.) Application of the periodical law for the purpose of determining the properties of undiscovered elements.

(5.) On the application of the periodical law for the purpose of correcting atomic weights determined by other means.

(6.) On the application of the periodical law for the purpose of enlarging our knowledge of chemical structural forms.

The same subject, further, has also been fully discussed from another standpoint by Lothar Meyer, both in the Annalen der Chemie und Pharmacie (VII Supplement, S. 354), and in his work entitled: "Die modernen Theorien der Chemie."

In a work by H. Baumhauer ("Die Beziehungen zwischen dem Atomgewichte und der Natur der chemischen Elemente") another exhaustive discussion of the same subject may be found.

Mr. Hodges appears to have overlooked the above-mentioned memoirs.

12. *Compressibility of Carbonic Acid.*—DR. ANDREWS, in a recent paper before the Royal Society, gives a continuation of his well known paper published in 1869, "On the Continuity of the Liquid and Gaseous States of Matter." A series of accurate measurements have been made of the corresponding temperature, pressure and volume of carbonic acid, to test the correctness of the laws of Boyle, Gay Lussac and Dalton. The apparatus employed was essentially the same as before, the chief improvement being in the method of ascertaining the volume of the gases before compression. The lower ends of the glass tubes containing the gases dip into small mercurial reservoirs formed of thin glass tubes, which rest on ledges within the apparatus. The leather washers used in packing the fine screws are now saturated with grease by heating them *in vacuo* under melted lard. The air enclosed in the pores of the leather was thus removed without the aid of water. This packing has never been known to fail in a

vessel filled with water. But with mercury it always yields after a few days, even under a pressure of 40 atmospheres. Unfortunately, the pressures must still be given as determined by an air or hydrogen manometer, as the only satisfactory method of reducing to absolute pressures would be by a mercury column, which, for the highest pressure would have to be over 1,200 feet high. Recently, however, a method has suggested itself by which it will probably be possible to measure pressures up to 500 or even 1,000 atmospheres directly in terms of a liquid column. The pressure required to produce liquefaction is given for various temperatures between 0° and 30° C., and agrees well with the results of the earlier measurements. The pressures are somewhat less than those given by Regnault, which were probably effected by the unavoidable pressure of air.

The compressibility was carefully measured at temperatures of 6·7°, 63·7° and 100°, the two latter temperatures being obtained by surrounding the apparatus with the vapor of methyl alcohol and water, and the pressure being carried to over 200 atmospheres. The results fully confirm those previously attained to the effect that its volume at high pressures and temperatures above the critical point is much less than that it should have if a perfect gas. Even at 100° the volume of 223 atmospheres is only about three-fifths of that given by the law, hence we may infer by analogy, that the critical points of the gases not yet liquefied are probably far below the lowest temperatures yet attained.

The law of Gay Lussac, while doubtless nearly true for the so-called permanent gases, is far from correct for gases at high pressures or near their critical points. The coefficient of expansion of carbonic acid under 1 atmosphere being ·0036, under 22 atmospheres it was ·0055 between 6° and 64°, and ·0051 between 64° and 100°. The corresponding coefficients for a pressure of 31 atmospheres were ·0068 and ·0059, and at 40 atmospheres ·0095 and ·0072.

A very large number of experiments were made on mixtures of carbonic acid and nitrogen, and the most important result was that the critical point is lowered by admixture with a non-condensable gas. Thus a mixture of 3 parts by volume of carbonic acid with 4 of nitrogen was reduced below −20° C. before condensation took place, even at the highest pressures. Even an addition of one-tenth its volume of nitrogen or air will materially lower the critical point. Finally, it appears that the law of Dalton is wholly incorrect at high pressures when one of the gases is not far from its critical point. This accounts for the observed anomalies in the tension of aqueous vapor when alone and when mixed with air, which are not accounted for as has been alleged by the hygroscopic action of the containing vessel. —*Nature*, xii, 300. E. C. P.

13. *Magnets formed of Iron Filings.*—M. Jamin has repeated the experiment of De Haldat, in which a brass tube was filled with iron filings and magnetized. The polarity was slight, not sensi-

bly increased when the stoppers closing the ends were tightened, and diminished slowly when sand was mixed with the filings. This observation is accurate but imperfect. In repeating it the filings were forcibly compressed by a small hydraulic press. When they begin to aggregate, the polarity is seen to augment considerably, and continues to increase with the pressure. Tubes were shown to the Academy from 8 to 10 cms. long and 3 cms. in diameter, which attract at least as many steel filings as pieces of good steel of the same dimensions. As the filings were of unknown origin some were prepared from very soft iron, perfectly reduced and without appreciable coercive force. The results were quite equal to the former. Here then is a metal which has no coercive force when it is continuous, but acquired a coercive force equal to that of steel when we reduce it to minute fragments, and then make them contiguous by pressure. Must not the polarity observed be attributed to this discontinuity? and is not the coercive force of steel to be explained by the same cause?

The distribution of the force in a magnet cannot be accounted for without regarding it as composed of rows of minute magnetic elements with opposite poles reacting on each other at a distance; and it is verified that the quantities of separate magnetism in each of them are increased by this reaction, from the extremity to the middle line (*Lamé Physique*, iii, 100). Up to the present time it seems to have been assumed that these elements are the molecules themselves; the preceding experiment seems to show that they are formed either by fragments of iron in proximity to one another, or by minute crystals agglomerated as in steel.

When before pressing the filings we interpolate among them substances which render the mass more homogeneous, the same polarity cannot be given as when they are unmixed. For example, by making a paste of chloride of iron and the filings, and rubbing it, we obtain, after a few days, a subchloride continuous in appearance, capable of being filed and polished like pure iron, but hardly magnetizable.

Iron reduced by hydrogen, and scales of oxide of iron, behave like iron filings; but magnetic and diamagnetic substances mixed with the filings alter notably the faculty they possess of being magnetized. The study of all these circumstances promises interesting researches. It is probable that by greatly increasing the compression of the powders the coercive force will be found to increase to a maximum, and afterward diminish when the approximation of the fragments has restored a sufficient degree of continuity to the mass.—*Comptes Rendus*, lxxxi, 205; *Phil. Mag.*, l, 255. E. C. P.

14. *Sounding Flames.*—M. C. DECHARME finds that persistent and varied sounds may be obtained by allowing a current of air to impinge on a jet of common illuminating gas issuing from a tube 3 to 5 mms. in diameter, and forming a flame 30 to 50 cms.

high. The air is conveyed in a similar tube, and the character of the sound will vary according to the part of the flame struck, the pressure of the air and the ratio of the diameter of the tubes.

When the jet of air, striking the flame near the top, gradually descends to within about a decimeter of the orifice, we see the column of flame first divide, lower itself, then twist under the jet, envelop it and let it pass, surrounding it with a blue flickering flame; a continuous tearing sound is then heard from this luminous vail. When the jet reaches within 2 or 3 cms. of the orifice of the gas (the air tube being held horizontally and being directed toward the flame) a loud hissing is produced. Finally, when the tubes touch, the hissing becomes harsher, or if the pressure is small, becomes a very clear and agreeable musical sound.

The experiment also succeeds well with a Bunsen burner, after closing the air holes. It is needless to add that no sound is produced with the simple gas jet if not lighted. By varying the conditions, as the nature and pressure of the gas and air, the position, diameter, form and nature of the tubes, various modifications of the sounds, and of the shape and color of the flame are produced. The rapid motions are readily analyzed by a revolving mirror. It would appear that the explanation of these facts depends not merely on the mechanical, but in a great measure on the chemical action of the air.—*Comptes Rendus*, lxxx, 1602.

E. C. P.

II. GEOLOGY AND NATURAL HISTORY.

1. *Evidence of glacial action upon the summit of Mt. Washington, N. H.*—Prof. C. H. HITCHCOCK has recently given the details of the discovery of transported bowlders upon the summit of Mt. Washington, 6,293 feet above mean tide water, in a paper read before the American Association for the Advancement of Science at Detroit. The following are the essential points of the communication. The bowlders do not exceed six inches in length so far as observed, and consist of the "Bethlehem gneiss" of the N. H. Reports, and must have been transported more than a dozen miles. They occur in the usual *moraine profonde* of the Northern drift and have the peculiar shapes common to glaciated stones, but do not well retain any glacial markings upon them. After observing that most of the angular blocks upon this summit rest upon this drift containing transported bowlders, the author comes to the conclusion that the greater part of the angular debris upon this mountain, though of the same material with the ledges, has been transported by the glacial agency from a few rods to three miles. Subsequently the frost has shattered them into angular fragments nearly covering the surface. Wherever ledges occur the same agency has broken off large masses which cannot be distinguished lithologically from those that may have been transported a considerable distance. Most of this angular debris is so far removed from existing ledges that frost

and gravity alone cannot have placed them where they now are though many of the piles are thus explicable. The ledges have their northwestern sides rounded and the southeast angular just like the striated bosses near the Lake of the Clouds.

Earlier observers have failed to discover these facts because the far transported stones have been brought to light by recent excavations in the earth beneath the almost universal carpet of frost-cleft fragments.

2. *Report on the Geology and Resourses of the Region in the vicinity of the 49th Parallel from the Lake of the Woods to the Rocky Mountains;* by G. M. DAWSON, Geologist and Botanist to the Commission. 388 pp. 8vo, with a colored geological map, views and sections. Montreal, 1875. Addressed to Major D. R. CAMERON, R. A., H. M. Boundary Commissioner.—This volume by an excellent geological observer, contains the results of an investigation of the Rocky Mountain formations north of and near the northern boundary of the United States, and is well illustrated by sketches, sections and maps. The author speaks first of the General physical geography and outline geology of the region, and then of the special geology of the Lake of the Woods, and the structure of the Rocky Mountains near the 49th parallel.

A large amount of facts are given respecting the Cretaceous and Tertiary strata, which will aid in settling doubtful points as to the distribution of the formations and as to the age of the Lignitic beds. Chapter VIII is devoted to this last subject, "the age of the Lignite-bearing formation and position of the line separating the Cretaceous and Tertiary," and the discussion is one of value as it comes from a careful worker over a new part of the Lignitic area. Many results of assays of the lignite are contained in a chapter on its economical value. Mr. Dawson discovered coccoliths and rhabdoliths in great numbers in the Cretaceous beds of Manitoba.

Chapters IX and X, 65 pages in length, treat of the Glacial phenomena and superficial deposits of the region examined, and Chapter XI of the Capabilities of the Region with reference to settlement. Following this there are Zoölogical Appendixes, Descriptions of fossil plants by Dr. Dawson, etc.

The Glacial facts collected are numerous and of wide import. The scratches over the region of the Lake of the Woods show that the movement of the ice there was in general *toward the southwest.* The courses, stated on page 205, vary from S. 5° W. to S. 87° W.; but the larger part are between S. 20° W. and S. 40° W. The author arrives independently at the conclusion, brought out by Major General G. K. Warren, that Lake Winnipeg formerly had a southern outflow—though differing from the latter in making this pre-Glacial in time. The author, to explain the glacial phenomena observed by him, supposes that the interior of the continent was under the sea, and that local glaciers and icebergs were the chief agents. He says that "the height of the highest terraces at the South Kootanie Pass is 4,400 feet, and I have little doubt but that these are of marine origin." Mr. Dawson appreciates the objection to this view from the absence of all marine remains and of other results of marine action that might, if such were the case, be reasonably looked for. He appears to suppose that there is no alternative between the theory of marine submergence and that of a great Polar ice-cap. The facts he presents with regard to the southwest course of the glacial scratches about the Lake of the Woods in connection with similar facts about Lake Winnipeg, and Lake Huron, in Ohio, etc., point to another view more consistent with the earth's meteorological laws, viz: that the glacier had its greatest thickness not in a Polar ice-cape, but toward the Atlantic border of the Continent, where the precipitation would necessarily have been the greatest; that the upper surface of the ice sloped thence away southeastward over New England, and southwestward over western New York, Ohio, Lake Huron and the region north of Lake Superior to the Lake of the Woods and Lake Winnipeg; that it thinned down to nothing over the great central and western area between 100° W. and the Cascade Mountains of Oregon, where precipitation now averages but one-third what it is on the eastern border and summer heat is great, and had much thickness in these latitudes only in the Arctic; but that the Arctic would have had its precipitation greatly diminished by the new conditions, and hence would, perhaps, have had less ice than now; that the assumed ice-cape was rather an ice-mantle thrown about the pole and descending along the borders of the continent, especially the eastern. Add local glaciers about the mountains, the Appalachians on the east and the various higher elevations on the west, and we have all the ice that is needed to explain the facts in North America upon the basis of a Glacier theory. The above mentioned meteorological conditions would have had special intensity, if the glacial cold and the excessive glacial precipitation were due to the confinement of the Gulf Stream with its heat to the temperate and tropical North Atlantic by a shoaling of the ocean between Scandinavia and Greenland. J. D. D.

3. *Report of a Reconnaissance of the Black Hills of Dakota made in the summer of* 1874; by WILLIAM LUDLOW, Captain of Engineers, Lieutenant-Colonel U. S. A., Chief Engineer Department of Dakota. 122 pp. 4to, with several maps. Washington, 1875. Engineer Department, U. S. Army.—This volume consists of a General Report of the Expedition by Col. Ludlow; a Geological Report, with a list of Trees and Shrubs, by N. H. Winchell; a Paleontological Report, by G. B. Grinnell; descriptions and figures of new Fossils, by R. P. Whitfield (including *Obolus Pectenoides* Whitfield, from Potsdam rocks on French Creek, Dakota, *Terebratula Helena* Whitf., from the Cretaceous on the northeast side of the Black Hills); and Tables of observations for time and latitude. Of the maps in the volume one is a Geological map, by Professor Winchell, of the Black Hills on a scale of 6 miles to 1 inch. The rocks described by Prof. Winchell

are the Cretaceous, the Jurassic and Triassic or Red beds, about 325 feet thick at the northern base of the Black Hills, a limestone formation lying *unconformably* beneath the Red beds, which is Carboniferous at top and may include other formations, and the Potsdam sandstone. Beneath the last are metamorphic schists and slate with high angle of dip, and granite. We understand from Mr. G. B. Grinnell, that the credit of identifying the fossils mentioned in Prof. Winchell's Report should have been given by the latter to Mr. R. P. Whitfield, instead of to himself. J. D. D.

4. *Discovery of the Horns of an extinct species of Ox in Ohio.*—A few years ago some workmen digging in the gravel on Brush Creek, Adams County, O., preparatory to laying foundations for the abutments of a bridge, discovered a pair of enormous horns about eighteen feet below the surface. On examination it was found that they were only the *cores* of the original horns, the exterior portion having perished. They measure nearly six feet from tip to tip, and are twenty-two inches in circumference. As the core of the horns of the ox is about one-third of the entire length, these must have been of enormous size. They were sent to Cincinnati, and have been kept in comparative obscurity until quite recently, when they came under the observation of Dr. Norton, who obtained permission to exhibit them before the Natural History Society. The Society has since secured them.

5. *On the Mesozoic formation of Mexico and its characteristic fossils;* by MARIANO BARCENA. 37 pp. 8vo. Mexico, 1875.—In this Memoir (in Spanish) Mr. Barcena treats of the distribution of the Cretaceous rocks of Mexico, their kinds and their fossils. The rocks include limestones, part of them metamorphic, different argillaceous rocks, micaceous, talcose and dioritic rocks, and they are stated to constitute a large part of the mountain elevations of the Mexican territory. The gray, compact limestones contain species of *Hippurites* and *Nerinea* in so great abundance that they are well called the Hippurite limestone and Nerinea limestone of Mexico. The author describes and figures two species of Hippurites, *H. Mexicana* and *H. calamitiformis*, several species of Nerinea, and also species of Ammonites and Scaphites, and an Aptychus, *A. Arenassii*. The reasons for referring the rocks of different districts to the Cretaceous, and especially to the Upper Cretaceous, are given at length by Mr. Barcena, in the course of his Memoir. From the evidence it is concluded that Cretaceous rocks occur in all the Mexican States; and the author consequently proposes that the map of North America in the Cretaceous period, which is given in Dana's Manual of Geology should be changed, by making the area of Cretaceous seas to extend from Texas westward to the Pacific Ocean—the facts serving to prove that the Gulf of Mexico and the Pacific were actually connected in the Cretaceous period. The Cretaceous area thus added, as shown on a map accompanying the Memoir, lies between a line from the Pacific to the Gulf northeast in course, running nearly through Chihuahua, and another parallel through Vera Cruz. Its extension northwestward and southeastward remains for future determination.

Mr. Barcena in another paper, published at Mexico in 1875, describes a fossil Sphæroma—*S. Burkartii*—from the valley of Ameca, Estate of Jalisco, which was obtained in an Artesian boring from beds that are probably Tertiary. No other fossils have been found at the place to fix the age; a figure of the species accompanies the paper. J. D. D.

6. *Mountain Making: The inequalities of the Earth's Surface viewed in connection with Secular Cooling.*—Mr. O. Fisher closes a mathematical discussion of this subject in the Cambridge Phil. Trans., vol. xii, part ii, with the following remarks:

Can we then attribute the intense corrugations which meet our observation to any other source than that of lateral pressure caused by a shrinking globe? It appears to me difficult to conceive any other. No local change in the condition of the superficial strata seems competent to produce a sufficient amount of extension in the superficial strata. I cannot perceive, as I have already endeavored to explain, how the deposition of thick beds upon the sea-bottom could affect it by their weight, and I have elsewhere, I think proved, that they could not do so by causing a new distribution of heat within the crust.* There is likewise much reason to believe that consolidation and metamorphism are accompanied by a contraction in volume.

The supposition that the earth became solid throughout, or nearly so, before a crust began to be formed, necesitates the consequence that the contraction, out of which the compression would arise, must be almost wholly confined to the cooled upper portion: it is upon this supposition that we have calculated its amount, and found it so much smaller than is warranted by natural appearances. If, however, we could suppose that a solid crust was formed upon the surface, long before its interior parts had fallen to the melting temperature, it seems that a much greater amount of compression might result, through the contraction extending far below the cooled upper portion. The objection made to this is, that the crust would break up as fast as it formed, and sink into the underlying fluid until the whole was brought to the melting temperature. But Mr. Mallet has shown, in the paper already more than once referred to,† that the difference between the specific gravities of solidifying and molten rock is so small, being scarcely six per cent, that when we consider the intermediate condition of viscosity, we need not assume this breaking up and sinking of the crust. And if in its early stages shrinkage cracks did form, it seems likely that the fluid which welled up into them would immediately solidify and seal them up. Scrope tells us that "the interior of a lava-stream often retains a very high temperature for a great length of time after its emission, continuing to send forth vapor from its crevices and fumeroles, and probably remaining liquid, and even more or less in motion, throughout its central and lower portion for years.‡"

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* Geographical Magazine, vol. x, p. 248.

† Phil. Trans. 1873, p. 160, § 50.

‡ Volcanoes, 2nd edition, p. 84, § 8.

Sir W. Thomson clearly contemplates the mode of solidification from without inwards as not impossible, for he says "If experimenters will find the latent heat of fusion and the variation of conductivity and specific heat of the earth's crust up to its melting point, it will be easy to modify the solution given above so as to make it applicable to the case of a liquid globe gradually solidifying from without inwards in consequence of heat conducted through the solid crust to a cold external medium."

If this supposition is admissible, there may have been a considerably larger nucleus enclosed within the crust in early times than we have at present, and a great portion of that nucleus may have consisted in superheated water, the rocks being in a state of igneo-aqueous solution:* and much of this water may have been blown off in steam during volcanic eruptions, by that means materially contributing to the diminution of the volume. I have suggested in my former paper read before this Society, that Mr. Sorby's observations on the water enclosed in granitic crystals, along with crystals of chlorides, renders it probable that the steam emitted in eruptions may be a constituent part of the deep-seated rocks, for it is probable that but a small part of the water contained in any magma would become confined in the interior of the crystals.

Here, however, the question arises whether it would be possible for a crust to form over a layer of molten rock in a condition of igneo-aqueous fusion. Would not the escape of the water cause a state of constant ebullition which would prevent the formation of any crust until it had ceased through the escape of water?

The idea of igneo-aqueous solution involves a condition of chemical combination between the water and the elements of the rock such that while that condition lasted there would be no tendency to separation between the two, and evaporation would be in abeyance. We can therefore conceive that at depths where the heat and pressure were sufficient there might be no tendency to evaporation and consequent ebullition, so that after the water had escaped to a certain depth ebullition would cease, and a crust be formed; but that more water would be ready to separate to a greater depth when its affinity for rock became lessened through the abstraction of heat, or diminution of pressure owing to the crust being partially supported by corrugation.

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* The following remarks upon the above passage were received from a quarte which disposes me to place great reliance on them:

"It is probable, or rather certain, that water substance, if it exists at grea depths under great pressure and at high temperature, is neither a gas nor a liquid being above its critical point.

"In this state substances are easily dissolved in it, not however so much account of a greater tendency to combine with water, as on account of a greate tendency of their own to dissipation. At still higher temperature the water su stance becomes itself dissociated into oxygen and hydrogen. But it does n follow that the dissolved substances will be precipitated. The magma may be the more complete the higher the temperature, because, though the bonds of affi ity have fallen away, the prison-walls prevent the elements from escaping. B of all the known regions of the Universe the most unsafe to reason about is th which is under our feet."

If such was the condition of the interior in the early stages of the cosmogony, a large portion of the oceans now above the crust may once have been beneath it, and thus we gain a novel conception of a sense in which the fountains of the abyss may once have been broken up.

A somewhat analogous escape of elastic vapor from beneath a denser envelope is I believe considered to be now taking place in the Sun.

The comparative small specific gravity of the earth as a whole, considering the great pressure to which its interior parts must be subject, has been held to prove that it is even now in a state of expansion through intense heat.*

But the question of its true condition is surrounded with difficulties. The supposition made by Sir Wm. Thomson of a cool nucleus covered by a sufficiently deep layer of molten rock is adopted by Dr. Sterry Hunt,† and would afford the conditions required by the argument of this paper. It would also afford the mean rigidity required to meet the objection to a fluid interior drawn from the absence of internal tides. But it would not account for the small specific gravity of the whole, nor yet would it meet the argument from precession in the form in which it was originally advanced by Mr. Hopkins: for a layer of fluid beneath the crust would destroy a rigid connection between it and the interior. This form of the argument however has been attacked, and apparently with some success.‡ The late Archdeacon Pratt, in defense of the general argument from precession, placed the matter in a simple light in a letter to *Nature* in 1871,§ and in that form I think it would be met by the supposition of a cool nucleus covered by a molten ocean with a solidified crust. But I would invite attention to the fact that the conclusions I have arrived at, concerning a highly fluid condition of the interior, have reference to an early period of its history; while the tests of the tides, and of precession, are confined in their application to the present. Nevertheless I am disposed to think that, at any rate, what may be termed a superheated condition of the mass still exists at no very great depth below the surface. By which I mean that if it be solid the solidity is due to pressure.

7. *Remarks on the Sedimentary Formations of New South Wales;* by Rev. W. B. Clarke, M.A., F.G.S., &c. Third edition, 62 pp. with geological sections. Sydney, New South Wales, 1875. —This memoir is a review of the geology of Australia by one who has worked long in geological investigations over New South Wales. The question of the age of the coal formation is fully discussed; and between the extreme limits adopted by different writers, the Jurassic period and the Lower Carboniferous, Mr. Clarke holds to his old opinion that they are of the latter age.

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* Herschel's Phys. Geography, 2nd edit., p. 7.

† American Journal of Science, vol. v, p. 264.

‡ General Barnard on "Problems of Rotary Motion," Smithsonian Contributions, No. 240.

§ Nature, vol. iv, p. 344.

The writer in his Exploring Expedition Report (1849), on New South Wales, sustained the view that their age was half way between those extremes, or Permian, and this is adopted by him in the last edition of his Manual of Geology (1874, p. 370). The absence of Tertiary rocks from all of Eastern Australia is accounted for on the supposition that the eastern coast, since the Tertiary era, has subsided many fathoms, as indicated by the existence of the great barrier coral-reef off the coast, any Tertiary formations then formed having been thus submerged. J. D. D.

8. *Geological Survey of Brazil.*—Prof. C. F. Hartt, recently of the Cornell University, has been placed in charge of the Geological Survey of Brazil by the Emperor. It is reported that the survey is to be continued four years. Professor Hartt's previous explorations in Brazil, first in connection with the Thayer Expedition, have eminently fitted him for the work.

9. *Geologische Beobachtungen auf Reisen im Kaukasus*, von H. ABICH. 138 pp. 8vo, with one chart. Moscow, 1875.—Prof. Abich, who has long been investigating the country of the Caucasus, here treats of the Geology of the Beschtau warm-spring formation; the North Caucasus Jurassic Coal formation; the Quartz-trachyte formation of Tschegem; and the Glaciers of the northern side of the Caucasus.

10. *Old beach on the Isle of Portland, near Weymouth, Southern England.*—Professor PRESTWICH describes a raised beach on the east side of the Portland Bell, its south extremity, from 24 feet to 36 feet above the present beach, near its northeastern extremity, and 53 feet at its northwestern. It contains numerous marine shells, the most of which are those of the adjoining seas, while a few belong some degrees farther north. The raised beach is capped by other beds of loam and rubble, in places 5 to 10 feet thick. At the Admirality quarries, about 400 feet above the sea, a bed of drift has afforded some bones of Mammals—*Elephas antiquus* and *E. primigenius*, and species of *Equus*, *Cervus* and *Bos*. Prof. Prestwich closes his paper with a discussion of the geographical conditions under which the deposits were made.—*Q. J. Geol. Soc.*, Febr. 1875, p. 29.

11. *Another New York Mastodon.*—A skeleton of a Mastodon has been found at Lisle, near Binghamton, New York, and is being exhumed for the Museum of the Cornell University.

12. *Microscopical Structure of Rocks*, with plates 7, 8; and *Granitic and other Ingenite Rocks of Yar-Connaught and the Lower Owle*, with plates 9 to 12, and other papers; by G. H. KINAHAN. From the Proceedings of the Roy. Irish Acad., II, vol. ii, Dublin, 1875.—These papers contain valuable facts on the nature and microscopic structure of the rocks referred to in the title. The term *Ingenite*, meaning "born, bred, or created within or below," is from a paper, on *the Microscope in Geology*, by Mr. D. Forbes, published in the Popular Science Review for October, 1867.

13. *Elemente der Petrographie*, von Dr. A. VON LASAULX, A. ö. Prof. Min. Univ. Breslau. 486 pp. 8vo. Bonn, 1875. (Emil Strauss).—

The descriptions in this work by Dr. Lasaulx are clear and concise, and generally include one or more chemical analyses. The classification is based first on (I) the fact of one mineral constituent, or (II) many; and, under these heads, on texture, as noncrystalline or crystalline, etc. An introductory portion reviews the structure of rocks, and the character of their constituent minerals, and a closing chapter treats of the origin of rocks and meteorites. The work is handsomely printed on fine paper.

14. *Minerals of Bourbonne-les-Bains.*—In addition to the mineral species of these hot waters mentioned on page 228 of this volume, Daubrée has detected also the rare species *phosgenite* (chloride of lead), covered with a crust of mixed galenite and gypsum; and, from the interior of a tube of bronze, a coating consisting of *atacamite.*—*L'Institut*, Aug. 4.

15. *Mineralogische Mittheilungen;* von V. RITTER V. ZEPHAROVICH, VI, 1875.—This sixth number of the mineralogical contributions by Professor Zepharovich contains descriptions of crystals of aragonite from Eisenerz and Hüttenberg, of native arsenic from Joachimsthal, and a discussion of the crystalline form of cronstedtite. The aragonite crystals were remarkable in exhibiting a large number of planes, mostly in the prismatic zone, with abnormal fractional indices, analogous to those long since described by Websky. E. S. D.

16. *Wiserine is Octahedrite.*—Professor Klein, of Heidelberg, in a recent valuable memoir on the crystallization of anatase (octahedrite) has shown that the wiserine (xenotime) from the Binnenthal, described by Kenngott, is actually identical with octahedrite. He remarks, however, that true xenotime is found at that locality, but the crystals do not agree with those described as wiserine.—*Jahrb. Min.*, 1875, 337. E. S. D.

17. *Chalcophanite, a new mineral;* by GIDEON E. MOORE, Ph.D. —Occurs in druses of lustrous crytals, and in foliated aggregates, lining the walls of cavities or grouped in stalactitic forms; in the latter case the crystals are grouped around a central core of a manganiferous mineral. The crystals are minute, but occasionally sufficiently distinct to admit of determination. Rhombohedral; occurring planes O and R, the basal plane preponderating, the crystals are sometimes in the form of thin scales. Measurements gave for $R \wedge R$ 114° 30′, and $O \wedge R$ 104° 13′ (required 103° 48′), whence $a = 3{\cdot}5267$. Basal plane brilliant, rhombohedral faces often deeply striated. Cleavage, basal perfect; micaceous, the thin laminæ slightly flexible. H.=2·5. G.=3·907. Luster metallic. Color bluish-black to iron-black. Streak chocolate-brown, dull. Opaque. Before the blow-pipe turns to a yellowish-bronze or copper-red; exfoliates slightly, and on continued heating darkens in color, and fuses slightly on thin edges. With the fluxes gives a strong maganese reaction, and on charcoal with soda gives a coating of oxide of zinc. Soluble in hydrochloric acid.

Analyses: 1, of distinct crystals; 2, of stalactitic aggregates possessing a radiated structure. In the latter case the material was not so pure as that used in analysis 1.

		$\ddot{\mathrm{Mn}}$	$\dot{\mathrm{Mn}}$	$\dot{\mathrm{Zn}}$	$\dddot{\mathrm{Fe}}$	$\dot{\mathrm{H}}$
1.	⅔	59·94	6·58	21·70	·25	11.58=100·05
2.	⅔	61·57	4·41	20·80	—	12·66

Analysis 1 gives for the oxygen ratio of peroxide, protoxides and water 4 : 1 : 2, and the same is obtained from 2 after making deductions for impurities present. This ratio gives for the formula $\dot{\mathrm{R}}, \ddot{\mathrm{R}}_2 + \dot{\mathrm{H}}_2$, or more exactly $(\frac{1}{4}\dot{\mathrm{Mn}}, \frac{3}{4}\dot{\mathrm{Zn}}), \ddot{\mathrm{Mn}}_2 + 2\dot{\mathrm{H}}$. In this case one-half of the water is regarded as basic. It is thus closely related to wad and psilomelane.

Chalcophanite is a result of the decomposition of franklinite and associated minerals. Found at the calamine locality at Stirling Hill, Ogdensburg, N. J.

Named from χαλκός brass and φαίνω to appear, in allusion to the change of color which the mineral undergoes upon ignition.—*Am. Chem.*, July, 1875. E. S. D.

18. *Statistics of Mines and Mining* (*Sixth Annual Report*); by Rossiter W. Raymond, U. S. Commissioner of Mining Statistics. 8vo, pp. 585. Washington, 1874.—Dr. Raymond's Reports on the Mines and Mining in the States and Territories west of the Rocky Mountains now form a most valuable repository of scientific and technical literature on all topics germane to this subject. The present volume contains the usual data respecting the mining industry of the ten States and Territories enumerated for the year 1873, together with special chapters on the following subjects:—the Mining and Metallurgy of Quicksilver; Hints on the Washoe process; Smelting in Park County, Colorado; Lead and Silver Smelting in Chicago; the Wyandotte Smelting and Refining Works; the Smelting Works of the Harz; the desilverization of Lead by Zinc; avoidable Wastes of American Smelting Works; Kustel's Roasting-furnace; Brückner's Cylinders; a Metallurgical Laboratory; Sinking Shafts with the Diamond Drill, &c.

These reports have become an acknowledged authority on the subjects which they treat, and are remarkable for fairness and accuracy of statement. They have for years been quoted and discussed, and in a large part translated in Germany, where the general features of our western mining industry are almost as widely known as they are in this. Thence, many skilled metallurgists, mining mechanics, and men otherwise scientifically trained, have been drawn to this country as permanent residents, greatly to our advantage. B. S.

19. *The formation of Starch* in chlorophyll-grains (in the green parts of plants), under the action of light, was made out by Sachs ten or twelve years ago, and has been confirmed, and the conditions recently studied in various ways by Famintzin, Kraus, and Godlewski. It is maintained, if we rightly understand, that this starch is directly formed from carbonic acid under the action of light, i. e., is a primary product of assimilation. Böhm, of Vienna, who contested this, but whose view according to Sachs "has already been sufficiently refuted," has returned to the sub-

ject, with new observations and experiments, now asserts, upon reasons stated, that

"The starch appearing in the seed-leaves of plantlets of Cress Radish and Flax, is not a direct assimilation-product, formed by the immediate decomposition of carbonic acid, but a transformation-product from a reserve of nutriment already present."

One would surely expect that the primary product of assimilation of carbonic acid and water would be an organizable plasma from which the starch-grains in question are constituted, not an organic structure such as a starch-grain is. It is not clear that there is much real difference between the two views, at least between those of Sachs and Böhm. For since starch, as the former explains, is formed and reformed in various parts of the plant and away from light and from chlorophyll, and in the leaf is allowed to be only one of the products of assimilation, Sachs' assertion that starch in chlorophyll-grains is "a product of assimilation," and Böhm's that it is "not a direct assimilation-product," are by no means in necessary contradiction. Nor is there apparent reason for supposing that a starch-grain in a potato-leaf is differently originated from one in a potato, except that the former is constructed of new-formed material. A. G.

20. *A Report on the Trees and Shrubs growing naturally in the Forests of Massachusetts;* by GEORGE B. EMERSON. Second edition. Boston: Little, Brown & Co., 1875. 2 vols. 8vo; with many plates.—The original edition, published in 1846, agreeably to an order of the Legislature by the Commissioners of the Zoological and Botanical Survey of the State, was a single volume, of 534 pages, to which 17 outline plates were added, the first drawing of the kind by Mr. Isaac Sprague. It was a classical work, of so great use and value, that the edition (we believe a large one) was soon exhausted. Author and artist still flourishing, and the zeal and public spirit of the former unabated, a new edition now appears, re-elaborated, enlarged, and amply illustrated, forming two volumes, which reflect great credit upon author, artists, and printer. There are 143 plates; and many of those from Mr. Sprague's drawings are colored chromoliths, more effective and truthful than any which we remember to have seen before. Several which exhibit the winter state of some of our deciduous trees are full of character. The introduction of scenic views of many foreign, mainly European trees, copied from various sources, adds a novel feature. Those who plant trees, and the many more who ought to be planting them, will value these attractive pictures the more, since they mainly represent well-grown specimens of trees which are known or may by expected to thrive in this part of the country. Some of them, from Figuier's Vegetable World, border a little on the sensational or factitious. The only one we are disposed to find much fault with stands in the place of honor, fronting the title-page, "*Sequoia gigantea*, or Giant Pine of California." Why should it be called a Pine? Cypress would be nearer the mark if any of the old

common names need to be used. And why should the tree have a wall of the Yosemite Valley for a back-ground, since Mr. Emerson well knows that it does not grow there, however it may be with M. Figuier and his artist, Faguet. Through some oversight, in our copy, a plate, evidently representing the Black Oak, does supernumerary duty for Red Oak, which has two good plates of its own, one for the acorns and leaf, another for the port of the tree. There is a good figure of *Salix petiolaris*, but no letterpress. The severance of the long-standing connection between the Weeping Willow and Babylon, which has been referred to in this Journal, has escaped the venerable author's attention. We may be permitted to find some fault to relieve the monotone of praise which this attractive book calls forth. If space permitted we would call particular attention to the new part of the Preface, with its remark that, in the author's own experience upon our sea coast, European Oaks, Beech, Linden, Maple, Elm, Ash, Mountain-Ash, and Pine, are "more hardy than the corresponding American trees," and its warning close from Bryant's early poem, as well the important chapter on the uses, continuation and improvement of our forests. A. G.

21. *On the Classification and Sexual Reproduction of Thallophytes;* by W. T. Thiselton Dyer. An article of 33 pages 8vo, revised and reprinted from the Quarterly Journal of the Microscopical Society, London, July, 1875.—A timely and good *resumé* of all the important recent contributions to our knowledge of this subject, based upon the fourth edition of Sachs' *Lehrbuch*, with historical and critical discussion. The result is the provisional adoption of Sachs' classification, in which *Algæ*, *Fungi*, and *Lichens*, are relegated to the past, all Thallophytes are arranged in two parallel series, one with chlorophyll (*Algæ* and *Characeæ* of old) and one without, the latter being to the former nearly what *Monotropeæ*, *Orobancheæ*, *Cuscuta*, and the root-parasitic orchids are to the orders they respectively pertain to, or are parasitic representatives of; and the classes they are arranged under in a double series, viz: *Protophyta*, *Zygosporeæ*, *Oosporeæ*, and *Carposporeæ*, founded on characters of reproduction alone, and rising in increasing complexity and differentiation in the order indicated above. The green series begins below with the *Cyanophyceæ*, followed by the *Palmellaceæ*, and ends with *Florideæ* followed by *Characeæ*. The parallel *Fungi*-series begins with the *Schizomycetes* (Bacterians) followed by the *Saccharomycetes*, and these by the *Zygosporous Myxomycetes* and *Zygomycetes*, and ending with the *Ascomycetes*, under which are placed the *Lichens*, and finally the *Basidiomycetes*. This arrangement being "according to their morphological complexity," is thought to shadow forth, to a certain degree or with some probability, the "phylogeny" of the lower orders of the vegetable kingdom, although not with any strict genetic signification. As to this degradation of the *Lichenes*, it is remarked that the general result of the later investigations has been, on the whole, confir-

matory of Schwendener's view. A history of our knowledge of sexual reproduction in plants, and more particularly, of cryptogamous plants, occupies two or three of the earlier pages. To Thwaites is attributed the origination of the view that conjugation is sexual reproduction in its most generalized form. It seems strange that any different view could have been supported; but it is not so very long ago that it was maintained by Mohl. Thwaites and Jenner, as early as 1848, inferred "an apparent adumbration of the sexes" in *Zygnema*, &c., from the formation of the spore in the cells of one of the wholly similar parents, instead of between the two as in *Desmidieæ* and *Diatomaceæ*, but such adumbration is rather "apparent" than real. It was Thwaites who, in 1847, discovered conjugation in *Diatomaceæ*. Morren had long before discovered it in *Desmidieæ* (*Closterium*). The cilia of antherozoids were discovered in 1840, by Thuret, whose recent death, in the midst of important work, botanists may well deplore. In 1852 Thuret first obtained actual fertilization by the antherozoids in the case of *Fucus*. A. G.

22. *Monographie der Juncaceen vom Cap*, bearbeitet vom FRANS BUCHENAU. Bremen, 1875. 8vo.—This monograph of the *Juncaceæ* of the Cape of Good Hope, by Dr. Buchenau, is a separate issue from the *Abhandlungen des naturwissenschaftlichen Vereins zu Bremen*, Band iv, Heft 4, pp. 393–512, and is illustrated by 7 quarto plates, filled with details, devoted to the illustration of 32 species, chiefly of the genus *Juncus*. At the close is given a *resumé* of the literature of the South African *Juncaceæ*, from the Supplementum Plantarum of the younger Linnæus down to Steudel's Synopsis of *Glumaceæ*. Also names are given to the numbers in Drège's distributed collection. A. G.

23. *Elements of Zoology;* by SANBORN TENNEY. 8vo, 503 pp., with 750 wood-cuts. New York (Scribner, Armstrong & Co.) 1875.—In this work the author has attempted to give an outline of the Animal Kingdom by describing the larger groups, down to the orders and sub-orders. The principal characters of the classes are given with considerable fullness, in most cases, together with a summary of the orders or other subdivisions into which they are divided, and then each of the orders or sub-orders is briefly characterized and illustrated. The author has, in most cases, given under each class not only the classification that he adopts, but one or more of the different classifications that have been proposed by some of the leading writers on zoölogy. This feature will much increase the value of the work as a convenient book of reference, and will, perhaps, enable many teachers to use it as a text-book who may not wish to adopt the classification preferred by the author. The illustrations are very profuse and are, for the most part, well chosen and much superior in their execution to those ordinarily seen in similar text-books. This collection of figures would of itself render the work a very useful and valuable one for the use both of students and teachers. About five hundred of these cuts have been previously used in the

author's Manual of Zoölogy, and the two hundred and fifty additional ones have been selected from various other works. A list of the cuts is given, with the origin of each, which is a very commendable feature. The descriptions of the characters of the various groups are generally correct, though often rather too brief, and perhaps for this reason the statements are sometimes so general and indefinite as scarcely to be easily understood by beginners. It is natural, also, to expect positive errors in all works of this kind, for no one person can be familiar with the whole range of zoölogical science, as developed in recent times. We have noticed a few such errors in the work before us. Thus on pages 374 and 380 the well known Holothurian genus *Synapta* is included among the Gephyreans, an error so obvious that it would seem to have been accidental. A more serious error occurs on pages 216 and 227, where *Mosasaurus* and the allied genera are included among the Enaliosauria. On page 242 there is a confusion of ideas, although most of the facts are correctly stated, but the ordinary and normal larval state (Siredon-stage) of salamanders belonging to the genus *Amblystoma*, and many others, should not be confounded with the adults of genera like *Proteus* and *Menobranchus*. Yet, notwithstanding a few defects, we consider this the most useful manual of general systematic zoölogy that has hitherto been published in this country. V.

24. *First Book of Zoology;* by EDWARD S. MORSE. 12mo, 188 pp. 158 cuts. New York (D. Appleton & Co.) 1875.—In this little volume Prof. Morse has put, in a very simple and attractive form, a surprising amount of zoölogical instruction, such as beginners desire and need to have. The numerous figures are good and well chosen, and most of them are new and drawn from native animals by the author. They are mainly in outline and the author judiciously recommends learners to copy them in all cases, but if they should go farther and draw some of the real animals or their parts it would be still better. The book is devoted wholly to the Mollusca, Insects, Crustacea and Annelids, with a short but useful chapter on Vertebrates. The Protozoa, Radiata, Polyzoa, Brachiopoda, Tunicata, Cephalopoda, and most of the "Vermes" are not mentioned, the design being to describe those groups of animals that young students can most easily obtain for examination. Scientific names are wholly omitted, and although to many this may appear to be an advantage, we imagine that some indication of the name of each species figured, even in a list of figures at the end of the volume, would have considerably increased the value and usefulness of the book, both for pupils and teachers, for however little value we may attach to mere names, most persons, and especially young pupils, are generally anxious to know the names of animals that they may see, and read about, or at least to know whether they really *have* names, of which there is here no intimation, in most cases, and we fear that the majority of teachers and parents who may wish to use this book in teaching will hardly be able to give the missing

names of most of the figured species, which the author could so easily have supplied.

The style is clear and simple, and the descriptions are generally accurate, though brief. We have noticed but few errors worthy of note, and these will doubtless disappear in a second edition. In the figure of the mouth-parts of a craw-fish, on page 136, a superfluous appendage is given to the inner side of the mandible, and on page 147 the "*cervical suture*" of the carapax is erroneously said to represent "the dividing line between the head and thorax," which would be an absurdity, considering that the carapax is an outgrowth from the third and fourth (antennary and mandibular) cephalic segments. In the last part of the book the definitions of some of the classes and other groups are faulty, as for example, in case of Arachnida on page 185, where the definition would exclude a large proportion of the species belonging to it, as it really applies to but two of the four orders. The same may be said of the definition of "Vermes" both on pages 185 and 160. But there is much in the book that will be of interest and useful, even to advanced students. The chapter on the bones of the leg and wing of birds is particularly valuable. V.

25. *The Bones, Ligaments, and Muscles of the Domestic Cat;* by H. S. Williams. *With an atlas of photo-lithographic plates, reduced from those of Straus-Durckheim.* New York, (G. P. Putnam's Sons.) 1875.—The text of this work forms a small volume of 86 pages, devoted to the explanation of the accompanying plates. It is essentially a translation of the original explanations of Straus-Durckheim, with such revisions as the author has thought necessary, and with the substitution of Latin names of the parts for the original French terms. The plates have been well reduced by the Osborne photo-lithographic process to a small folio size, and correspond to the outline plates, 2 to 13, of the original work. The work is intended as a reference book for use in the practical study of comparative anatomy, and for this purpose will doubtless prove very useful, as it can be sold at a price that will bring it within the reach of all students, while the original work is costly and quite generally inaccessible in this country. We understand that Dr. Williams proposes to issue additional volumes with similar reductions of the plates illustrating the anatomy of the viscera and other organs. It is to be hoped that he may soon accomplish this, for in most cases it is quite as important, or even more so, that students should thoroughly study the digestive, circulatory, respiratory, and nervous systems, as the bones and muscles. V.

26. *Distribution of the Alcyonoid polyps of the Umbellularia group.*—The Umbellulariæ have been found

Off the coast of Greenland in 236, 410 and 121 fathoms.

In the *Atlantic* by the Challenger Expedition: (1) between Cape St. Vincent and Maderia, 35° 20′ N., 134½° W., in 2,125 fathoms; (2) 300 miles to the eastward of the St. Paul's rocks, 1° 47′ N., 24° 26′ W., in 1,850 fathoms; (3) off Brazil, 10° 11′ S., 35° 22′ W., in 1,600.

In the Antarctic by the Challenger Expedition: (1) between Prince Edwards' and the Crozet Islands, 46° 46′ S., 45° 31′ E., in 1,375 fathoms; (2) 84 miles to the westward of Hog Island, one of the Crozets, 46° 16′ S., 48° 27′ E., in 1,600 fathoms; (3) near the ice barrier, 62° 26′ S., 95° 44′ E., in 1,975 fathoms; (4) north of the ice barrier, 53° 55′ S., 108° 35′ E., in 1,950 fathoms; (5) south of Australia, 42° 42′ S., 34° 10′ E., in 2,600 feet; (6) also in the Pacific, southwest of the Louisinde group, in 2,440 fathoms.

The Umbellulariæ are usually associated with such deep sea animals as *Ophiŏglyphæ*, *Brisingæ*, *Pourtalesiæ*, *Ananchytids*, *Munopsids*, *Petalophthalmi*, *Gnathophausiæ*, *Macruri*, *etc.*—*R. V. Willemoes-Suhm in Ann. Mag. N. H.*, IV, v, 312.

III. ASTRONOMY.

1. *Observations de Poulkova;* publiées par OTTO STRUVE, Directeur de l'Observatoire Central Nicolas, volume vi. *Observations faites au Cercle Méridian*, S' Petersbourg, 1873, folio, pp. v and 545.—Among the most important astronomical publications of the past few years, must be enumerated the magnificent series of volumes published at Poulkova, one of the beautiful suburbs of St. Petersburg, by the Imperial Central Astronomical Observatory. The sixth volume of this series which contains the observations made from 1840 to 1855 by means of the Repsold meridian circle, has recently been received in this country. The number of observations contained in this single volume amount to 21,000, and appertain to the stars from the first to the sixth magnitudes, situated between the north pole and 15° of southern declination. The whole series of meridian circle observations will require still another volume, the seventh, and have been amassed through the labors of Sabler, 1840 to 1854, Döllen, 1844 to 1849, Lindhagan, 1854 to 1855, Winnecke, 1858 to 1864, and Gromadski, 1866 to 1869. Through the whole of this long period, no important changes have been made in the construction of the instrument, and but few changes in the methods of observation. The computations have been executed within the past few years under the successful direction of Wagner and Von Asten. C. A.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Journal of the Scottish Meteorological Society*, with Tables for year ending 31st December, 1874. Published quarterly. July, 1874—July, 1875. (William Blackwood & Sons, Edinburgh and London.)—This number of the Scottish Meteorological Journal contains, among its papers, a very important memoir on the influence of weather on mortality from different diseases and for different ages, by Mr. Alexander Buchan and Dr. Arthur Mitchell, extending to 80 pages. The enquiry embraces the three decades from 1845 to 1874 inclusive; and the Registrar-General's weekly reports of deaths in London are the basis of the

curves in the diagrams, the reports being full and the deaths all having taken place under one climate. To construct a curve for the deaths by any disease an average weekly death-rate of 20 is stated to be required. In connection with the tables of diagrams for each disease, there are meteorological reports for the same period.

It appears that in London the deaths from all causes for the 30 years, had their extreme maximum in December and January; that with the beginning of April there commenced a long decline in number to the extreme minimum in June; then began a rise to a second lower maximum in the latter half of July, continued through the first half of August; and then a decline to another minimum in October, after which the rise toward the December maximum began.

The following are some of the facts indicated with regard to deaths in London: The death-rate for scarlatina is low for March, April, May, and high for October and November; for whooping cough, high from February, March to April, and low for September and October; for typhoid fever, low for May, June and early July, and high for November and late October; for dysentery and diarrhœa, low for January and June, and very high in the latter half of July and August; for bronchitis, pneumonia and asthma, high in December, January and February, with the extreme in January, and low from June to October; by suicide, maximum from April to the last of June, and an extreme minimum in the early part of February. These cases and those of all other diseases of which the deaths were sufficiently numerous are exhibited in the diagrams.

The paper next takes up the question of deaths and their diseases during six sections of the year distinguished by differences in temperature and dryness; then the influence of weather on deaths at different ages, showing one maximum for children under one year in the latter half of July and beginning of August, and another higher in December, January and February, while for individuals above one year only the latter maximum is distinct. Afterward the influence of weather on the mortality of the two sexes is illustrated, bringing out a maximum for both in December, January, and the latter part of March, a minimum for both in June, a second maximum in the latter half of July and first half of August, and a second minimum in October, with the winter maximum higher, and the summer minimum lower, for females than for males. Another table shows that, deducting the deaths from bowel complaints, there is no summer maximum for deaths by all other diseases, but instead a continued minimum which is greatest in July and August. The diseases which are like bowel complaints in having their maximum in summer, are shown to be jaundice, tabes mesenterica, enterites, thrush, atrophy and debility. The article closes with four long tables; Table I, showing the different diseases for each year, together with a column stating the population of London; Table II, giving the mean weekly death rate on an average of the 30 years for each of the different causes of death; Table III, showing for the different diseases and other registered causes of death for the 30 years the *percentages* of *excess* or *defect* of the death-rate of each week of the year as compared with the general average of the 52 weeks; and Table IV, showing the average death-rate *at different ages* from all causes.

2. *Voyages a la Cote Nord-Ouest de l'Amérique exécuté durant les Années*, 1870–72; par ALPH. L. PINART. Vol. I, Partie I, Histoire Naturelle. Paris, 1875. 4°, pp. 51, with plates A to E.—Mr. Pinart has devoted himself to the self-imposed task of exploring the Northwest Coast of America, with special reference to the ethnography of the tribes inhabiting Alaska and the Aleutian Islands. The author, being himself especially devoted to ethnographical and linguistic studies, has sought, in this part of his beautiful work, the aid of men eminent in their own departments in working up the natural history materials collected by him. M. Jannettaz, President of the Geological Society of France, has prepared the catalogue of geological specimens and observations on them; M. Léon de Cessac has made the Microscopical and Chemical analyses of some of the rocks of Alaska; the Paleontology is prepared by M.M. Gaudry and P. Fischer; and the Zoology is by Fischer, E. Perrier and P. Gervais. The author proposes to issue three volumes of this series, each to contain six parts similar to the one now issued. He has favored us with a sight of the plates illustrating the ethnology of his voyage, which he has enriched by researches during three winters in St. Petersburgh, where the extended collection made by the missionaries of the Greek Church, during a century or more, were liberally placed at his disposal by the Russian Government. Having completed the manuscript, and left the execution of the work to his publisher, he is again about entering on a new exploration, under a commission from the French Government, having for its object the study of the ethnology and languages of the southern races of the west coast of both North and South America. After visiting Washington he plans to go to San Francisco and thence by the coast to Valparaiso, with a view of determining if possible, besides other things, the source and direction of migration of the native American tribes of both hemispheres. Fortunately for science, M. Pinart is willing to devote his time and ample private fortune to these researches. His Government has encouraged him by giving him a commission which requires him to report specially upon certain points with reference to the Central American States, with which his former voyages have already made him familiar. B. S.

3. *General Index of Professional and Scientific papers contained in the United States Coast Survey Reports from* 1851 *to* 1870. Constituting Appendix 17 in the Report for 1871.—The Coast Survey Reports issued under the late eminent Superintendent, Prof. Bache, and also his able successor, Prof. Peirce, contain a large number of original memoirs of high scientific character,

and consequently this Index has great importance. The volumes contain many papers discussing tides in general, and the tides of the Atlantic, Pacific, and of special portions of the coast of the United States by Professor Bache; also others on tides, by C. A. Schott, G. Wurdemann, &c.; several papers on the Gulf Stream, its depth, temperature and soundings, by Prof. Bache, with others by E. B. Hunt, H. Mitchell, &c.; papers on hydrographic changes produced by tides and currents; on the measurements of heights, by T. J. Cram; on the chronometric determinations of longitude, by W. C. and G. P. Bond, A. D. Bache, C. A. Schott; on telegraphic determinations of longitude, by S. C. Walker, B. A. Gould, G. W. Dean, &c.; on longitude by occultations of the Pleiades, and lunar tables used in the same, etc., by Prof. Peirce; on the observations of the solar eclipse of July, 1851, with reference to longitude determinations, by Prof. Peirce; use of zenith-telescope for observations of time, by J. E. Hilgard; on star-catalogues, by C. A. Schott; on solar eclipses. by various authors; on solar spots, by C. A. Schott; on the moon's mass, as deduced from a discussion of the tides of Boston harbor, by William Ferrel; on deep sea soundings, by W. P. Trowbridge; on terrestrial magnetism, giving observations in the United States and general discussions, by Prof. Bache (and including his memoir on his magnetic survey of Pennsylvania and the adjoining States in 1834 to 1862), Mr. Schott (whose papers are very numerous), W. P. Trowbridge, J. E. Hilgard, G. W. Dean; on deep sea dredgings, by J. W. Bailey, L. F. Pourtalès, Prof. Agassiz; on the Florida coral reef, by Prof. Agassiz, also by E. B. Hunt; on earthquake waves in the Pacific, by A. D. Bache, also by J. E. Hilgard; besides a large number of papers bearing on other topics arising out of the survey. The papers on physical subjects are among the most important that have been anywhere published.

4. *On a Discovery of Meteoric Iron in Missouri*; by G. C. BROADHEAD (Mines, Metals and Arts, St. Louis, for Sept. 20).—Nearly six months ago I obtained knowledge of a mass of meteoric iron in Bates County, but only recently found out just where it was, and last week I went to Butler and obtained it. It was plowed up in a field by a man named Abram Crabbe, living eight miles southwest of Butler. For a long time it remained scarcely noticed by him, but at last, thinking it rather heavy, he brought it into Butler and left it at a blacksmith's. When I heard of its being there, I requested a fragment. A piece was cut off; the smith, first heating it, was occupied nearly two hours in the cutting.

This is the first meteorite that we know of having been found in Missouri. Its total weight is a little less than 90 pounds, and it is a rough-looking, rather irregular mass, somewhat pitted over the surface, as they generally are. From its great weight in comparison with its size and luster, I suppose it to be nearly all native iron with undoubtedly some nickel in its composition.

Pleasant Hill, Mo., September 1.

5. *On Poisons in relation to Medical Jurisprudence and Medicine.* By ALFRED SWAINE TAYLOR, M.D., F.R.S., &c. Third American, from the third English edition. Philadelphia, H. C. Lea. 1875. 8vo, pp. 788, with 104 illustrations in the text.—Dr. TAYLOR's manual on poisons long ago became a standard authority, and the appearance of a new and thoroughly revised edition brought down to May, 1875, will be welcomed by all interested in such studies. A rapid survey of the volume shows extensive changes, both by addition and omission. In fact the work has been remodelled in accordance with the changes of toxicological science, to meet the wants of students in law and medicine. B. S.

6. *On the Strength of the Lion and the Tiger*; by Rev. SAMUEL HAUGHTON.—In Nature, vol. xii, p. 474, in a review of Dr. Fayer's book on the tiger, doubts are thrown by the reviewer on the statement that the tiger is stronger than the lion. Dr. Fayer's statement cannot be contradicted by any person well acquainted with both animals. In my book on "Animal Mechanics," published in 1873, I have proved, p. 392, that the strength of the lion in the fore limbs is only 69·9 per cent of that of the tiger, and that the strength of his hind limbs is only 65·9 per cent of that of the tiger.

I may add that five men can easily hold down a lion, while it requires nine men to control a tiger. Martial also states that the tigers always killed the lions in the amphitheatre. The lion is, in truth, a pretentious humbug, and owes his reputation to his imposing mane, and he will run away like a whipped cur, under circumstances in which the tiger will boldly attack and kill.—*Nature, Oct.* 7, *in a letter dated Trinity College, Dublin, Oct.* 1.

OBITUARY.

WILLIAM JORY HENWOOD, F.R.S., F.G.S., died on the 5th of August, in his 71st year. He was the author of papers on Mineralogy, and of elaborate memoirs on the metalliferous deposits of Cornwall and Devon, and various other topics connected with mines and mining.

SAMUEL D. TILLMAN, Ph.D., LL.D., died September 4th, at the age of 62 years. Prof. Tillman's contributions to chemical literature have been mainly in the departments of chemical philosophy and nomenclature.

Unsere Körperform und das Physiologische Problem ihrer Entstehung. Briefe an einen befreundeten Naturforscher von WILHELM HIS. Mit 104 Holzschnitten. Leipzig, Verlag von F. C. Vogel, 1875. 8vo, pp. 224.

Cholera Epidemics of 1873 in the United States. 1026 pp. 8vo. Washington, 1875. 43d Congress, 2d session, House of Representatives. Ex. Doc. No. 95. Besides the long detailed Report on the Cholera epidemic of 1873 in North America by John M. Woodworth, M.D., this volume contains a report by Ely McClellan, M.D., on that of 1832, 1833, 1834, of 1848, of 1854, and of 1865, 1866, in North America; and also a history of the travels of Asiatic Cholera in Asia and Europe, by John C. Peters, M.D. The volume is illustrated by maps, and closes with a very extended bibliography.

APPENDIX.

ART. LI.—*On the Odontornithes, or Birds with Teeth;* by Prof. O. C. MARSH. With plates IX and X.

REMAINS of birds are among the rarest of fossils, and few have been described except from the more recent formations. With the exception of *Archæopteryx* from the Jurassic, and a single species from the Cretaceous, no birds are known in the old world below the Tertiary. In this country, numerous remains of birds have been found in the Cretaceous, but there is no satisfactory evidence of their existence in any older formation, the three-toed footprints of the Triassic being probably all made by Dinosaurian reptiles.

The Museum of Yale College contains a large series of remains of birds from the Cretaceous deposits of the Atlantic coast and the Rocky Mountain region, thirteen species of which have already been described by the writer. The most important of these remains, so far as now known, are the *Odontornithes*, or birds with teeth, and it is the object of the present communication to give some of the more marked characters of this group, reserving the full description for a memoir now in course of preparation.

The first species of birds in which teeth were detected was *Ichthyornis dispar* Marsh, described in 1872.* Fortunately the type specimen of this remarkable species was in excellent preservation, and the more important portions of both the skull and skeleton were secured. These remains indicate an aquatic bird, fully adult, and about as large as a pigeon.

The skull is of moderate size, and the eyes were placed well forward. The lower jaws are long, rather slender, and the rami were not coössified at the symphysis. In each lower jaw there are twenty-one distinct sockets, and the series extends over the entire upper margin of the dentary bone, (Plate IX, figures 1 and 2). The teeth in these sockets are small, compressed and pointed, and all are directed more or less backward. The crowns are covered with nearly smooth enamel. The maxillary teeth appear to have been numerous, and essentially the same as those in the mandible. Whether the premaxillary bones supported teeth, or were covered with a horny beak cannot be determined from the present specimen.

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* This Journal, vol. iv, p. 344, and vol. v, p. 74.

The scapular arch and the bones of the wings and legs all conform closely to the true avian type. The sternum has a prominent keel, and elongated grooves for the expanded coracoids. The wings were very large in proportion to the legs, and the humerus had an extended radial crest. The metacarpals are coössified, as in recent birds, thus differing widely from those of *Archæopteryx.* The bones of the posterior extremities are slender, and resemble those of some aquatic birds. The centra of the vertebræ are all biconcave, the concavities at each end being distinct, and nearly equal. (Plate IX, figures 3 and 4.) The sacrum is elongated, and made up of a large number of coössified vertebræ. Whether the tail was elongated or not cannot at present be decided.

The jaws and teeth of this species show it to have been carnivorous, and it was probably aquatic. Its powerful wings indicate that it was capable of prolonged flight.

Another Cretaceous bird, (*Apatornis celer* Marsh,) belonging apparently to the same order as *Ichthyornis*, was found by the writer in 1872 in the same geological horizon in Kansas.* The remains preserved indicate an individual about the same size as *Ichthyornis dispar*, but of more slender proportions. The vertebræ are biconcave, and there were probably teeth.

The most interesting bird with teeth yet discovered is perhaps *Hesperornis regalis*, a gigantic diver, also from the Cretaceous of Kansas, and discovered by the writer in 1870. The type specimen, which was found by the writer in 1871, and described soon after, consisted mainly of vertebræ and the nearly complete posterior limbs, all in excellent preservation.†

A nearly perfect skeleton of this species was obtained in Western Kansas by Mr. T. H. Russell and the writer in November, 1872, during the explorations of the Yale College party, and several other less perfect specimens have since been secured, and are now in the Yale Museum. These various remains apparently all belong to one species.

The skull of *Hesperornis* has the same general form as that in *Colymbus torquatus* Brün., but there is a more prominent median crest between the orbits, and the beak is less pointed. The brain cavity was quite small. The maxillary bones are masive, and have throughout their length a deep inferior groove which was thickly set with sharp, pointed teeth. These teeth had no true sockets, but between their bases there are slight projections from the sides of the grooves. (Plate X, figure 2.) The teeth have pointed crowns, covered with enamel, and supported on stout fangs. (Plate X, figure 1*a*.) In form of crown and base, they most resemble the teeth of Mosasauroid reptiles. The method of replacement, also, was the same, as some of the teeth preserved have the crowns of the successional teeth implanted in cavities in their fangs. The maxillary grooves do not extend into the premaxillaries, and the latter do not appear to have supported teeth. The external appearance, moreover, of the premaxillaries seems to indicate that these bones were covered with a horny bill, as in modern birds.

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* This Journal, v, 74, Jan., 1873. † This Journal, iii, 360, May, 1872.

The lower jaws are long, and slender, and the rami were united in front only by cartilage. The dentary bone has a deep groove throughout its entire length, and in this, teeth were thickly planted, as in the jaws of *Ichthyosaurus.* The lower teeth are similar to those above, and all were more or less recurved. (Plate X, figure 1.) These grooves contain slight projections from the sides, but there are no true sockets. (Plate X, figure 2.)

The scapular arch of *Hesperornis* presents many features of interest. The sternum is thin and weak, and *entirely without a keel.* In front, it resembles the the sternum of *Apteryx*, but there are two very deep posterior emarginations, as in the Penguins. The scapula and coracoid are very small. The wing bones are diminutive, and the wings were rudimentary, and useless as organs of either flight or swimming.

The vertebræ in the cervical and dorsal regions are of the true ornithic type, the articular faces of the centra being quite as in modern birds. (Plate X, figures 3 and 4.) The sacrum is elongated, and resembles that in recent diving birds The last sacral vertebra is quite small. The caudal vertebræ, which are about twelve in number, are very peculiar, and indicate a structure not before seen in birds. The anterior caudals are short, with high neural spines and moderate tranverse processes. The middle and posterior caudals have very long and horizontally expanded tranverse processes, which restrict lateral motion, but clearly indicate that the tail was moved vertically, probably in diving. The last three or four caudal vertebræ are firmly coössified, forming a flat terminal mass, analogous to, but quite unlike, the "ploughshare" bone of modern birds. The anterior two at least of these caudals have expanded transverse processes.

The pelvic bones, although avian in type, are peculiar, and present some well marked reptilian features. A resemblance to the corresponding bones of the Cassowary is at once evident, especially in a side view, as the ilium, ischium, and pubis all have their posterior extremities separate. The two latter are slender, and also free back of their union with the ilium at the acetabulum. The ischium is spatulate at its distal end, and the pubis rodlike. The acetabulum differs from that in all known birds, in being closed internally by bone, except a foramen, that perforates the inner wall.

The femur is unusually short and stout, much flattened antero-posteriorly, and the shaft curved forward. It somewhat resembles in form the femur of *Colymbus torquatus* Brün., but the great trochanter is proportionally much less developed in a fore-and-aft direction, and the shaft is much more flattened. The tibia is straight and elongated. Its proximal end has a moderately developed cnemial process, with an obtuse apex. The epi-cnemial ridge is prominent, and continued distally about one-half the length of the shaft. The distal end of the tibia has on its anterior face no ossified supratendinal bridge, differing in this respect from nearly all known aquatic birds. The fibula is well developed, and resembles that of the Divers. The patella is large, as in *Podiceps*, and in position extends far above the elevated rotular process of the tibia.

The tarso-metatarsal bone is much compressed transversely, and resembles in its main features that of *Colymbus*. On its anterior face there is a deep groove between the third and fourth metatarsal elements, bounded on its outer margin by a prominent rounded ridge, which expands distally into the free articular end of the fourth metatarsal. This extremity projects far beyond the other two, and is double the size of either, thus showing a marked difference from any known recent or fossil bird. There is a shallow groove, also, between the second and third metatarsals. The second metatarsal is much shorter than the third or fourth, and its trochlear end resembles in shape and size that of the former. The existence of a hallux is indicated by an elongated oval indentation on the inner margin above the articular face of the second metatarsal. The free extremities of the metatarsals have the same oblique arrangement as in the *Colymbidæ*, to facilitate the forward stroke of the foot through the water. There are no canals or even grooves for tendons on the posterior face of the proximal end, as in the Divers and most other birds; but below this, there is broad, shallow depression, extending rather more than half way to the distal extremity.

The phalanges are shorter than in most swimming birds. Those of the large, external toe are very peculiar, although an approach to the same structure is seen in the genus *Podiceps*. On the outer, inferior margin, they are all deeply excavated. The first, second, and third have, at their distal ends, a single, oblique, articular face on the inner half of the extremity, and the outer portion is produced into an elongated, obtuse process, which fits into a corresponding cavity in the adjoining phalanx. This peculiar articulation prevents flexion except in one direction, and greatly increases the strength of the joints. The terminal phalanx of this toe was much compressed. The third, or middle, toe was greatly inferior to the fourth in size, and had slender, compressed phalanges, which correspond essentially in their main features with those of modern Divers.

The remains preserved of *Hesperornis regalis* show that this species was larger than any known aquatic bird. All the specimens discovered are in the Yale College Museum, and agree essentially in size, the length from the apex of the bill to the end of the toes being between five and six feet. The habits of this gigantic bird are clearly indicated in the skeleton, almost every part of which has now been found. The rudimentary wings prove that flight was impossible, while the powerful swimming legs and feet were peculiarly adapted to rapid motion through the water. The tail appears to have been much expanded horizontally, as in the Beaver, and doubtless was an efficient aid in diving, perhaps compensating in part for want of wings, which the Penguins use with so much effect in swimming under water. That *Hesperornis* was carnivorous is clearly proven by its teeth; and its food was probably fishes.

The zoological position of *Hesperornis* is evidently in the *Odontornithes;* but the insertion of the teeth in grooves, the absence of a keel on the sternum, and the wide difference in the vertebræ require that it be placed in a distinct order, which may be called *Odontolcæ*, in allusion to the position of the teeth in grooves.

The two orders of birds with teeth would then be distinguished as follows:—

Sub-Class, ODONTORNITHES (or AVES DENTATÆ).

A. Teeth in sockets. Vertebræ biconcave. Sternum with keel. Wings well developed.

Order, Ichthyornithes.

B. Teeth in grooves. Vertebræ as in recent birds. Sternum without keel. Wings rudimentary.

Order, Odontolcæ.

In comparing *Ichthyornis* and *Hesperornis*, it will be noticed that the combination of characters in each is very remarkable, and quite the reverse of what would naturally be expected. The former has teeth in distinct sockets, with biconcave vertebræ; while the latter has teeth in grooves, and yet vertebræ similar to those of modern birds. In point of size, and means of locomotion, the two present the most marked contrast. The fact that two birds, so entirely different, living together during the Cretaceous, should have been recovered in such perfect preservation, suggests what we may yet hope to learn of life in that period.

The geological horizon of all the *Odontornithes* now known is the Upper Cretaceous. The associated vertebrate fossils are mainly Mosasauroid reptiles and Pterodactyls.

A full description with plates of all the known *Odontornithes* is now being prepared by the writer.

Yale College, New Haven, Oct. 18th, 1875.

EXPLANATION OF PLATES.

Plate IX.—*Ichthyornis dispar* Marsh. Twice natural size.
Figure 1. Left lower jaw; side view.
Figure 2. Left lower jaw; top view.
Figure 3. Cervical vertebra; side view.
Figure 4. Same vertebra; front view.

Plate X.—*Hesperornis regalis* Marsh.
Figure 1. Left lower jaw: side view: half natural size.
Figure 1*a*. Tooth; four times natural size.
Figure 2. Left lower jaw; top view; half natural size.
Figure 3. Dorsal vertebra; side view; natural size.
Figure 4. Same vertebra; front view.

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[THIRD SERIES.]

ART. LII.—*On Southern New England during the melting of the Great Glacier;* by JAMES D. DANA. No. IV.*

IV. DEPRESSION OF THE LAND, OR AMOUNT OF SUBSEQUENT ELEVATION.

THE higher of the terraces of stratified drift, especially of those along the shores of estuaries and tidal inlets, or within the lower tidal part of the river valleys, are rightly regarded as evidence, *after making the necessary deductions*, of the amount of upward change of level which the region containing them has undergone. The following are some heights above high-water or flood level observed in Southern New England:

1. The terrace plain, or upper level of the stratified drift, in the northeast corner of the New Haven plain, near Whitneyville—50 feet; and in the northwest corner, at Westville—47 feet.
2. On the Housatonic River near Birmingham, ten miles from the Sound and nine miles west of New Haven—95 feet.
3. In the Connecticut valley, in the vicinity of Middletown, about thirty miles from the Sound and twenty-three miles northeast of New Haven—150 feet.
4. At the head of the fiord called the River Thames, at Norwich, sixteen miles from the Sound and nearly fifty miles north of east of New Haven—110 feet.
5. At the head of Narragansett Bay, about Providence, a mile above the mouth of Providence River, and about 100 miles north of east of New Haven—80 feet.

Here we have terraces at five places in Southern New England, thirty miles or less from the Sound, and within reach of the tides, having the following heights above flood-level: near New Haven, 47 to 50 feet; at Birmingham, 95 feet; near Middletown, 150 feet; at Norwich, 110 feet; and at Providence, 80 feet. What are *the necessary deductions* to be made from these numbers in order to obtain for each place the amount of that change of level (the depression of the land when the deposits were made, and the subsequent elevation) which led to the present elevated position of these plains of stratified drift, making them into river-valley and estuary terraces? If any elevated shell-bearing sea-beaches existed on the shores of the Sound, the question as to the amount of elevation since the Champlain period would be easily answered. But such evidence as already explained, is wanting; and wanting also outside of the limits of the Sound, even to Cape Cod.

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* For the preceding parts of this memoir see pp. 168, 280, 353, of this volume.

In order to make correct observations on this subject and arrive at right conclusions, several points have to be noted, a brief review of which is here given by way of introduction.

1. The height to be measured should be that of the highest terrace plain above the level of the highest modern flood-plain or lower flats. For the terrace-plain, where of full height, marks approximately the flood-level of the Champlain era, just as the flood-plain of a stream in these modern times marks the level nearly of modern floods. This point has been considered in obtaining the numbers for the heights above given, each being the height of the terrace-plain above flood-water level.

2. An allowance has to be made for the excess in height of the Glacial flood—that attending the melting—over the height of modern floods. If modern floods raise a stream 20 feet above low water mark, and hence make a flood-plain at that height, and a Glacial flood were so great as to produce a rise of 50 feet above the same low-water line, the flood-plain made from the depositions of such a Glacial flood, if at the water-level, would be 30 feet higher than that from ordinary floods: and when the flood subsided, the flood-plain so made would exist as an elevated terrace, 30 feet above the lower flats, although there had been no change of level whatever. The terrace here is evidence of the height of the flood, and not of any change of level in the land. Moreover, supposing in addition, an elevation to have taken place, 30 feet of the height of the Glacial flood-plain, if of the full normal height, would have to be credited to the excess in height of the Glacial flood.

Moreover, the deposits of those thirty feet may all have been made in *shallow* water, that is, each near flood-level, as the flood rose in height; so that the marks of shallow-water origin in the lower and overlying beds would not in this case be proof of a gradual subsidence as the deposition went forward.

3. A further allowance has often to be made on account of

local obstacles to the free discharge of the waters, giving a local height to the flood, much exceeding that usual along the valley. When a river valley is suddenly contracted, or when large tributaries join it at a given point, the stream may be carried in such places to an abnormal height—and so produce deposits up to that abnormal level—-because the water cannot escape as fast as it is supplied. Moreover, for the same reason, the depth of water in the receiving estuary, and in the Sound just off the mouth of the stream, are elements to be considered; since, if very shallow, the water, under a flood of supposable magnitude, might be raised in an estuary several yards above the true tide-level; and a few feet perhaps over shoals off its mouth in the Sound, notwithstanding the wide spreading of the water on either side.

4. To guard against error, the true limit between the *stratified* drift of a valley or estuary and the *unstratified* of the adjoining hills must be carefully determined, for only the former is the terrace or flood-made formation. To be sure of correctness on this point, it has to be considered that the *unstratified* drift has sometimes locally a degree of stratification of a tumultuous kind, due to the rush of waters from the melting glacier; and, again, the *stratified* drift may fail of distinct bedding. Hence in all cases a wide range of level terrace-plain—so wide that there can be no reasonable doubt that it was levelled under water, should be sought for, in order thereby to confirm the evidence from other sources as to the deposits being of under-water origin.

An important mark of such river-valley, estuary or sea-border formations consists in the total or nearly total absence of great bowlders from their level surface. The slopes of the hills above carry bowlders usually down to the level of the plain; then they cease suddenly, scarcely one existing over the plain that tops the water-made deposits. This may be observed along all the river valleys of New England, and also in many places on its shores. Between Watch Hill and Point Judith, the hills are thickly strewn with the huge blocks, down to within eleven feet of high water level; there, along a large part of that coast, wide meadows are spread out without a bowlder. But wherever the surface over the meadows swells upward above the eleven-foot level, it is peppered as thickly with bowlders as the slopes of the neighboring hills. Some bowlder-areas occur that are scarcely above it because the surface there was at the water's level when the bowlders were dropped. The ice, as melting went forward, let go first the great rocks that were in its bottom or lower part, and where there was water underneath instead of land they sunk and were spread, along with much coarse gravel, over the bottom, there to be buried beneath later depositions. The material of these later beds along the river valleys was brought down and deposited by the flooded river as the melting went forward and the glacier retreated; or they were carried into the valley from the hills either side of the valley—all the slopes under the deluge of waters sending down earth and stones, as well as the ravines and side-valleys, to be stratified and levelled off by the rushing stream.

It is not safe to infer that all New England hills with grassy bowlderless surfaces are parts of a valley terrace; for some regions afforded few bowlders to the ice; or only soft easily decomposable material; but when the hills rise to a common height and have a flat top, there can be little reason for doubt.

5. It is further to be noted that the terrace or terraces of a region may be only one or more of the *lower* terraces instead of the highest; that is, in other words, the *highest* normal to the region may be wanting, either (1) because there the flood-waters made no depositions up to flood-level, owing to its fierce rate of flow, or some other cause, but built up only to lower levels; or (2) because the highest part of the formation, after its deposition, was swept off and so reduced to a lower level by the flood when in its period of greatest violence, leaving only a lower terrace in its place, with perhaps traces of the higher, or often, not even these.

6. A careful study of the kind of *stratification* in the deposits is needed in order to determine whether the beds were deposited (1) under the action of the *incoming tide* on the sands and gravel from the dissolving glacier or drift-covered hills; or (2) under the action of the *outflowing river*. This study requires that the cuttings for all road-gradings, wells, sewers, and cellars, in the stratified drift of an estuary, should be carefully examined. When the facts as to structure are fully understood, the question whether *sea-made* or *river-made* may in this way receive a positive answer even along shores having no raised shell-bearing sea-beaches.

7. An examination should be made for stratified deposits of the *later* or "Alluvian" part of the Champlain period, and a comparison of the heights of such beds with that of the *early* Champlain beds or true "Diluvian" stratified drift; for such beds may show at what level the waters of an estuary or inlet stood after the Glacial flood had for the most part or wholly subsided.

The mere statement of these points is sufficient to evince that the problem before us—that of determining the amount of depression of Southern New England during the Champlain period, or, of elevation since then—is beset with difficulties. These difficulties I have not been able in all respects to surmount; and my present purpose is to state the facts which I have observed, and not to announce positive results. The observations needed to settle all doubtful points demand long-continued investigation in each estuary region of the coast, such a study as can be satisfactorily carried forward only by one who is living in the region and is thus always at hand to take advantage of every excavation that may be made in the deposits for cellars or other purposes. And to this should be added an accurate topographical and hydrographic survey of the estuary regions, and of the river valleys within thirty miles of the coast.

After these preliminary considerations I proceed to the discussion of the facts bearing on the level of Southern New England in the Champlain period, or, its equivalent, the amount of elevation which may be proved, from existing conditions, to have taken place since that period.

I. New Haven Region.

The evidence with regard to the amount of depression over the New Haven region which has been obtained is from the following sources:

1. From the height of the upper or highest terrace of a region above mean high tide.
2. From the depth of the valley-excavations along the rivers and estuaries, or the height of the upper terrace above the flood-plain of the present streams.
3. From the structure of the beds.
4. From the existence of "Alluvian" deposits.

1. *Height of the terrace-plain or stratified-drift formations above mean high tide.*—This terrace-plain in the New Haven region is the wide plain upon which the city is built. (The map of the region is here reproduced, in order that the relations of the localities referred to beyond may be appreciated). It extends *southward*, by the west side of the New Haven Bay, through West Haven to the Sound east of Savin Rock—a rocky point terminating the first range of hills on the west. It is also continued northward through Hamden to Mount Carmel; but it gradually diminishes in width in this direction and at last is confined to the vicinity of Mill River. The distance from Savin Rock to Mount Carmel in an air-line is eleven and a half miles, and it is one sloping plain. From the upper part of the city (about four miles north of Savin Rock), where the land is 45 to 47 feet above mean high tide, there is a gradual slope seaward (that is, southward), toward the West Haven coast, of 10 to 11 feet a mile; and there is also the same gradual rise northward toward Mount Carmel. No trace of any beach-like break is to be found across the surface; no line of sand hills or ridges; nothing whatever to make one part more than another to look like the course of a beach or water-line.

This is well seen, for the lower part of the plain, when standing on a bridge in the West River valley: an unbroken terrace makes the sides of the river valley from Westville, where the height is 47 feet above mean high tide, to the mouth of the stream, where, opposite Oyster Point, it is 20 feet; and going on toward the West Haven beach, the plain continues its slope, terminating with a height of 5 feet at the beach, or 8 feet just back near the western hills. And for the middle and upper part of the plain it is apparent along the road (Dixwell avenue) passing over the great plain from New Haven northward into Hamden, where the surface over a very wide area is all open to view so that any abrupt change of level or variation in the surface would be easily detected if such existed. There are hence on the surface of the plain no marks of a shore-line of the Champlain period.

Again, no sea-shore or beach deposit is found anywhere about the lower slopes of the hills above the level of the plain. The passage is every where abrupt from the even plain to the rising hill-side, that is, from the terrace of stratified drift to the unstratified drift of higher levels, and this is true quite to Savin Rock on the Sound. On the east side of this bay the same is true; but the terrace on that side is mostly wanting, and the cape is rocky, so that the fact is less significant.

The following are the elevations of the plain at different points between the Sound and Mt. Carmel village.

					Feet.
Near Savin Rock beach, on the Sound,					5
0·5	mile	north of	Sound,	highest near foot of hills,	10–14
1·5	miles	“	“	S.W. of bridge over West R.,	18
2	“	“	“	c. Kimberly and Greenwich sts.,	23
2·5	“	“	“	on Putnam st.,	31
2·9	“	“	“	c. of Congress av. and Cedar st.,	35
3·5	“	“	“	c. of Elm and York sts.,	42
4	“	“	“	c. of Dixwell av. and Henry st.,	49
4·5	“	“	“	c. of Dixwell av. and Division st.,	52
5·3	“	“	“	c. of Dixwell av. and Dudley st.,	61
6	“	“	“	Dixwell, near house of H. Munson,	68
6·5	“	“	“	Hamden church on Dixwell av.,	76
7·25	“	“	“	Dixwell av., near J. E. Bassett's,	82
9·5	“	“	“	N. Centreville, at meeting of roads,	103
11·5	“	“	“	near Mt. Carmel village,	115

An even slope is not to be expected since (1) the shore flats or flood-grounds of a river or lake are never even; and because (2) terraces have often lost much of their height by denudation, or gained or lost in height through drifting sands.*

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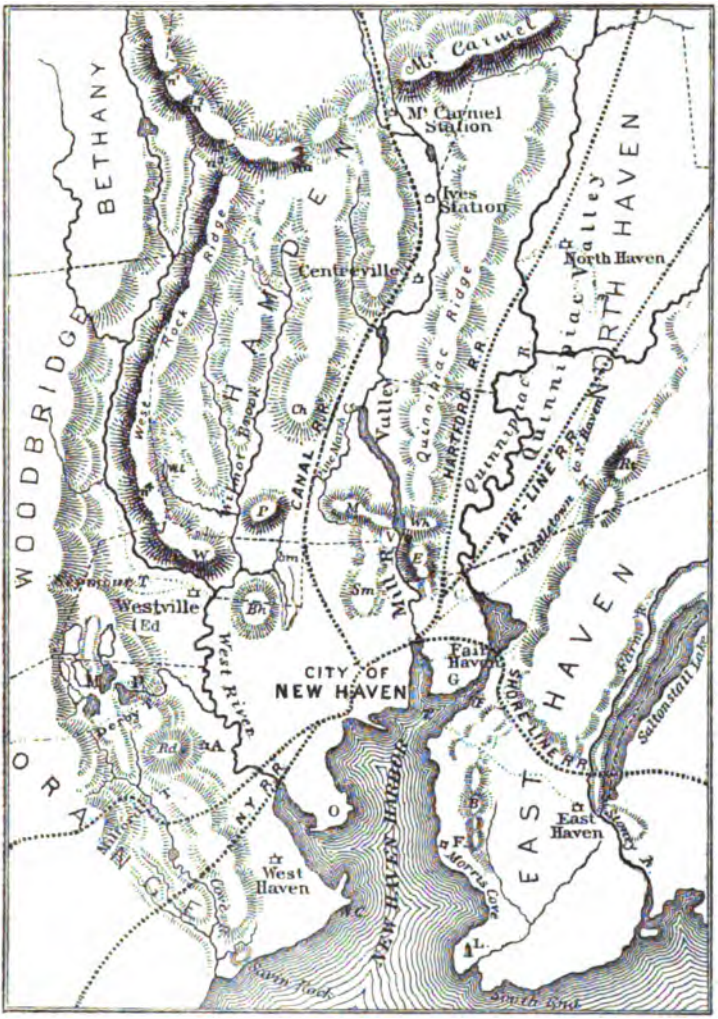
* I am indebted to Mr. C. E. Fowler, New Haven City Surveyor, for the heights of the various parts of the plain along the streets within the limits of the city; and to levelings by Mr. G. B. Chittenden and D. H. Pierpont, carried on under the direction of Mr. R. M. Bache of the Coast Survey, for heights through Hamden to Mt. Carmel.

MAP OF THE NEW HAVEN REGION.



Explanations of the map.—A, Allingtown village. B, Beacon Hill. Bh. Beaver Hills. Ch, Cherry Hill. C, Air Line Railroad cut, near south end of East Rock. E, East Rock range, consisting of East Rock proper to the northwest, Indian Head, and then Snake Rock. Ed, Edgewood, the estate of Donald G. Mitchell, Esq. F, Fort Hale. *F*, Ferry Point, or Red Rock, on the Quinnipiac. J, Judges' Cave, on the West Rock ridge. L, Light House. M, Mill Rock. M P, Maltby Park, only three of the proposed lakes of which are constructed. O, Oyster Point. P, Pine Rock. Rd, Round Hill. Rt, Rabbit or Peter's Rock. Sm, Sachem's ridge. T, Turnpike; also Tomlinson's bridge, across the head of New Haven bay. V, Whitneyville. W, West Rock, the south end of the West Rock ridge. W C, West Cape, or West Haven Point. Wh, Whitney Peak. W L, Wintergreen Lake, just north of Wintergreen Falls. Wn, Warner's Rock. *bm*, Beaver Pond Meadows; *m*, Mineral Spring, southeast of North Haven; *n*1, *n*2, *n*3, *n*4, different notches in the West Rock ridge; *n*1, *n*2, the upper and lower Bethany Notches; *n*3, the Hamden Notch; *n*4, the Wintergreen Notch.

Scale 4-10ths of an inch to the mile.

The part of the plain stretching northward through Hamden to Mount Carmel village, reaching there a height of about 115 feet above the sea, owes its slope to the pitch of the flooded stream, which there flowed over a sandstone region with no deep channel to confine its waters and direct their course. That it was not due to marine submergence to a depth of 115 feet is proved by the facts in the adjoining Quinnipiac valley, a mile and a half to the eastward. For at North Haven, six miles north of New Haven, the stratified drift or terrace has a height above flood level not exceeding 45 feet; and this is but two feet above its level five miles to the south, just north of west of New Haven city; facts which show that there was a nearly level surface over the great Quinnipiac basin or harbor between these distant points, and that the greatest height of the marine submergence, if there was one, could not have exceeded 45 feet.

If then the upper part of the sloping plain from Mount Carmel southward was made by fresh waters from the melting glacier, shall we conclude that this was true also of the lower part, nearly to the West Haven shore? The slope of this lower part is no greater than that more to the northward; and hence there is little objection to the conclusion from this source. The bay is small and throughout shallow, being but 3 to 18 feet deep at low tide, and little of it over 12 feet; so that it did not afford a capacious channel for the escape of the flood waters.

2. *Height of the terrace-plain above flood-level in the estuary and river-valleys.*—About the New Haven bay, the plain, as has been stated, rises gradually to the northward. It reaches its greatest height above the lower flats or flood-plain of the streams near where they leave their valleys to pass through the plain.

A. *West River.*–The greatest depth of the cut along West River, or greatest height of the terrace-plain above the lower flats, exists near Westville, where it is 45 to 47 feet. Going up West River this height above flood-level diminishes to 33 feet in the course of a mile, and to 6 to 8 feet in the next half a mile; but this is due to the rapid descent in the stream, which is 22½ feet in the first mile, and 50 feet in the following half a mile, 72½ feet in all.

B. *Mill River.*—Along Mill River, the height of the terrace for the first mile and a quarter above the head of New Haven bay, that is, through the New Haven plain, is ten feet or so below the height normal to that part of the plain; and this was a consequence of the denudation by the Glacial flood, as before explained.* But at Whitneyville, near the dam, the height—or the depth of the cut—is 50 feet; and this continues to be the

* This volume, page 177.

depth up stream for the next mile and a half; and for the next three-fourths of a mile, or up to Augurville, the height is 45 feet. Toward Mt. Carmel village the height diminishes to 36 feet above flood level; and then in the next half mile above to 25 feet.*

C. *Quinnipiac River.*—The terrace plain along the Quinnipiac near where it leaves its valley, or at the Air Line Railroad cut, is 43 feet. North of this lie the wide marshy flats, which continue for five miles to North Haven; and at the latter place the height of the terrace on the west of the river is but 45 feet; the waters having stood nearly on a level between the northern and southern limits of this great interior basin.

These are the facts from the river-valleys. Along each the cut made by the denuding waters since the Glacial flood is not far from 45 feet. This depth of cut is just what would at the present time result from the denuding action of the rivers, if the land were to be raised 45 feet or so—supposing there were no rocky ledges in the beds of the streams to prevent excavation downward.

On the other hand, if the Glacial flood were vast enough to raise the waters to that level and so to build up (out of the sands and gravel within its disposal) the drift formation there existing, without any aid from an elevation of the land, or of one of only 5 to 10 feet, then, when the flood had passed, the streams would have sunk their beds to their present level, and so have left the terrace-plain, where not more or less washed away, at its present height above them.

Thus either hypothesis, (1) that of an elevation of 45 feet since the Champlain period, or (2) that of the accumulation of stratified drift in the valleys to its present height by the Glacial flood, with little or no subsequent elevation of the land, will explain the facts. If the former were the actual condition, then the waters would have continued their work, about the estuary or inlet, of building Champlain deposits up to, or toward the 45 foot level, through all the Champlain period, or until the elevation of the land closing the period began. If the latter, the sinking of the rivers as the flood subsided would have lowered their working ground down to the existing level of those plains; and hence, little or no estuary deposits of the later or "Alluvian" part of the Champlain period, beyond a height of 5 to 10 feet above high tide level, should be looked for.

For further evidence we proceed now to facts from the *structure* of the beds of stratified drift, and from the occurrence of deposits of the later or "Alluvian" part of the Champlain period.

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* This diminution in the depth of the cut, or in the height of the terrace-plain, to only 15 feet, is owing to a trap ledge which crosses the bed of the stream in a line with the trap ridge at Mt. Carmel; this ledge prevented excavation.

3. *Structure of the beds formed.*—Throughout the stratified drift-deposits of the New Haven region for four and a half miles north of Savin Rock on the West Haven coast, the finer beds of the stratified drift are characterized by the flow-and-plunge structure; and the oblique lamination in it—certain river valley regions excepted—*rises to the northward* (that is, dips to the southward). This *rising to the northward* I have attributed, in a former part of this memoir, to movement from the southward in the waters, that is, to plunging waves accompanying the *inflowing* tides—supposing that ordinary inflowing tidal waters may push up the sands of the bottom before them and so make ordinary obliquely-laminated layers dipping seaward or rising landward: and that, by the addition of plunging waves, the flow-and-plunge structure produced would also have the layers dipping seaward. That in such a case the dip of the little layer would be seaward I inferred, perhaps without due consideration, from the direction of movement in the water, and from observations long since made by me on the sandstones in the vicinity of Sydney, New South Wales.* If, as assumed in Part I of this memoir, the above conclusion is correct, the seaward dip of the oblique laminæ beneath the New Haven plain, up to a height of at least 45 feet, is proof that the sea was the agent, and that therefore the land at the time was at least 45 feet below its present level.†

Further, the deposits through the long Air-Line Railroad cut at the mouth of the Quinnipiac valley, have, as described on page 175 of this volume, the oblique lamination in the upper stratum of 20 feet *reversed;* that is, dipping landward instead of seaward; and this difference in the lower and upper strata naturally suggests that there was a change in the direction of movement of the depositing waters; a change from that of the inflowing tide to that of the outflowing flood, as stated on page 176; or, if the lower stratum was made by the flood, from the outflowing flood to the inflowing tide. Either way we reach the conclusion that, at some time in the early Champlain period, if not through all of it, the land was at least 45 feet below its present level, and that a rise to this extent has since taken place.

Still certain facts dispose me now to question the inferences that oblique lamination made by the inflowing tide would have the dip in general seaward, and that the reversion of the dip in the upper twenty-foot stratum on the Quinnipiac has no other explanation than a change in the direction of the current. Among them, I here mention only that I have observed several cases along river-valleys, out of the reach of the sea, of dip in both directions; and, in some of them, obliquely laminated layers dipping down stream had a thickness of several feet. Such facts bear against the view.*

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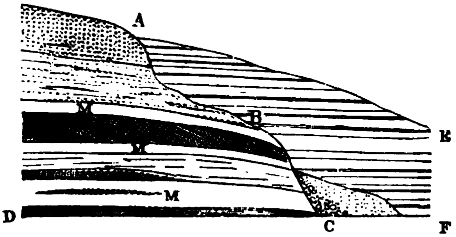
* See the writer's Exploring Expedition Report, 4to, 1849, pp. 465, 523. The observations were made in 1839.

† Owing to the want of good sections I have been unable to study the character of the lamination over the Hamden Plains, where the height of the surface rises beyond 45 feet.

4. *Alluvian Deposits*—Another fact favorable seemingly to the conclusion that the land remained 40 feet or more below its present level for some time after the Glacial flood had passed, is afforded by the existence of the deposit of horizontally-bedded white sands against the northward slope of the terrace of stratified drift at the northeast extremity of the Air Line Railroad cut, near south of East Rock. The figure is here reproduced that the relation of the deposit to the stratified drift may be appreciated. The conclusion, explained on p. 180, that the white sands were washed and accumulated on the south shore of a great "Interior basin" occupying the Quinnipiac valley, appears to be the only reasonable one; and if so, this "Interior basin" continued to exist for a while after the flood and flood-depositions had reached their height, as an area of high water. Yet, as the bed is of very limited extent and the terrace formation is generally free from Alluvian deposits, the time may have been only that of the latter part of the flood, prolonged after flood-deposition had ceased by the melting still going on to the north; and the region of melting may have been north of the head of the New Haven streams, since it is possible, as I show beyond, that the flood waters of the Connecticut and Farmington rivers overflowed into the New Haven region and bay.

1.



Section of stratified drift, A C D, and later unconformable Champlain deposits, A C F E, Air Line Railroad cut.

5. *Conclusion.*—While the height of the terrace-plain above the sea and its height above the flood-plain of the river, fail of demonstrating that the New Haven region was below its pres-

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* The gentle pushing of the waters over sands makes ripple-marks, or elevations having a seaward and a landward slope. The query now is whether, in the heavy pushing producing the oblique lamination, the laminæ would conform to the former or the latter of these slopes.

ent level in the Champlain period, the evidence from the structure of the beds and from the Alluvian deposits suggests that the region was submerged to a depth of at least 45 feet.

But if so deeply submerged, why should the terrace plain slope off gradually toward the coast and have no line of old beaches across it? And why is there no 45-foot sea-shore formation or terrace along some part of the bases of the hills?

The ways in which such a slope in the plain may be supposed to have resulted are the following:—(1) By a subsidence causing a bending downward of the coast-region after the elevation of 45 feet had taken place.—(2) By an elevation of the land which should be 45 feet four miles from the coast, but 10 to 12 feet near the coast.—(3) By the sweeping action of the flood giving a slope to the deposits toward the Sound and beneath its waters, and preventing the formation of seashore flats at the water's level.—(4) By the denuding action of the waves and tidal currents as the land rose to its present level.—(5) By the deposits, because of a diminution in the amount of material southward or increase in the depth of the waters, conforming in slope to the pitch of the surface on which they were spread out.

The first and second of these ways have little probability in their favor. The others I make the chief source of the result in my memoir on the New Haven region, saying (p. 99) after remarks on each method: "That part of the rapidity of slope in the lower portion of the plain below 40 feet in height is due to tidal, wave and river action over the region of the bay: and part to increasing depth over the borders of the bay southward and a decrease southward in the amount of transported sand." How far these methods are sufficient may be better considered after a survey of the facts from other parts of Southern New England.

2. THE LOWER PART OF THE HOUSATONIC VALLEY.

On page 409 I have stated that the height of the terrace of stratified drift in the Housatonic valley near Birmingham is 95 feet above high-water level. The terrace has the same height at Birmingham, and at the manufacturing village of Shelton on the opposite side of the river.* These places are situated at the junction of the Naugatuck river with the Housatonic. The Housatonic river comes down from the northwest, and the Naugatuck from the north: and on the point of land between lies Birmingham, with Shelton opposite on the west (or southwest) bank of the Housatonic, and the village of Derby on the east bank of the Naugatuck. The streams rise in the high regions of Western Massachusetts and Connecticut, in part over 1000 feet above the sea, and flow in rapids through much of their course down to their confluence, where they reach tide-level, ten miles from the Sound.

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* For this altitude I am indebted to measurements by Mr. D. S. Brinsmade, of Birmingham, surveyor. The leveling was made at Shelton, and gave for the average height of the plain above low-water 106·64 feet. Mr. Brinsmade states that some points were five to eight feet above this. I have taken as the maximum height of the terrace 112 feet. The height of flood-level above low water seldom exceeds 17 feet; and this subtracted from 112 gives 95 feet for the height above flood-level. A higher flood, Mr. Brinsmade says, occurred in 1857, when the river rose to 22 feet 3 inches.

As Birmingham is but nine miles to the westward of New Haven, it is improbable that the amount of elevation which the two places have undergone since the Champlain or Fluvial period should differ much; and hence there must be a special cause for the great height of the Housatonic terrace. For this cause we have only to look to the topographical conditions of the region.

Besides the fact of (1) the junction there of two rapid streams, whose united drainage area is about 1800 square miles, there are the following additional facts bearing on the subject:

(2) Both streams at that place have narrow terrace regions, the valleys between the hills having little width.—(3) On the east side of the Naugatuck the high bordering hills leave almost no space at their foot for the one-street village of Derby, so that the flooded waters cannot spread in that direction, and are naturally forced over against Birmingham and Shelton.—(4) The hills that lie back of Shelton, embaying its terrace region, reach to the river just below the village, and so do those on the Derby side, making thus, below Derby, a comparatively confined passage for the united waters, which has long been called the "Derby Narrows."—(5) The river channel below Derby, through its ten miles to the Sound is very shallow, being navigable for vessels drawing four feet of water only at high tide; and besides there are sandbanks at its mouth reaching a mile and a half beyond.—(6) The terrace along the valley below the Narrows is narrow for nearly seven miles out of the ten, the width in no part exceeding two-thirds of a mile.

Under such circumstances it is natural that the waters of the Glacial flood plunging down the two valleys should have entered the region of Birmingham and Shelton faster than they could have escaped. The fact that a cataract of great volume passed the region of Birmingham and dashed violently against the Shelton shore is abundantly manifested by the multitudes of large water-worn rocks and thick coarse cobble-stone deposits in the upper portion of the lower as well as high upper terrace, and in the imperfect stratification of portions of the deposits.

Even modern floods in the region bring down waters faster than they are discharged through the Derby Narrows and the channel beyond. The Housatonic river below the confluence river to the Sound. The heights, on the west side of the Connecticut, going *southward*, are as follows:*

2	m. below the Narrows,	the Maromas plain	133½	ft.
5	m. " "	near Higganum	100–105	"
7½	m. " "	near Haddam station	82– 85	"
10½	m. " "	near Goodspeed's station	75	"
14	m. " "	near Chester station	60– 62	"
20	m. " "	1½ m. below Essex	42– 45	"
22½	m. " "	near ferry and railroad	30– 31	"
24½	m. " "	Saybrook Plain	20– 21	"
26	m. " "	Seashore House, on Sound	13½	"

The plains, the heights of which are here given, were well displayed, leaving no doubt that they corresponded to the true terrace; and they were the highest in each region. They show that, from the Narrows, the river waters plunged along with a pitch of several feet a mile.

It may seem almost incredible that waters so violent should have made terraces in many places along the sides of the narrow valley. But the material of these terraces—mainly sand and coarse gravel, and the coarser above as usual—was to a large extent washed in from either side by the waters descending the side valleys, ravines, and all sloping surfaces; for there was gravel and sand everywhere over the hills from the dissolving glacier, and water everywhere in profusion for the work of transportation. The waters merely stratified and leveled off at top the material thus contributed. Moreover, we can follow the rise of the flood in the succession of the deposits; in clays, in some places, near the bottom—one bed supplying a brick-yard just below the Narrows—made when the waters were yet low; the sands next deposited while the waters were rising; the fine and coarse gravel mostly above—at the top of the lower, as well as of the highest terraces—spread out when the flood was at its height. The Middletown region is mostly one of fine earth, and the sand and gravel of the terraces below the Narrows could not have been carried over it by the stream, without making large depositions,—except in ice-floes, and these certainly did little of the transportation in the case here referred to.

Finally, how far was the height of the terraces described due to an elevation of the land since the beds were deposited?

Judging from the seashore terrace, on Saybrook point, the elevation of the land did not exceed 15 feet. I have not succeeded in finding other evidence on this point.

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* The height of the Maromas plain was determined by levelling, by Messrs. Pillsbury and Clarke, of the Wesleyan University. They obtained 133½ feet above flood-level. The extended terrace on the opposite (east) side of the river has about the same height. The other measurements were made by the author.

4. THE RIVER THAMES.

The conditions of the stratified drift deposits of Norwich are similar in several respects to those about Birmingham. Two rivers from the northward, the Quinebaug and the Yantic, pass either side of the city and unite below it to make the Thames. They rise in Massachusetts, and have nearly the same drainage-area as the two that join at Birmingham. They descend from high land and most of the way in rapids. Immediately below Norwich the valley is so narrowed by hills over 150 feet high that the width of the river, were the water 100 feet above its present level, would be hardly half a mile. But below these Narrows the valley expands, though still closely bounded by high land. The points in which the two cases differ are these:

(1.) The Narrows have greater width, and the modern stream below has much greater depth as well as breadth: the channel being one of the tidal, navigable fiords of the coast, the tides at Norwich having a range of more than three feet. Consequently it offered the flood-waters a less contracted way to the Sound.

(2.) The distance from the Narrows to the Sound is greater, being 14½ miles.

(3.) Although this distance is so much greater, the highest spring floods cause a rise above high tide of but 7½ feet—much less than half that in the Housatonic at Birmingham.

Again, (4,) the terrace plain of upper Norwich—the site of Broadway and its many fine residences—has over its northern part (at the foot of the little triangular Green) a height above tide level of 117 feet;* and, since the spring floods raise the water but 7½ feet above high tide, about 110 feet above flood level, against 95 feet at Birmingham.

The height of the Norwich plain above flood level diminishes from 110 in its northern part to 101 feet in its southern, a distance of about a mile; and this is very nearly the level of the terrace to the eastward of the latter at the Old Cemetery. The Norwich plain is so extensive, and the deposits so clearly consist of bedded sands and gravel, with the flow-and-plunge structure in some places quite to the top, that there is no occasion to doubt the statement that the water once stood at its high level. And since the deposits—those beneath the Old Cemetery, for example,—are made up in places of the coarsest gravel, the waters were the hurrying waters of a great torrent.

That this was the actual condition is proved further by the heights and nature of the terraces below Norwich. For they declined southward, precisely as in the Housatonic and Connecticut River valleys. The heights are as follows:*

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* I have these numbers from the Water Commissioner of the city of Norwich, Mr. Winship. I have been much aided in the study of this region by a map of the Thames, giving contour lines for 50, 100 and 200 feet, which was furnished me by the Superintendent of the Coast Survey.

Highest part of Norwich Plain,			110	feet.
1 m.	South,	on Norwich Plain,	101	"
2½ m.	"	at Thamesville, below the Narrows,	88	"
5½ m.	"	near Mohegan R. R. Station,	75	"
8 m.	"	½ m. W. of Montville R. R. Station,	61	"
10½ m.	"	in Smith's cove, 250 yds. W. of R. R.,	50	"
13½ m.	"	at New London, northeastern part,	24–25	"
14 to 16 m.	"	between New London and the Lighthouse, none distinct.		

Below the Narrows the valley opens widely on the west about Thamesville, and consequently has there a broad terrace. The same is true also of the region and terrace of Montville where a lateral valley comes in. The material of the upper part of the formation on the river near the Montville railroad station is very coarse, some layers being made up largely of cobble stones. On the point just north of Smith's Cove, the terrace is only 25 to 27 feet high; but it shows that it is below the normal height by its stony character; and, further, just around the point, 250 yards inside of the Cove, there is a much more extended terrace at a height of 50 feet, made of finer material.

The terrace in the northeast part of the city of New London (near where the track of the Northern Railroad leaves the city) is also very stony, and must be below its normal height, and probably as much as ten feet.

In passing the Narrows to Thamesville the decline of the terrace plain is 13 feet; and below Thamesville to Smith's Cove, on an average nearly five feet per mile. This last pitch if continued to New London would make the normal height there 35 feet instead of 25 feet. The terrace is equally low on the east side of the Thames in Groton; but it is also very narrow there, little room existing at the base of the hills for a terrace.

South of New London, between it and the Sound, I saw no distinct terrace.

The decline southward in the height of the terrace plain, as in the case of that along the other rivers, does not suggest marine action as the means of deposition. It manifestly points to the pitch of flowing waters in the channel, and to such a flow as could exist only in a period of incredible floods—when an outlet averaging over half a mile in width was not wide enough to discharge the waters coming from the hills and valleys within a range of forty miles.

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* For the height of the plain near the Montville Station, I am indebted to a levelling by Mr. C. P. Jennings, Surveyor of the city of New London.

Direct evidence as to the actual level of the coast about the mouth of the Thames during the era is wanting; both because of the absence of a seashore terrace, and because no sections were exposed along the Thames that afforded a chance for studying satisfactorily the character of the bedding. But if the New London Lighthouse Point is without terraces, I have seen several examples of them at the head of broad bays between New London and Stonington, and in no case have I found the height over 15 feet, and generally it is but 10 or 12 feet. Again, Fisher's Island—about seven miles in length—lies off this same coast, and within five miles of it; and at the head of West Harbor, there is a well defined shore terrace of 15 feet; and, at East Harbor, one of less certain nature nearly 25 feet. 25 feet is therefore the greatest height inferable from this kind of evidence, and 15 may meet the facts.

5. NARRAGANSETT BAY.

Providence is situated on Providence River within a mile of the head of Narragansett Bay. Its terrace, averaging 80 feet in height above high water, is extensive, and part of the city is built upon it. The river opens into the broad northern arm of the Bay, and affords 14 feet of water for shipping to the city. The Bay has passages one to three miles in width among its islands, and enters the Sound about 26 miles south of Providence. With so open a passage for the waters we might infer that certainly the Providence terrace must mark a sea level of the Champlain period. But in view of the facts detailed in the preceding pages it is evident that something of this height is attributable to the flood descending the river valleys.

I have not been able to study carefully all the shores of the great bay with reference to its terraces. Ten and a half miles south of Providence, east of East Greenwich, there is a very wide terrace-plain, which extends south toward Wickford. Near the railroad in East Greenwich, the height is 56 feet, showing a loss of 24 feet of elevation in the 10½ miles from Providence. At Wickford, 7 miles farther south, the height is much less, little exceeding 30 feet.

There is a terrace at Fall River, on the west shore, about 17 miles from the Sound, and between this place and Tiverton. The height, just below the depot at Fall River, is 35 feet above high water; but the beds are very stony toward the top, and hence it is that the terrace is 30 feet below the normal height; some of the stones are a foot in diameter. The terrace south of Fall River has no greater height. About Newport there appeared to be no well defined terrace.

But direct evidence bearing on the height of the region in the Champlain period is afforded by the coast region between

Point Judith (the west cape of Narragansett Bay) and W
Hill (the cape south of Stonington). Along much of
shore, as stated on page 411, there are wide flat meadow
tween the hills and the sea, having a height of about 11
above high tide. The road passes between these flat mea
on the south, and the bowlder-covered hills on the north,
the contrast is very striking: the former sandy or gravelly
thinly grassy among the great bowlders, the latter havin
even dense turf over a rich black soil. Above the level c
meadows no trace exists of sea-shore flats or beaches;
stony hill-sides rise gradually, with nothing about them to
gest a Champlain submergence.

Off this shore, one to two miles, there is a sandy bar
rising into hills to the westward, and it may be questi
therefore, whether the meadows may not owe a part of
height to the flood waters. But the black soil appears
evidence that they long lay with the surface at the sea-
perhaps as a salt marsh of the Champlain period. Large
tions are thickly strewn with broken oyster shells, left b
Indians, and this may be one source of the fertility.

No marine relics have yet been found in Champlain de
about any part of Narragansett Bay to mark the sea-level.
fossils should be looked for with more care than has hi
been used; but much looking will probably end in finding
The great glacier must have filled the channels amon
islands; and as the ice disappeared, the floods, having a s
pitch owing to the height at Providence, would have m
profound sweep through them. Absence of marine foss
therefore what should reasonably be expected.

An 80-foot terrace at Providence, a 56-foot terrace at
Greenwich, and an 11-foot plain on the sea-coast with no
of any other terrace-level on the coast hills, are the po
facts gathered from the vicinity of Narragansett Bay.

6. PRE-GLACIAL SAND-HILLS.

To define more clearly what are true deposits of str
drift of the era of the melting in Southern New Engl
add a few remarks on certain sea-shore sand-hills, that are
mistaken for drift formations. I refer to ridges and h
stratified material along the shores between Watch Hil
Point Judith.

It has long been known that Martha's Vineyard c
largely of Tertiary sands interstratified with clays, unc
dated—except in some places through limonitic depo
making a limonitic or iron conglomerate. Block Island,
Island, and Fisher's Island, just off the New England
are also made up to a great extent of such unconsol

pre-glacial beds,—as I shall more particularly explain in another memoir.

The New England coast region, from Watch Hill eastward, is but a continuation of Fisher's Island, in its features, and also, as there is reason to believe, in its Tertiary deposits. Its hills or ridges are of various heights, up to 180 feet; and nothing besides unconsolidated, though bedded, sand, gravel and clay, occurs in their constitution. Clay-beds are not in sight. But near the Ocean House, one of the large Watch Hill hotels, a pond, near the sea-level, is called Clay pond, because of the clay beneath the water; and I am informed by a resident in the region, that masses of clay are sometimes thrown up by the sea on the beach, showing that it exists at the base of the sand hills.

Over the stratified material of the hills lies the unstratified drift, with multitudes of large bowlders .

These hills show that they do not consist of Champlain or later deposits by the following characteristics.

In the first place, the hills are, as stated above, covered throughout with bowlders, down to within 10 or 12 feet of the sea level, and this demonstrates that the hills were there before the deposition of the bowlders and the associated gravel and sand.

Further, the features as to the bowlders and the hills are just those of Fisher's Island, which lies in their line at a distance of only three miles to the west;—and not that distance, since there are intermediate islets and reefs connecting the two; and on Fisher's island the clay beds and sand beds which underlie the top-dressing of unstratified bowlder drift are in some places upturned and folded—proving thus their anterior origin.

We may hence set aside those sea-border ridges as not of the Champlain period, and regard them as prior in elevation even to the Glacial period. Only the low grassy plain at their foot along the shores is of the Champlain water-arranged drift formation.

CONCLUSIONS.

The observations described in the preceding pages relate only to five of the river valleys of Southern New England. Although I have made no systematic measurements of terrace-heights in other valleys, I have seen enough in many of them to assure myself that all have the same class of facts to afford. The conclusions which are here reached with regard to the river floods are therefore conclusions for all Southern New England, and beyond this, I believe, for all New England, and other regions covered by the great glacier.

depression in Southern New England during the melting of the glacier.

The evidence that some elevation took place in Southern New England after the Champlain period, based on the existence of elevated flats along its sea-border, we may regard as conclusive. The height of the marine terraces where not modified by fluvial action varies between 5 feet and 25 feet, and rarely exceeds 15 feet. Taken alone, this affords no ground for believing that the elevation was less than 10 feet or greater than 25 feet. I think that 15 feet is the most reasonable inference, for the portion of the coast considered in the preceding pages.

The evidence derived from the structure of the bedding in the New Haven region—that is, the direction of the dip in the flow-and-plunge portion (p. 418) appears to be good; but since it is difficult to believe that the coast region of the Champlain period should have had no flats at the water level, to be placed, by an elevation of 40 feet, at a height of 40 feet, it is hard, in the absence of such terraces, to set aside all doubt with regard to that evidence. Hence, although at present unable otherwise to explain those facts, I am led to hold the conclusion in abeyance. This doubt is in opposition to my former statements. But those statements were based mainly on a study of the New Haven region, and the few facts from Connecticut previously on record. A decline in the height of the terrace-plain toward the Sound down to only 5 feet of elevation at the coast, along an estuary receiving three streams swollen by glacial floods, seemed to be accounted for satisfactorily by the sweeping action of the floods, and the erosion by waves during the progress of an emergence. But now that we know that such a seaward pitch in the terrace plains exists along all the valleys of Southern New England, and that the sea-coast terrace is universally but 5 to 20 feet in height, those causes appear to be inadequate. It is difficult to believe that they could have prevented so completely the existence of shore plains above the low height mentioned, through the whole line of coast, even (1) where no rivers sent down their floods, and (2) where distant barriers protected the shores from the heavier seas. Were the land 40 or 50 feet below its present level, as hitherto supposed, some of the islands off the coast would still have been a shelter from the heavier waves; and many of existing shore-hills would have been islands, protecting an inner region wholly from violent seas; so that deposits at or near a 40-foot or 50-foot level might well have been formed. And yet none such exist.

In the present state of the facts I therefore think that the argument for an elevation of only 10 to 20 feet is the strongest, although not yet decisive.

2. *The river-valley formations not marine.*—The ocean took no part in the formation of the river terraces. The pitch in the terrace plains of the lower Housatonic, lower Connecticut, and the Thames, is alone sufficient evidence against marine action in the matter.

3. *The height of the flood the chief cause of the height of the terraces.*—The facts from the valleys of the Housatonic, Connecticut and Thames prove that the river-valley formations—which now stand at so great a height above the river's surface—were not wholly, or for the greater part of their height, formed *when the land was at a much lower level than now;* but, on the contrary, that they were formed *when the river's waters were at a greatly higher level than now*, and as a consequence, chiefly of the glacial flood. If the whole elevation of the land since they were made is but 15 feet, then the rest of the elevation of the high terraces was owing to the height of the flood.

The facts hence teach that we may have high terraces along valleys, and many terraces, and yet none be due to an elevation of the land. Further, the height of the lower terraces of stratified drift is *comparatively* of little geological interest. For, in New England, they cannot be proved to have had anything to do with successive stages of elevations; and, in general, they were underwater flats of different levels; or they originated as levels up to which the flood, according to its rate of flow, built the stratified material, or down to which they swept off that which had before been laid down; or they mark the level of oscillations in the height of the flood after this had reached its height.

4. *Height of the streams during the flood.*—The height of these valley formations above *flood-level* has been stated on preceding pages.

The following table contains these heights, and also the corresponding heights above mean *high-tide level.*

	Above flood level.	Above high-tide level.
On the Housatonic, at Birmingham,	95 feet.	110 feet.
On the Connecticut, at Middletown,	150 feet.	170 feet.
" " at Hartford,	160 feet.	185 feet.
" " at Springfield,	180 feet.	237 feet.
On the Thames, at Norwich,	110 feet.	117 feet.
Head of Narragansett Bay, at Providence,	80 feet.	80 feet.

To obtain the actual height above high-tide level, these numbers should be reduced by whatever was the amount of depression of the coast region during the Champlain period—that is by 15 feet, if that was the true amount.

Another reduction also is required.

The heights of shell-bearing beaches on the coasts of Massachusetts, Maine, the Labrador coast, Lake Champlain, and the St. Lawrence, show that the amount of depression in the Champlain period (or of elevation since) *increased to the northward.* If this was true along the eastern coast of New England, it was probably true also for the interior; indeed, Lake Champlain and the St. Lawrence at Montreal are part of the interior. Since the height of these beaches at Montreal have been shown by Dawson to indicate a depression there of 500 feet, and since that on the Sound was certainly not over 45 feet, and probably not over 15 feet, the average rate of increase from the Sound northward was about *one foot and a half* a mile.*

Adopting this as the average rate, the amount of depression in consequence of it was increased at Birmingham, 10 miles from the Sound, by 15 feet.

At Middletown,	20 miles in an air-line,		by 30 feet.
At Hartford,	35 miles,	"	by 52½ feet.
At Springfield,	58 miles,	"	by 87 feet.
At Norwich,	15 miles,	"	by 22½ feet.
At Providence,	28 miles,	"	by 42 feet.

Providence, though 26 miles from the Sound, is 28 miles from a line having the course of the shore meadows west of Point Judith.

With an increase northward, in the depression, of only 1 foot a mile, the depression derived from the source here considered, would have been *one-third* less at each of the places.

5. *Pitch of the stream during the flood.*—Owing to this depression, the pitch of the streams seaward was diminished. From the above numbers we obtained for the *average* pitch of the rivers to the Sound approximately:

8	feet	per mile	below	Birmingham.
4·5	"	"	"	Middletown.
5	"	"	"	Norwich.
1	foot	"	"	Providence.

In the highest modern floods, the pitch is about two feet below Birmingham; nine inches below Middletown; six inches below Norwich; and not over one inch below Providence.

* This subsidence, so much greater to the north over New England than to the south, must have occasioned, as I have stated in other places, nearly or quite a cessation in the movement of the glacier; and in this condition the ice appears to have melted away. One important consequence of this is the absence from New England of all terminal moraines. Terminal moraines are made from long-continued successive depositions of moraine-material (rocks, gravel, sand) at the terminus or melting-place of the *moving* glacier; and were there no motion none would form. On this account, the subject of terminal moraines has not been included in this memoir on "Southern New England during the melting of the great glacier."

Or, supposing the rate of increase of depression northward over *Southern* New England only one foot per mile, or less than farther north, the pitch would have been approximately:

8·5	feet	per mile	below	Birmingham.
5	"	"	"	Middletown.
5·5	"	"	"	Norwich.
1·5	"	"	"	Providence.

If the land were 15 feet below its present level along the southern coast of New England, and the average rate of increase of depression northward were 1 foot per mile, the height of the flood waters above high tide would have been

At Birmingham,	85	feet.
At Middletown,	135	"
At Hartford,	135	"
At Springfield,	164	"
At Norwich,	87	"
At Providence,	37	"

It hence appears that, with 1 foot a mile as the northward increase of depression of the land, the waters would have been at Hartford on the same level as at Middletown; and at Springfield, but 29 feet above the level at Hartford, instead of 38½ feet, the present difference; and the pitch from Springfield to Hartford would have been about a foot a mile. With 1½ feet a mile of increase of depression northward, the flood at Springfield would have been only 10 feet higher than at Middletown.

7. *Glacial conditions.*—Whatever the rate of increase northward in the depression, and whatever the amount of actual depression along the Sound, only a small part of the marvellous in the glacial flood is removed. The rivers in the lower parts of the Housatonic, Connecticut and Thames valleys were cataracts on a scale beyond all modern knowledge. The waters from the melting glacier must have been poured down the streams in vast volume to have piled to so great heights before outlets so wide and so deep. And such facts are but examples of a condition that prevailed generally over the glacier-covered lands.

6. *The Champlain a Fluvial period.*—It is to be remembered that the glacier consisted of the precipitated waters of many thousands of winters, each winter, too, a year in length. Hence, when the melting reached its height, some centuries of precipitated moisture were let loose at once. The results from the action of the great rivers of the era are registered in the height and width of the valley formations. The Champlain was eminently the Fluvial period of the earth's history, while *pluvially*, or as it respects rain, it may not have exceeded the present time.

This last section of my Memoir, which purported at the outset to treat of the amount of depression of the land during the melting of the glacier, has turned largely into a discussion of the evidences and effects of the Glacial flood—the subject of the first section. This is a consequence of the fact that the extent of the upper valley-terraces afford stronger testimony to the flood than the material of the beds, enabling us even to deduce the depth and spread and pitch of the flowing waters through the New England valleys, large and small.

In another paper I propose to present other facts on this and connected topics derived from Long Island and the associated lands off the Southern New England coast.

ART. LIII.—*Ammonia a constant contaminant of Sulphuric Acid;* by F. H. STORER, Professor of Agricultural Chemistry in Harvard University.

IN reflecting upon some of the reactions by which ammonia is known to be formed, it occurred to me that this substance would very probably, or perhaps necessarily, be produced in the ordinary process of making sulphuric acid, and that it might remain as a contamination in the acid as used in the chemical arts. Acting upon the idea I have myself tested and have caused to be tested carefully and methodically a considerable number of samples of sulphuric acid obtained from different chemical works, and have found that every one of the specimens examined contained appreciable quantities of ammonia. I find, moreover, on looking the matter up, that the observation is not new, inasmuch as Schœnbein* has stated, so long ago as 1862, that he found traces of ammonia in all the samples of oil of vitriol which he had tested for that substance. My experiments would nevertheless seem to be worthy of publication, both because they confirm Schœnbein's statement and because they go to show that ammonia is far more generally distributed as an impurity of chemical substances than has been commonly supposed hitherto.

The following acids were examined quantitatively by distilling a small portion of each of them with milk of lime, free from ammonia, and applying Nessler's reagent to the distillate, in the manner described by Wanklyn, in his "Water-Analysis," London, 1874.

I. Oil of vitriol from a carboy bought at Bay-Side Alkali Works, South Boston.

II. Oil of vitriol from the Chemical Works at North Billerica, Mass.

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* Wagner's Jahresbericht Chem. Technologie, viii, 266.

III. Oil of vitriol from Eaton's Chemical Works, South Wilmington, Mass.
IV. Oil of vitriol, "chemically pure," from Trommsdorff of Erfurt.
V. Pan acid from the Works at North Billerica.
VI. Pan acid from Eaton's Works.
VII. Chamber acid from Eaton's Works.
VIII. Chamber acid from the Works at North Billerica.
IX. Chamber acid from the Merrimack Print Works at Lowell.

Five cubic centimeters* of the acid	Gave grams of ammonia (NH_3).
No. I	0·000150
" II	0·000075
" III	0·000245
" IV	0·000095
" V	0·000070
" VI	0·000140
" VII	0·000050
" VIII	0·000158
" IX	0·000090

Care was taken to procure the above samples of acids (excepting Nos. I and IV) directly from the works where they were made. Each of the specimens was kept in a full and tightly closed glass stoppered bottle until tested, and the portion tested was taken from the middle of the bottle. With the exception of Trommsdorff's acid (No. 4) all the samples are known to have been made from sulphur, i. e., not from pyrites. Several other samples of oil of vitriol of unknown origin were found to contain ammonia when tested qualitatively with Nessler's reagent, and with the even more delicate reagent of Einbrodt.† So too when applied, for the sake of control, to the distillate from acid No. 4 of the foregoing list, Einbrodt's test gave a very strong reaction for ammonia.

There are several ways in which sulphuric acid may be contaminated with ammonia. Some insignificant traces of this substance are of course contained in the air which is used for making the acid, and a still larger amount is often contained in the water that plays so important a part in the process of manufacture. It is not impossible indeed that nitrogen compounds in the water may sometimes be the cause of appreciable traces of ammonia in the acid. It is easy to conceive moreover that considerable quantities of ammonia may be formed in the apparatus of the sulphuric acid maker through reduction of nitric acid or other oxide of nitrogen that is necessarily present, formed when warm dilute nitric acid is made to act upon lead or upon sulphur.

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* The weight of 5 c. c. of the oil of vitriol was rather more than 9 grams in each instance; that of the pan acid was about 8½ grams, and that of the chamber acid rather more than 7 grams.

† Mercuric chloride in alkaline solutions. See Liebig & Kopp's Jahresbericht, 1852, v, 723 and 1863, xvi, 167.

Action of dilute Nitric Acid on Lead.—A quantity of soft, clean commercial lead that had just been remelted was placed in a small glass flask and 50 c. c. of dilute nitric acid (sp. gr. 1·15) were poured upon it. The flask was closed against the air with a gas delivery tube, and after the action of the acid had ceased the solution was boiled with the milk of lime and the distillate tested for ammonia. But the reaction with Nessler's liquor was so strong that no estimation of the amount of ammonia could be made. A quantity of the lead (30 grams) boiled by itself in the milk of lime gave a distillate in which no ammonia could be detected by the Nessler test. On the other hand, 50 c. c. of the dilute nitric acid were found to contain 0·000025 gram of ammonia.

In a second trial 50 grams of the commercial lead were warmed during three hours with 50 c. c. of the dilute nitric acid. The solution was distilled with the milk of lime and the ammonia in the distillate was estimated by titration with standard oxalic acid:—0·002483 gram of ammonia was found.

In a third trial 25 grams of pure lead (from Marquart of Bonn,) were warmed with 50 c. c. of the dilute nitric acid and 0·003279 gram of ammonia was found in the solution of nitrate of lead.

Action of dilute Nitric Acid on Copper.—15 grams of clean copper clippings were gently warmed with 50 c. c. of the dilute nitric acid until there was no more action. The solution was distilled with milk of lime, and the ammonia estimated by Nessler's test. 0·00004 gram of ammonia was found.

Action of dilute Nitric Acid on Sulphur.—20 grams of powdered brimstone were added to 50 c. c. of the dilute nitric acid and the mixture was maintained at or near the temperature of boiling for three hours. On testing the liquid an abundance of ammonia was found.

In another trial, 20 grams of the powdered brimstone were mixed with 100 c. c. of the dilute nitric acid. The mixture was allowed to stand in the cold for 48 hours, and then boiled gently during 8 hours. On testing the liquor by the Nessler process 0·00225 gram of ammonia was found in it.

A small amount of nitrogen oxides may perhaps be reduced to ammonia in the process of sulphuric acid making by other deoxidizing agents, such as the organic impurities of crude sulphur,* or sulphuretted hydrogen,† or even by sulphurous acid, though in a single experiment in which sulphurous acid, evolved from copper clippings, was passed into dilute nitric acid (sp. gr. 1·15) for a couple of hours no ammonia could be detected in the liquid. The experiment of Schœnbein* moreover is to be remembered, in which ammonia, as well as sulphurous and sulphuric acids, was detected in water above which sulphur had been burned in the air. It would seem to be plain, however, that the substances previously mentioned must usually be the most efficient agents for the production of the ammonia.

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* See Wagner's Jahresbericht Chem. Technologie, x, 149.

† Compare Johnston, in Gmelin's Handbook, ii, 396.

Doubtless a good deal of the ammonia thus formed by reduction is destroyed again by reacting upon oxides of nitrogen in the liquid during the process of concentrating the weak sulphuric acid. Pelouze proposed long ago† to free the acid from nitrous contaminations by heating it with sulphate of ammonia. The reaction, though efficient, is simply incomplete.

The presence of ammonia in sulphuric acid having been proved, the question presents itself whether, in spite of the tendency of ammonia to change to nitrous and nitric acid through oxidation, traces of it may not occur in a great variety of the chemicals in whose preparation sulphuric acid takes part. The following list comprises several substances, chosen somewhat at random, which I have caused to be tested for ammonia, some of them for the sake of proving this idea, though mostly for a different purpose. The substances under examination were boiled with milk of lime that had been proved in each instance to be free from any trace of ammonia by long continued boiling, and testing of the distillates obtained from it. The distillates obtained after the addition of the substance to the lime were received in graduated tubes and tested successively with Nessler's reagent until they ceased to show any coloration.

	100 grams (or c. c.) of the substance contained gram of NH_3.
Nitric acid, taken from a carboy of pure acid from Bayside Alkali Works, 10 c. c. gave 0·000035 gram of ammonia (NH_3).	0·00035
Chlorhydric acid, the ordinary concentrated commercial acid, 10 c. c. gave 0·000015 gram of ammonia,	0·00015
Ditto from a carboy of pure acid from Bayside Alkali Works, 10 c. c. gave very little ammonia,	still less than the preceding.
Acetic acid, chemically pure, from C. White & Co., Philadelphia, 10 c. c. gave no reaction for ammonia,	0·00000
Potash alum, pure from Marquart of Bonn, gave strong reaction for ammonia,	much.
Ditto, pure from Trommsdorff, gave strong smell of ammonia when heated with lime. There was so much ammonia that it could not be estimated by the Nessler test, when 10 grams of the alum were operated upon,	much.

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* Journ. prakt. Chemie, lxxxvi, 145.

† Gmelin's Handbook, ii, 183.

	100 grams (or c. c.) of the substance contained gram of NH_3.
Sulphate of alumina, pure from Marquart, 20 grams gave too much ammonia to be measured by the Nessler process (as used by us), and so did 5 grams,	much.
Sulphate of iron (ferrous sulphate), 5 grams purified, from Powers & Weightman of Philadelphia, gave 0·00062 gram of ammonia,	0·01240
Ditto, common copperas, the last of an apothecary's barrel, 5 grams gave 0·00507 gram of ammonia,	0·10140
Ditto, copperas of unknown origin taken from a bottle in store room of Bussey Laboratory, 100 grams gave only 0·00053 gram of ammonia,	0·00053
Ditto, a sample of pure sulphate of iron from Marquart, gave a strong reaction for ammonia when tested qualitatively, and a reaction was obtained also from a sample of Marquart's sulphate ("pure by alcohol,")	
Sulphate of lime, pure precipitated, from Marquart, 35 grams gave 0·0002 gram of ammonia,	0·00057
Other samples of sulphate of lime, tested in a slightly different way, gave the following results:—545 grams of ground gypsum, obtained originally at a seed store but kept in a store-room of the Bussey Institution for a year or more, were percolated with pure water (two liters) until no reaction for ammonia could be detected and the percolate was boiled without addition of lime, 0·00392 gram of ammonia was found in the distillate. On adding lime a new portion of ammonia equal to 0·00008 was given off,	0·000734
20 grams of ground gypsum bought at a seed store and tested immediately on reaching the laboratory, gave 0·00003 gram of ammonia on being boiled in pure water, and no more ammonia came off on adding milk of lime,	0·00015
150 grams of plaster of Paris, taken from a box in the Bussey store-room, gave 0·0035 gram of ammonia on being boiled with water alone, and 0·00002 gram more of ammonia came off on boiling with lime,	0·00234
20 grams of plaster of Paris taken from a keg found standing in a recently built house, gave 0·000035 gram of ammonia on being boiled with water alone,	0·000175+
Sulphate of potash, chemically pure, from Trommsdorff, 20 grams gave only a faint trace of ammonia,	trace.
Sulphate of soda, chemically pure crystals from Marquart, 20 grams gave no trace of ammonia. This result was verified by many qualitative trials,	0·00000
Bisulphate of soda from Marquart, gave strong reaction for ammonia when tested qualitatively,	much.
Sulphate of copper, pure from Marquart, 10 grams gave 0·000105 gram of ammonia,	0·00105

	100 grams (or c. c.) of the substance contained gram of NH_3.
Nitrate of potash, pure crystals from Marquart, 20 grams gave 0·000015 gram of ammonia,	0·000075
Nitrate of soda, taken from a bag of the crude nitrate of commerce, 20 grams gave 0·00004 gram of ammonia,	0·00020
Common salt taken from middle of a large lump of native rock salt bought at a grocer's shop, 20 grams gave 0·000022 gram of ammonia. Compare Vogel's detection of chloride of ammonium in rock salt (Gmelin's Hand-book, ii, 416),	0·00011
Ditto, pure from Marquart, 20 grams gave 0·0000425 gram of ammonia,	0·000213
Ditto, a sample prepared by dissolving pure crystals of carbonate of soda in pure chlorhydric acid gave no reaction when tested directly with Nessler's reagent,	none.
Phosphate of soda, pure crystals from Marquart, 20 grams gave no reaction for ammonia,	0·00000
Acetate of soda, pure crystals, 20 grams gave no reaction for ammonia,	0·00000
Carbonate of soda, pure crystals from Marquart, gave no reaction for ammonia when tested directly with Nessler's reagent,	0·00000
Carbonate of potash, pure from Marquart, gave no reaction when tested directly,	0·00000
Hydrate of soda, pure from Marquart, 5 grams gave no reaction for ammonia,	0·00000
Hydrate of lime, obtained by slacking the excellent quicklime from Brandon, Vt., 20 grams of the whitest portion of the mass gave no reaction for ammonia, while 20 grams of the grayest portion gave 0·00001 gram of ammonia,	0·00005
Chlorate of potash from Marquart gave 0·0001 gram of ammonia,	0·00050
Crude sulphur, from a box in Bussey store-room, 10 grams gave 0·00015 gram of ammonia,	0·00150
Flowers of sulphur from Marquart, 10 grams gave 0·00075 gram of ammonia,	0·00750
Sulphide of sodium from Marquart, 5 grams gave 0·001 gram of ammonia,	0·02000
Sulphide of potassium from Marquart, 5 grams gave 0·0009 gram ammonia,	0·01800
Sulphide of iron from Marquart, 5 grams gave 0·00085 gram of ammonia,	0·01700
Ditto, from a quantity of the foregoing that had just been fused in a Hessian crucible, 20 grams taken from the middle of the solid cake, gave 0·00025 gram of ammonia,	0·00125

potassium and iron, on being tested with Einbrodt's reagent for the sake of control, gave strong reactions for ammonia.

In order to avoid any confusion that might arise from coloration of the Nessler reagent by sulphuretted hydrogen, the following modifications of the ordinary process were employed in testing sulphur and the sulphides. In the case of sulphide of potassium and sulphide of sodium, the weighed substance was dissolved in about half a litre of pure water, free from ammonia, the solution was distilled and a quarter litre of distillate was collected, experience having shown that all the ammonia in the sulphide came forward in this amount of liquid. The distillate was acidified with a few drops of sulphuric acid, boiled until sulphuretted hydrogen had ceased to come off and then redistilled with milk of lime, the ammonia in the new distillate being determined with Nessler's reagent in the usual way.

In the case of sulphur and sulphide of iron the finely powdered, weighed substance was distilled with milk of lime, the distillate was acidified and boiled to expel sulphuretted hydrogen and finally redistilled in the manner just described.

Oxalic acid, "tertium depuratum" from Marquart, 20 grams gave 0·0003 gram of ammonia,	0·00150
Tartaric acid, from Marquart, 10 grams gave 0·0001 gram of ammonia,	0·00100
Vogel (Wagner's Jahresbericht, ix, 526) found 0·012 per cent of ammonia in a sample of crude tartar,	0·01200
Boracic acid, pure from Marquart, 10 grams gave 0·000055 gram of ammonia,	0·00055;

that is to say, rather less than might have been inferred from the fact that ammonium compounds accompany boracic acid in the Tuscan lagoons (see Gmelin's Handbook, ii, pp. 97, 98; Bechi, Wagner's Jahresbericht, ix, 355; Vohl, ibid. xii, 205, and ibid. (N. S.) i, 210.)

Though the figures in the foregoing table may seem small or even insignificant to persons unaccustomed to use Nessler's test, they are really large in several instances and noteworthy in all. It should be understood, moreover, that in working with Nessler's process it is easy to exclude ammonia from the water and from the other reagents that are employed and to avoid the ammonia of the air. It is hardly necessary to urge that the utmost care has been exercised in these respects in all the foregoing tests. A large proportion of the substances tested were taken from the tightly closed, particularly well-ground, glass-stoppered bottles in which they had been imported and which had never been opened until the time of applying the test, but it is noteworthy that this precaution seemed to be devoid of significance.

Those substances, such as sulphate of soda for example, which contained no ammonia when taken from freshly opened bottles, likewise contained none when taken from bottles that had been frequently opened and which had stood in a store-room with other chemicals during three or four years. I find naturally enough, that filter paper and other porous materials that have been exposed to the air of a laboratory are highly charged with ammonia, in the same way that the reagent bottles upon our shelves become coated with ammonium compounds, but it would seem, nevertheless, that there is a limit to the penetrative power of the ammoniacal gases. It will be noticed for that matter that a tolerably large proportion of the substances examined contained no ammonia whatsoever, and in general I have not observed that chemicals taken from their bottles at the moment of reaching the laboratory are any more liable to be free from ammonia than those which have been long in store.

A good idea of the relative importance of the ammonia found in the chemicals above described may be got by contrasting the figures given in the table with the amounts of ammonia that occur in natural waters as given by Wanklyn and by many other authorities, or by comparing the above results obtained from chemicals with the following statement of results that were obtained from rain water in this laboratory at the same time and by the same operator.

	Contained milligrams of		100 c. c. of the water contained grams free NH^3.
	Free NH^4.	Albumenoid ammonia.	
One litre of rain water taken from south cistern of Bussey Inst.			
April 20, 1875,	·532	·08	0·0000532
April 20, 1875,	·532	·06	0·0000532
April 22, 1875,	·480	....	0·000048
May 4, 1875,	·500	·10	0·00005
June 7, 1875,	·480	....	0·000048
June 7, 1875,	·475	....	0·0000475
One litre of rain water caught in dish on roof of Bussey Inst.			
July 23, 1875,	·100	....	0·00001
July 29, 1875,	·300	....	0·00003
One litre of rain water from snow caught			
April 20, 1875,	·147	·06	0·0000147

It is worthy of mention that water obtained by melting ice —such as sold hereabouts, as in all American towns, for domestic use—is remarkably free from ammonia. Thus a litre of water obtained by melting a block of Muddy Pond ice was found to contain only ·013 milligram of ammonia. In other words, 100 c. c. of the melted ice contained no more than melting the clear portion of a block of Jamaica Pond ice, contained ·04 milligram of ammonia, and a litre of water obtained by melting the cloudy porous portion of the same block yielded precisely the same amount, viz: at the rate of 0·000004 gram of ammonia in 100 c. c. of the water. By distilling off from a glass vessel a comparatively small proportion of the melted ice it is easy to expel these traces of ammonia and to obtain water that is practically free from that substance, such as is needed for making the standards of comparison upon which the Nessler test depends. It appeared in fact that before such distillation the mere melted ice was as free from ammonia as the water of deep wells in this neighborhood, that had been slowly boiled down in a copper still to four-fifths of its original volume for the express purpose of expelling ammonia. Both the melted ice and the purified well waters had to be distilled anew in glass vessels in order to obtain water that was completely free from ammonia, but the proportion of impure distillate to be thrown aside was no larger in the one case than in the other.

I am indebted to my assistant, Mr. D. S. Lewis. for his skillful coöperation in this research, and to my friend Mr. F. P. Pearson, chemist of the Merrimack Print Works, for a number of samples of acids.

Bussey Institution, Jamaica Plain, Mass., September, 1875.

Art. LIV.—*Abstract of a Memoir on the Origin of the Alps;* by Prof. Edward Suess, of Vienna.*

According to the views of the early geologists, still widely accepted, the origin of mountains is to be ascribed to the elevation of a molten or semi-molten mass which has thrown up the rocks along its axis, and crowded the upper strata to the right and left, forming in this way a mountain-chain. This principle has been applied to the Alps by Studer, and, in accordance with this it has been customary to speak of a middle zone, embracing the isolated central masses, with parallel subordinate zones to the north and south. The folding and banded arrangement in the outer chains has been ascribed to a mighty pressure which has been exerted in a northerly or southerly direction by the central zone, as it was elevated from below. The cause of the elevation has been left entirely unexplained.

This view is, however, at variance with all the facts observed. It may be true that the granites of the Alps are in great measure of eruptive origin; but they are unquestionably much older than the Molasse of Lucerne, so that they can have had no influence in the dynamic changes in which it has been involved; moreover, with the exception of one or two unimportant cases, no example can be shown in which eruptive rocks have been the cause of change of position in the older sedimentary strata. Another argument against this view is found in the irregular, shattered condition of the central masses as contrasted with the even trend in the folds of the outer-lying mountain chains. A glance, for instance, at the position of the crystalline rocks of the Finsteraarhorn, overlying the younger strata, shows that the folding-over must have originated, not in the eruption or expansion of isolated central masses, but in some general, horizontal movement of the mountain-system as a whole.

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* Die Entstehung der Alpen, 168 pp. 8vo. Wien, 1875.

In the general consideration of this subject it is to be explained that the term Alpine System is intended to include all the mountain chains, with their various branches, from the Jura Mountains to the Appenines in the south, and the Carpathians in the east—in other words all those mountains which show a constant predominance of certain trends or lines of directions. The western and northern limits of this extended region are formed by the older elevations of the Iles d'Hières, the eastern edge of the Central-Plateau of France, the southern extremities of the Vosges Mountains and the Black Forest, with the southern border of Bohemia. Within this limit, the Alps are developed with wonderful regularity, stretching in great curves from the end of one of these older mountain points to the next; and against them the rocks have been pressed up and shoved on in parallel lines, as against immovable barriers. An example of this action is furnished by the island of gneiss and *rothliegendes* at Dôle, which forms the southeastern continuation of the Vosges Mountains, where the dependence of the folds and fractures in the Jura on the distribution of the older rocks can be most clearly seen. The whole Jura Mountains have been here pressed up into many parallel bands, while on the other side of the obstruction the Jurassic deposits cover a wide area without showing any trace of this tremendous horizontal movement. This same principle is true of the Alps to the east, but it is to be noticed that in the Juras the rocks in the northern border are continued immediately beyond the limits of the mountains, while in the eastern Alps the rocks, which on the northern side tower over the plain, have as a rule no distinct continuation on the other side of it.

Again, each branch of the Alpine system is everywhere *one-sided*, not symmetrically formed; while, at the same time, the moving power was alike throughout. This point is especially well exemplified by the Appenines, whose structure deserves a

Molasse and Flysch rise gradually from the Piedmontese plain, and extend southward in great curves. In the neighborhood of Bologna the Flysch forms the dividing line between the inner-Appenine depressions of Tuscany and the outer-Appenine region of Adria, and it stretches on in an unbroken course through the peninsula to the Gulf of Tarentum. Within this limit the limestone mountains extend uninterrupted from Spezia southward, embracing the Abruzzen, the Gran Sasso and the elevations of the Basilicata. Still within the line, on the western coast of Italy, are found the isolated fragments of the older crystalline rocks. As traces of the action of the mighty forces which caused this great horizontal shove, we may point to the wide areas of depression of the Tyrrhenian and eastern part of the Ligurian seas, while between the ruins of the ancient rocks the fissures are to this day in part open, on which are situated a long series of volcanoes, and along which earthquake shocks are propagated. Thus the Appenines show two sides differing essentially from each other—one the side of shoving and folding, the other of fracture and volcanic phenomena; the former is convex and continuous, the latter is interrupted by areas of depression.

The western Alps repeat the same contrast of a folded outer side, and an inner side of fracture, though here the volcanic mountains are wanting. At no point on the southern side of these western Alps can an equivalent of the long anticlinal of the Molasse be found; in no case can a profile be given which shall show an older middle zone flanked by symmetrical side zones. The Juras, too, are a model of a true one-sided movement, caused by pressure against an immovable foreign mass of older rocks. The fracture line is in this case turned toward the Alps.

The eastern Alps alone show a great series of Mesozoic and Tertiary rocks on their southern side, which might be regarded as belonging to the hypothetical southern zone. If we attempt, however, to compare the long series of regular folds, which are so conspicuous in the northern zone, with the rocks on the other side, we find that nowhere in the latter is there the slightest correspondence. The careful consideration of the relations here exhibited shows that the strata do not conform in strike with those of the northern zone. On the contrary, we are justified in concluding that this broad mountain girdle separates toward the east into several one-sided chains.

The same one-sided structure belongs to the Carpathians and the other branches of the Alpine system to the east and south. This principle established, it becomes clear that we must abandon the idea of a symmetrical structure—a middle zone with two equal and corresponding side zones, and grant that the whole mountain-chain, from the Appenines to the Carpathians, is the product of a common force, which has acted more or less in a horizontal direction.

In regard to the age of the Alps, or more properly the epoch in which they were elevated, a somewhat different view must be adopted than that which was accepted in former years. It is now unquestioned that strata belonging to the middle Tertiary have shared in the general movement. This shows that the movements which have ended in the elevation of this chain continued up to a comparatively recent time. It is equally true, however, that the same regions have in much earlier times repeatedly suffered similar movements, as is shown by the position of the younger sediments on the oldest rocks of the same chain. The many examples of this truth which might be quoted show that, up to a time which extends far into the Mesozoic age, the region of the Alps was often the theater of great catastrophes. The greater abundance of eruptive rocks in the southern Alps shows that in the earlier times the course of action was essentially the same as in later epochs.

The consideration of all the subjects touched upon in the preceding paragraphs lead to conclusions which to a very considerable extent agree with those arrived at by Prof. Dana in his discussion of mountain-making in general.

The force which acted to produce the results, which we see to-day must have been a *horizontal* one, as is abundantly proved by a survey of all the facts. The exertion of this horizontal force was essentially influenced by resistance from four different sources: 1, from the presence of foreign masses of older rocks; 2, from the folding mass itself; 3, from the occasional introduction of older volcanic rocks, as granite and porphyry, in the moving mass; 4, finally, it appears that single mountain masses, like the Adamello or the red-porphyry, near Botzen, have exerted an essential influence on the development of the surrounding mountain region.

The examination of the various mountain regions of Europe not included in the Alpine system, gives confirmation of the views thus far expressed in regard to the one-sided nature of mountains, and the horizontal shove which has been the cause of their elevation. This is true of the Bohemian region, taken as a whole; of the Riesengebirge, the Erzgebirge, and so on.

[For a detailed discussion of the subject, reference must be made to the complete memoir, of which this is an abstract.]

The direction of the fracture lines varies from northeast to northwest, and the motion was mostly to the northward, though some isolated exceptions, in the case of a southerly movement, however, and pass out of Europe to America, and then further study in as great detail as is now possible the great mountain-chains of Asia, we arrive at this grand conclusion: throughout, mountain-masses and mountain-movements are *one-sided*, and the direction of the movement is in general *northwest*, *north*, *or northeast*, in North America and Europe, but southerly, or southeasterly, in central Asia. There is no regular geometrical arrangement in the mountain chains.

Looking at the facts which have been stated, making only the supposition that an unequal contraction of the surface of the planet has taken place, we see that the simplest form of mountain consists in a fracture, which runs at right angles to the direction of the contraction; the fractured part moves forward in the direction of the force from contraction, while volcanic phenomena may manifest themselves at the line of breakage. The Erzgebirge forms an example of such a mountain-mass.

The second and most common form begins with a main fold striking across the direction of contraction, and inclined toward that. The breakage takes place in the fold in the line of greatest tension. If the force continues, the part of this fold in advance will be pushed still farther on, piling up before it the sedimentary strata into broader, subordinate folds, while the part behind sinks down, and, between its fragments, the volcanic phenomena appear. Thus it is with the Appenines. In case of an obstruction in front the mass may be turned aside, and, in fact, many complications may thus arise.

Still a third form consists in the formation of a large number of parallel folds which cover a considerable area, but with a steep line, as a rule, on the inner side of the fracture, while the volcanic phenomena are wanting. Here belong the Jura Mountains. Cases may also occur in which the width of the main-fold is so great that there results, not a mountain-chain, but a general mass-elevation; an example of this may be found in the recent changes of level observed on the Scandinavian coast.

In regard to the depth at which the contraction producing the lateral pressure took place, it may be safest to say that while in many cases the depth must have been very great, in others the contrary is true. Thus the movement which occasioned the fracture of the Erzgebirge must have taken place at a great depth, as also those which elevated the oldest rocks of the Alps. On the other hand, the shoving forward of the northeastern Alps, and the deviation in their direction, belong to a higher horizon, and the elevation of the Molasse to one still higher, while examples may also be given of foldings which must have been very shallow.

No influence of a radial contraction has bee is there any effect in the direction of the wave which can be attributed to the rotation of the ea

In conclusion, it may be remarked that mou a whole can be regarded as a stiffening of the which process has been determined by the certain older rigid masses. These may be mad ain lines pushed up together, and crossing ea Bohemia, or they may consist of wide extended strata, even the oldest, have retained their hori as in the great Russian plain. These primitive to no geometrical law either in outline or distr they have determined the form and course of contraction has produced in the more pliant earth's surface between them.

Art. LV.—*Studies on Magnetic Distribution*; Rowland, of the Johns Hopkins University,

(Continued from page 335.)

Table I. is from a bar 17½ inches long wit helix 1½ inch long at one end, the zero-point be Table II. is from a bar 9 feet long with a helix quite near one end, the zero-point being at 1 inc toward the long end. Table III. is from a bar a helix 4½ inches long near one end, so that its inches from the end on which the experiments zero-point being at the end.

Table l. Bar ·18 inch diameter. 0 at end of

L.	Q'_ε. Observed.	Q'_ε. Calculated.	Error of Q'_ε.	Q'. Observed.	Q'. Calc lated
0	2·7	----	----	0	0·
3	3·2	----	----	2·7	3·5
5	2·0	2·0	0	5·9	6·6
6	2·5	2·4	— ·1	7·9	8.6
7	3·2	2·8	— ·4	10·4	11·0
8	2·7	3·5	— ·2	13·6	13·8
9	4·3	4·3	0	17·3	17·3
10	5·3	5·2	— ·1	21·6	21·6
11	6·5	6·5	0	26·9	26·8
12	7·7	8·0	+ ·3	33·4	33·3
13	9·5	9·9	+ ·4	41·1	41·3
14				50·6	51·2

$$Q' = 2{\cdot}60(\varepsilon^{\cdot 213L} - \varepsilon^{-\cdot 213L}).$$

$$Q'_\varepsilon = r2{\cdot}60(\varepsilon^{\cdot 213L} + \varepsilon^{-\cdot 213L}) = {\cdot}554(\varepsilon^{\cdot 213L} + \varepsilon^{-\cdot 213L}).$$

we may assume that at the end of the bar $s = \infty$ and $C = 0$, which is equivalent to assuming that the number of lines of induction which pass out at the end of the rod are too small to be appreciated.

In Table II. observations were not made over the whole length of the rod, and the zero-point was not at the end of the bar. It is evident, however, that by giving a proper value to s we may suppose the bar to end at any point. As the rod is very long, expressions of the form

$$Q' - C'' = C'\epsilon^{-rL} - C'' \text{ and } Q'_\epsilon = rC'\epsilon^{-rL}$$

will apply.

Table II. Bar ·39 inch diameter. 0 at 1 inch from helix.

L.	Q'_ϵ. Observed.	Q'_ϵ. Calculated.	Error of Q'_ϵ.	$Q'-C''$. Observed.	$Q'-C''$. Calculated.	Error of Q'.
0	....	....	....	....	902·5	
1	71·7	70·8	− ·9	825·2	825·9	+ ·7
2	65·2	65·3	+ ·1	753·5	755·1	+1·6
3	59·5	60·2	+ ·7	688·3	689·8	+1·5
4	53·5	55·5	+2·0	628·8	629·5	+ ·7
5	51·2	51·2	0	575·3	574·3	−1·0
6	46·7	47·2	+ ·5	524·1	523·1	−1·0
7	43·2	43·5	+ ·3	477·4	476·0	−1·4
8	40·0	40·1	+ ·1	434·2	432·5	−1·7
9	37·2	37·0	− ·2	394·2	392·5	−1·7
10	34·7	34·1	− ·6	357·0	355·6	−1·4
11	31·7	31·4	− ·3	322·3	321·5	− ·8
12	29·5	28·9	− ·6	290·6	290·1	− ·5
13	25·7	26·6	+ ·9	261·1	261·2	+ ·1
14	25·5	24·6	− ·9	235·4	235·5	− ·9
15	22·0	22·7	+ ·7	210·9	209·0	+ ·1
16	21·5	20·9	− ·6	187·9	187·3	− ·6
17	20·0	19·3	− ·7	166·4	166·4	0
18	19·1	17·8	−1·3	146·4	147·1	+ ·7
19	32·5	31·5	−1·0	127·3	129·4	+2·1
21	27·5	26·7	− ·8	94·8	97·8	+3·0
23	23·0	22·8	− ·2	67·3	71·1	+3·8
25	18·5	19·4	+ ·9	44·3	48·6	+4·3
27	14·5	16·5	+2·0	25·8	29·0	+3·2
29	11·3	14·0	+2·7	11·3	12·6	+1·3
31				0	− 1·2	−1·2

$$Q' - C'' = 983\epsilon^{-\cdot 08135L} - 80{\cdot}5 = 983(10)^{-\cdot 0353L} - 80{\cdot}5.$$

$$Q'_\epsilon = r983\epsilon^{-\cdot 08135L}\Delta L = 80(10)^{-\cdot 0353L}\Delta L.$$

In Table II. the observations were near the end of the rod, and were repeated several times. Neglecting the end of the rod, we have $s = \infty$.

In these tables we see quite a good agreement between theory and observation; but on more careful examination we observe a certain law in the distribution of errors. Thus in Table I. the

TABLE III. Bar ·39 inch diameter. 0 at end of bar.

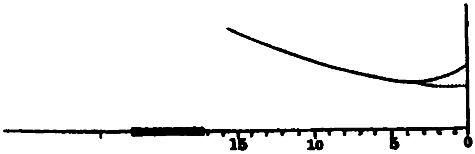
L.	Q'_ε. Observed.	Q'_ε. Calculated.	Error of Q'_ε.	Q'. Observed.	Q'. Calculated.	Error of Q'.
0				0·	0·	0
1	19·7	15·2	−4·5	19·7	15·2	−4·5
2	16·3	15·3	−1·0	36·0	30·5	−5·5
3	16·0	15·5	− ·5	52·0	46·0	−6·0
4	15·8	15·9	+ ·1	67·8	61·8	−6·0
5	16·5	16·3	− ·2	84·3	78·1	−6·3
6	17·0	16·9	− ·1	101·3	95·0	−6·3
7	17·6	17·6	0	118·9	112·6	−6·3
8	18·4	18·4	0	137·3	130·9	−6·4
9	19·2	19·4	+ ·2	156·5	150·3	−6·2
10	20·3	20·5	+ ·2	176·8	170·7	−6·1
11	21·8	21·7	− ·1	198·6	192·2	−6·4
12	22·8	23·1	+ ·3	221·4	215·3	−6·1
13	24·8	24·7	− ·1	246·2	239·9	−6·3
14	26·8	26·5	− ·3	273·0	266·4	−6·6
15	28·8	28·4	− ·5	301·8	294·6	−7.2
16	31·8	30·5	−1·3	333·6	325·1	−8·5

$$Q'_\varepsilon = 7{\cdot}6(10^{\cdot 037L} + 10^{-\cdot 037L});\quad Q' = 89(10^{\cdot 037L} - 10^{-\cdot 037L}).$$

errors of Q′ are all positive between 0 and 8 inches; and this has always been found to be the case at this part of the bar in all my experiments.

The explanation of this is very simple. In obtaining the formulæ we assumed that the magnetic permeability of the bar μ was a constant quantity; but it has been shown by Dr. Stoletow and myself, independently of each other, that μ increases as the magnetism of the bar increases when the latter is not great. Hence between 0 and 8 inches the resistance of the bar R is greater than at succeeding points, and hence a less number of lines of induction pass down the bar from 8 towards 0 than would be given by the formula which has been adapted to the average value of R at from 9 to 14 inches. In Table II. this same fact shows itself towards the last of the table, and would probably be more prominent had the table been carried further. However, in this table all things have combined to satisfy the formula with great accuracy.

2.



Distribution at end of bar.

In Table III. we come across a fact of an entirely different nature from the above. Fig. 2 is the plot of this table, and gives the values of Q'_ε at different parts of the rod.

The horizontal line in the figure represents values of L, and the vertical ordinates are values of Q'_ϵ. The full line gives the observed distribution, and the dotted line that according to the formula.

The formula gives the distribution very nearly for all points except those near the end. The formula indicates that Q'_ϵ decreases continully toward the end; but by experiment we see that it increases near this point. On first seeing this, I thought that it was due to some residual magnetism in the bar; but after repeating the experiment several times with proper care, I soon found that this was always the case. I give the following explanation of it:—In the formulæ we have assumed R′, the resistance of the medium, to be a constant; now this resistance includes that of the lines of force as they pass from the rod through the medium and thus back to the other end of the rod; and of this whole quantity the part which affects the relative distribution at any part of the rod most is that of the medium immediately surrounding that part; and so the parts near the end have the advantage over those further back, inasmuch as the lines can pass forward as well as outward into the medium. The same thing takes place in the case of the distribution of electricity, where the "density" is inversely proportional to the resistance which the lines of inductive force experience from the medium; and here we find that the "density" is greatest on the projections of the body, showing that the resistance to the lines of induction is less in such situations, and by analogy showing that this must also be the case for lines of magnetic force. But this effect is not very great in cylinders until quite near the end; for Coulomb, in a long electrified cylinder, has found the density at one diameter back from the end only 1·25 times that at the center, and so there is probably a long distance in the center where the density is sensibly constant. Hence we may suppose that our second hypothesis that R′ is a constant will be approximately correct for all parts of a bar except the ends, though of course this will vary to some extent with the distribution of the lines in the medium; at least the change in R′ will be gradual except near the end, and so may be partially allowed for by giving a mean value to r.

Hence we see that could the formula be so changed as to include both the variation of R and of R′, it would probably agree with the three tables given.

To study the effect of variation in the permeability more carefully, we can proceed in another manner, and use the formulæ only to get the value of r at different parts of the rods.

No matter how r may vary, equations (2) and (3) will apply to a very small distance l along the rod; and as the origin of coordinates may be at any point on the rod, if Q' and Q'_ϵ are taken at one point and Q and Q_ε at another point whose distance from the first is l, we shall have the four equations

$$Q = C, \qquad Q' = \frac{C}{A+1}(A\varepsilon^{rl} + \varepsilon^{-rl}),$$

$$Q_\varepsilon = \frac{A-1}{A+1}Crl, \qquad Q'_\varepsilon = rl\frac{C}{A+1}(A\varepsilon^{rl} + \varepsilon^{-rl}).$$

Calling $\frac{}{Q} = H$ and $\frac{Q'}{Q} = G$, we shall find, on eliminating C and A and developing ε^{rl} and ε^{-rl},

$$r^2 = \frac{2}{l^2}\left(\frac{GH+1}{G+H} - 1\right); \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

or to a greater degree of approximation,

$$r^2 = \frac{1}{l^2}\left(\sqrt{12\left(2\frac{GH+1}{G+H} + 1\right)} - 6\right). \quad . \quad . \quad . \quad . \qquad (9a)$$

Before applying these formulæ to any series of observations, the latter should be freed from most of the irregularities due to accidental causes. For this purpose the following Tables have been plotted and a *regular* curve drawn to represent as nearly as possible the observations; in other cases a column of differences was formed and plotted. In either case the ordinates of the curves were accepted as the true quantities. But for fear that some may accuse me of tampering with my observations, I have in all cases added these as they were obtained.

The correction is necessary, because small irregularities in the observations will produce immense charges in r^2.

Table IV. contains some of the best observations I have obtained. It is from a bar 57 inches long with a helix 1½ inch long in the center to magnetize it. Each quantity is the mean of six observations, these being made on both ends of the bar and with the current in opposite directions.

In this table a source of error was guarded against which I have not seen mentioned elsewhere. When a bar of iron is magnetized at any part and the distribution over the rest quickly measured, on being then allowed to stand some time and the distribution again taken, it will have changed somewhat, the magnetism having, as it were, creeped down the bar further. Hence in this table time was allowed for the bar to reach its permanent state.

On looking over column 6, which contains the values of $\frac{1}{r^2} = \frac{R'}{R} = R'\alpha\mu$ (equation 7), we observe that as Q' increases, the value of $R'\alpha\mu$ first increases and then decreases. Now it is not probable that R' undergoes any sudden change of this sort,

L.	Q'_{ε}. Observed.	Q'_{ε}. Corrected.	Q'. Corrected.	$r^2 = \frac{R}{R'}$.	$\frac{1}{r^2} = \frac{R'}{R}$.
1			151·7		
2	24·0	24·0	127·7		
3	17·0	17·0	110·7	·041	24·4
4	13·7	13·7	97·0	·0256	39·1
5	11·6	11·65	85·4	·0192	52·1
6	10·2	10·15	75·2	·0168	59·5
7	9·0	9·0	66·2	·0150	66·7
8	8·0	8·0	58·2	·0153	70·4
9	7·1	7·15	51·1	·0150	66·7
10	6·4	6·35	44·7	·0142	62·9
11	5·7	5·65	39·1	·0160	62·5
12	4·9	5·0	34·1	·0167	59·9
13	4·4	4·4	29·7	·0180	55·6
14	3·6	3·9	25·8	·0184	54·3
15	3·3	3·4	22·4	·0184	54·3
28½	22·4	22·4			

and so it is probably due to change in the permeability of the rod. Hence by this method we arrive at the same results as by a more direct and exact method.* But by this means we are able to prove in the most unequivocal manner that *magnetic permeability is a function of the magnetization of the iron and not of the magnetizing force.* Hence it is for this reason that I have preferred in my papers on "Magnetic Permeability" to consider it in this way in the formulæ and also in the plots, while Dr. Stoletow in his paper (Phil. Mag., Jan., 1873) plots the magnetizing-function as a function of the magnetizing-force.

When we plot the results in this table with reference to Q' and $R' \alpha \mu$, the effect of the variation of R' is apparent; and we see, on comparing the curve with those given in my paper above referred to, that R' increases as L increases, at least between $L = 2$ and $L = 8$, which is as we should suppose, from the arrangement of the apparatus. For this table I happen to have data for determining Q in absolute measure, and these show that the maximum value of μ should be about where the table shows it to be.

This method of finding the variation of μ is analogous to that of finding conductivity for heat by raising the temperature of one end of a bar and noting the distribution of heat over the bar; and indeed the curves of distribution are nearly the same in the two cases.

If it were thought worth while, it would be very easy to obtain a curve of magnetic distribution for a rod and then enclose the whole rod in a helix and determine its curve of permeability.

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* Phil. Mag., August, 1873.

This would give data for determining R′ in absolute measure at every point of the rod.

To complete the argument that the variation of r^2 is in great measure due to that of μ, I have caused the magnetizing-force on a bar to vary. Tables V, VI, and VII. are from a bar 9

TABLE V. Magnetizing current ·176.

L.	Q'_ε. Observed.	Q'_ε. Corrected.	Q'. Corrected.	$r^2=\frac{R}{R'}$.	$\frac{1}{r^2}=\frac{R'}{R}$.	Q'_ε. Calculated.
0	2·7					0
1	6·9					2·40
2	12·7	----	----	----	----	7·32
3	18·2	----	----	----	----	12·54
4	24·4	----	----	----	----	18·31
5	32·4	31·7	220·5	----	----	24·87
6	31·5	32·0	188·5	·0190	52·4	32·38
7	28·2	28·2	160·3	0212	47·2	$Q'_\varepsilon=3{\cdot}34(10^{\cdot 08(b-x)}-10^{\cdot 08x})$.
8	24·9	24·7	135·6	·0218	45·9	
9	21·4	21·7	113·9	·0236	42·4	
10	18·6	19·0	94·9	·0252	39·7	
11	16·8	16·4	78·5	·0298	36·0	
12	14·2	14·2	64·3	·0311	32·2	
13	12·0	12·0	52·3	·0367	27·2	
14		10·0	42·3	·0404	24·8	
15	17·7	8·2	34·1	·0440	22·7	
16		6·6	27·5	·0445	22·5	
17	11·6	5·1	22·4	·0570	17·5	
18		22·4				
End.	22·4					

TABLE VI. Magnetizing current ·31.

L.	Q'_ε. Observed.	Q'_ε. Corrected.	Q'. Corrected.	r^2.	$\frac{1}{r^2}$.	Q'_ε. Calculated.
0	16.3					0
2	22·0					17·3
3	32·4	----	----	----	----	22·3
4	43·8	----	----	----	----	32·28
5	55·9	----	----	----	----	43·34
6	55·2	----	391·9	----	----	55·90
7	46·8	55·1	336·8			$Q'_\varepsilon=7{\cdot}74(10^{\cdot 08(b-x)}-10^{\cdot 08x})$.
8		48·1	288·7	·0204	49·0	
9	81·3	42·3	246·4	·0201	49·7	
10		37·4	209·0	·0202	49·5	
11	61·8	33·0	176·0	·0220	45·5	
12		29·0	147·0	·0243	41·2	
13	46·4	25·3	121·7	·0262	38·2	
14		21·9	99·8	·0300	33·3	
15	35·4	18·7	81·1	·0352	28·4	
16		15·6	65·5	·0405	24·7	
17	22·0	12·7	52·8	·0479	20·9	
18		9·8				
End.	43·0					

feet long and ·25 inch diameter. At the center a single layer of fine wire was wound for a distance of 1 foot, and the current for magnetizing the bar was sent through this. The zero-point was at the center of this helix and at the center of the bar, so that the observations on the first 6 inches include the part of the bar covered by the helix.

The values of Q'_ε are the sum of four observations on each end of the bar and with the current reversed. The three tables are comparable with each other, the same arbitrary unit being used for all.

TABLE VII. **Magnetizing current 1·12.**

L.	Q'_ε. Observed.	Q'_ε. Corrected.	Q'. Corrected.	r^2.	$\frac{1}{r^2}$.	Q'_ε. Calculated.
0	3.5		762·4			0
1	9·4	----	758·9	----	----	8·58
2	15·4	----	749·5	----	----	8·29
3	27·5	----	734·1	----	----	15·78
4	44·3	----	706·6	----	----	26·70
5	66·6	----	662·3	----	----	43·36
6	71·2	71·2	595·7	----	----	69·37
7	59·5	59·7	524·5	·0239	41·8	
8	51·0	51·2	464·8	·0200	50·0	
9	45·2	45·2	413·6	·0162	61·7	$Q'_\varepsilon = \cdot 35(10^{\cdot 26(6-\varepsilon)} - 10^{\varepsilon})$.
10	40·3	40·3	368·4	·0141	70·9	
11	36·3	36·8	328·1	·0120	83·3	
12	33·3	33·5	291·3	·0107	93·5	
13	30·6	30·5	257·8	·0110	90·9	
14	28·1	28·0	227·3	·0116	86·2	
15	25·6	25·4	199·3	·0118	84·7	
16	23·4	22·7	173·9	·0140	71·4	
17	20·0	20·3	151·2	·0147	68·0	
18	34·0	18·1	130·2	·0161	62·1	
19		16·0	112·8	·0180	55·6	
20			96·8			
End.	96·8					

Here we see an excellent confirmation of the results deduced from Table IV. In Table V, where the magnetizing-force is very small, and where, consequently, no part of the iron has yet reached its minimum resistance, the value of $\frac{1}{r^2}=\frac{R'}{R}=R'\alpha\mu$ decreases continually as the value of Q' decreases, as it should do. In Table VI, with a higher magnetizing-power, which was sufficient to bring a portion of the bar to about the minimum resistance, we see that $\frac{1}{r^2}$ remains nearly stationary for a short distance from the helix and then decreases in value. In Table VII, where the bar is highly magnetized and the portion near the zero-point approaches the maximum of magnetization, $\frac{1}{r^2}$ increases in value as we pass down the bar, and having reached its maximum at $L = 11\frac{3}{4}$ nearly, it decreases. These tables, then, show in the most striking manner the effect of the variation of the magnetic permeability of iron upon the distribution of magnetism.

It is evident that these tables also give the data for obtaining the relative values of R' at different parts of the bar; but the results thus obtained are conflicting, and will need further experiment to obtain accurate results. Where such a small magnetizing force is used as in Table V. it is almost impossible to attain accuracy, and allowance should be made for this in deducing results from it. The greatest liability to error is of course where the magnetization is small; for any small residual magnetism which the bar may contain will be more apparent here, although great care was taken to remove all residual magnetism before use. Besides this there are many other disturbances from which the higher magnetizing-powers are free.

If we accept Green's formula as correct, *these observations give us data for determining the magnetizing-function of iron in a unique manner*, for nearly all other methods depend on absolute measurements of some kind. Thus the least value of r^2 in Table IV. for a rod ·19 inch diameter is ·0142, which gives $p = {\cdot}01132$, which in Green's formula (equation 8) gives $\mu = 3308$ for the greatest permeability of this iron; and this is as nearly right as we can judge for this kind of iron. It is to be noted that Green's formula has been found for the portion of the bar covered by the helix, but as seen from my formulæ it will approximately apply to all portions, though it would be better to find a new formula for each case.

We shall toward the last resume this subject again, and so we will leave it for the present.

The results which I have now given, and indeed all the results of this paper, have been deduced not only from the observations which I publish, but from very many others; so that my Tables may be *considered to represent the average* of a very extended series of researches, though they are not really so.

[To be continued.]

ART. LVI.—*Notes on a New Feature in the "Comstock Lode;"* by G. P. BECKER. Ph.D., etc.

THERE is probably no metalliferous vein in the world which excites a greater interest than the "Comstock Lode" of Nevada, whether regarded from a scientific point of view, as the largest and richest argentiferous vein yet discovered, or from an economical standpoint, as the source for many years past of an immense proportion of the silver, and a considerable fraction of the gold added to the circulation of the world; and the details of its geological character as developed, therefore acquire an importance not possessed by those of less prominent deposits.

The Virginia Range is a due north and south branch of the Sierra Nevada system of mountains. On the eastern slope of Mt. Davidson, the chief peak of the range, were found the outcroppings of the vein which have been followed somewhat to the north, and a long distance to the south of that mountain, and shown to possess a total length of four miles in the general direction of the Virginia Range. Decidedly the most important portion of the Lode—that in which the celebrated "Gould and Curry," "Hale and Norcross," "Ophir," "Consolidated Virginia" and "California" mines as well as others have been opened—lies at the base of Mt. Davidson, and is included within the limits of Virginia City.

It is well known to those who have had occasion to make themselves familiar with the Comstock lode* that throughout that portion of the vein which lies in Virginia City, the west or foot wall is a continuation of Mount Davidson, and consists, like the mass of that mountain, of syenite, while the east wall is a porphyry, more nearly described as trachytic greenstone, or propylite, with which the country was overflowed during the Tertiary period. The direction of the fissure subsequently filled by the vein matter of the Comstock, was plainly determined by the previously existing surface of contact between the syenite and propylite, which naturally offered less resistance to rupture than the solid mass of either adjoining rock. This is clear from the fact that for over a thousand feet from the surface, only insignificant masses of either rock were found on the side of the vein opposite to that of which they are especially characteristic. So strong was the influence of the direction, both in strike and in dip, given to the fissure by the presence of this comparatively weak surface of contact in the rocky mass, that the fissure extending in both directions away from Mount Davidson into solid propylite, unaccompanied by syenite, retained the strike, (nearly north and south,) and the dip (from 35° to 50°,) of the Virginia portion, almost unaltered.

This very clearly defined influence of the contact surface, was of itself good reason for supposing that the fissure in Virginia must follow the dividing plane between the syenite and propylite to a very considerable depth, and not improbably to one beyond the reach of mining operations. Nevertheless, although the subject has been very wisely left in abeyance by the geologists who have publicly discussed the Comstock, it cannot have escaped any of them that the fissure could not indefinitely follow this contact; for the propylite is recognized by them as a volcanic rock which has overflowed the older syenite, and therefore, unless we should make the highly improbable assumption that the syenite, (which in Virginia is plutonic,) the propylite, and the vein matter emanated successively from a single fissure in the earth's crust, a lowest point in the contact between the underlying syenite and the overlying propylite must eventually occur, from which the fissure would naturally descend toward the seat of the force by which it was caused. From the most depressed point in the contact surface, the fissure would pass into the underlying, or syenite rock, and doubtless out of it farther below, into whatever might underlie it in its turn.

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* **Vide von Richthofen, "The Comstock Lode, San Francisco, 1865," and "U. S. Geological Exploration of the 40th Parallel," Vol. III.**

From the point where the fissure leaves the contact between the two species of rock, its dip would in all probability be controlled by circumstances very different from those which decided it in the position lying near the croppings; for from the point at which the seismic shock impinged upon the lower surface of the syenite, the fracture must have followed the direction of least resistance, or presumably a straight line, and the shortest line from the point in which the subterranean force encountered the syenite, to the upper surface of that formation; and a moment's thought will serve to show that, apart from local and altogether incalculable irregularities in the lower surface of the rock, this line could continue in even approximately the direction of the present dip, only in case the syenite not only formed a cone above the general level of the country, but filled a vast depression below it as well. The passage of the lode into the syenite would therefore naturally be accompanied by a change in dip, toward the perpendicular.

In May last I made observations, repeated and extended in August, which seem to establish beyond question the hitherto unnoticed fact that the lower workings of the mines in Virginia City, have reached the point at which the fissure passes from the contact surface between the syenite of Mt. Davidson and the country propylite, into the underlying syenite. The depth below the surface at which the change occurs is not very far from 1500 feet.

The details of these observations would scarcely be of general interest away from the Pacific Coast, while the conclusion drawn will probably be considered as amply justified by the following general results of examination, viz: The east wall of the lode in the lower levels (1400–2300 feet) wherever struck between the north end of the "Ophir" and the south end of the "Chollar Potosi," a distance of over a mile, is syenite; that in the three mines, of the eight within these limits, in which for fear of water the East

Clay has not been pierced, good grounds (presence of included syenite and absence of included propylite) exist for believing that the hanging wall is syenite:—that although the east country syenite has been pierced in a number of places, both vertically and horizontally, to a depth of several hundred feet, no other formation has been reached, nor any indication that the limits of this have been approached; on the contrary, this eastern syenite is apparently as solid as Mount Davidson itself:—that wherever the relations between the walls of the fissure have been most completely exposed, the occurrence of syenite on the east wall is accompanied by a very decided increase in the angle of dip.

The last point is most clearly shown in the Ophir, where on the 1700 feet level the walls are almost exactly perpendicular, as shown by the very complete prospecting on that level.

The Gold Hill mines seem nowhere to have reached the syenite, as was to be expected, since from the conformation of the country, the syenite, if it underlies the porphyry in Gold Hill as well as in Virginia, will probably be met with only at a much greater depth.

To establish the exact line of the passage of the vein from the propylite into the syenite, would of course require a specially authorized examination of the mines; since a majority of points in which the east wall has been struck are hidden from view, either by the closure of drifts no longer essential to the working of the mines, or by timbering in the shafts, etc.

Many important deductions might be based upon a change in the conditions of the Comstock lode, which must be fraught with exceedingly important consequences to the greatest mining interest of the Pacific coast; but I prefer to confine myself to submitting the fact of this remarkable, and at this time most unexpected, alteration in the character of the vein.

Berkley, California, October, 1875.

ART. LVII.—*Notice of New and Interesting Coal Plants;* by E. B. ANDREWS.

[Read at the Detroit Meeting of the American Association.]

Two or three years ago, I noticed in a ditch by the roadside in the western part of Perry county, Ohio, a thin layer of bituminous shale. Its stratigraphical position is near the base of the Ohio Coal-measures, perhaps thirty feet above the Maxville limestone, the Ohio equivalent and representative of the Chester limestone of Illinois. A few strokes of the hammer revealed a fragment of a coal plant entirely new to me. This led to subsequent visits to the locality and the discovery of a large number of new forms of ancient vegetation. At the bottom of the layer is found less than an inch of a very peculiar substance of vegetable origin of brown color, soft like rotten wood, without lamination and filled with fragments of a minute form of plant resembling an *Asterophyllites.* In it are fish scales indicating, according to Dr. Newberry, a new genus and species, and also a small *Lingula,* perhaps too indistinct for specific determination. Above the half-solidified brown band we find an inch of highly bituminous laminated cannel shale, presenting a satin surface in its fracture. This shale contains a few plants, the most numerous form being leaves of *Lepidodendron.* This shale passes upward into an ordinary bituminous shale, in the lower two inches of which nearly all the new plants are found.

We have here the evidence of a marsh in which there was acccumulated upon a micaceous sandy bottom the minute moss-like calamitoid plants. These were buried by a highly bituminous sediment into which fell innumerable leaves of *Lepidodendra,* long straight leaves, often a foot in length. Then came in a luxuriant vegetation, chiefly ferns, which are buried in the higher shale.

So far as I know, this marsh must have been of very limited extent, for I have found no trace of it in the same geological horizon elsewhere in the neighborhood. The shale, on its outcrop, is so deeply buried by soil that so far only a few square yards have been uncovered and examined.

In this little marsh grew plants of well-marked Devonian types, and others of a type generally found in formations more recent than the Coal-measures. Besides these, there are many Coal-measure forms, but with scarcely an exception, they are of new species.

Of the Devonian types, one is a new species of *Archæopteris* Dawson, (*Palæopteris* Schimper.) The *A. Jacksoni* Dawson, from the Devonian of New Brunswick and Maine may be regarded as the typical form of this genus. The new Ohio species I have called *A. stricta.* It is a fern of great beauty, smaller and more delicate than *A. Jacksoni.* The pinnæ are alternate, somewhat closely set, growing out of the rachis at an angle of 70° to 80°, rarely as small as 45°. The pinnules are alternate, oblanceolate, obtuse, decurring on the narrow rachis, disconnected to the base, with a strong nerve, dividing near the base into three to five branches, which themselves fork once or twice before reaching the margin. Prof. Fontaine reports the finding of *A. Jacksoni* (or possibly a closely-allied species), in his conglomerate coal series on New River, W. Va., over a coal seam perhaps 500 feet above the base of the Coal-measures. This is more than 1500 feet lower in the series of the Alleghany

Coal-measures than my Ohio form. He also finds the same *A. Jacksoni* in Vespertine slate in Greenbrier Co., W. Va.

Another Devonian genus found in the little marsh in Perry county is the *Megalopteris* Dawson. The only species of this genus known was first found in New Brunswick by Prof. Hartt and described by him as *Neuropteris Dawsoni.* Prof. Fontaine has recently found the same in the lower Coal-measures on New River, where he found the *A. Jacksoni.* Of this genus I have found four new species, which I have called *Megalopteris Harttii*, *M. ovata*, *M. lata* and *M. minima.* The first three of these are closely allied to *M. Dawsoni* Hartt. The last, *M. minima*, has much the appearance of an *Alethopteris*, but it is undoubtedly a true *Megalopteris.* Some of the new species were ferns of large size and of great beauty. This genus has special interest and value because found in the Coal-measures of Ohio, only about 700 feet below the great Pittsburgh seam of coal. The West Virginia species found by Prof. Fontaine came from a horizon more than 1500 feet lower. But from this horizon, which is low in the original geosynclinal trough or depression in West Virginia (described by me in another paper read at Detroit and published in this Journal for Oct.), it is a long way down to the Devonian formation, which, in New Brunswick, yielded to the hammer of Prof. Hartt his *Megalopteris Dawsoni.* The new species from Ohio are directly separated from any Ohio Devonian rocks by, first, a few feet of Coal-measure strata, second, by the lower Carboniferous Maxville limestone, the equivalent and representative of the Chester limestone of Illinois, there 600 to 800 feet thick, and third, by the Waverly sandstone, over 600 feet thick. These facts are of no little interest as showing the distribution in the great time-series of this genus of ferns. If, however, we should apply the republican rule of majorities to science, the finding of several species in the Coal-measures and but one in the Devonian would determine this to be a true Coal-measure form. Then the Devonian species becomes the prophetic one.

Growing in our little marsh with the *Archæopteris* and the *Megalopteris* is a new and beautiful fern of a new genus of a more recent type—the *Tæniopteridæ.* No form of this order has hitherto been found so low in the Coal-measures. *Tæniopteris multinervis* Weiss, belongs to the upper Coal-measures and Permian of Europe, but most ferns of this order are of Mesozoic age. The only American representatives of this group, so far as I know, are *T. magnifolia* Rogers, from the Triassic coal-field near Richmond, Va., used with other plants by Prof. W. B. Rogers to determine the geological age of that field, and *T. vittata* Brongt., reported by Pres. Hitchcock, from the Red Sandstone of the Connecticut valley. The new Ohio genus I have called *Orthogoniopteris*, from its rectangular nervation. The following is a description of the genus:

Frond simply pinnate. Pinnæ alternate, lanceolate or oblong, linear, rounded or tapering to an acute point, entire or undulate, enlarged and decurrent on lower side, rounded on the upper to the median nerve, and joining it a little above the point of its attachment to the rachis. Median nerve prominent, thick and ascending to the apex. Nervules forking once very near the median nerve, extending at right angles to it, and curving upward slightly at the margin, very fine, numerous and uniform.

Two species have been found, *O. clara* and *O. Gilberti*. This genus is allied to *Tæniopteris* Brongt., to *Angiopteridium* Sch., and to *Neriopteris* Newb. The nervation is similar to *Tæniopteris*, but *Tæniopteris* has a simple frond, while this is pinnate. In *Angiopteridium* the frond is pinnate and the pinnæ are cordate or rounded, with a marginal fructification. In Dr. Newberry's new genus, *Neriopteris*, the pinnæ are similarly cordate with acute-angled nervation and with a supposed marginal fructification. In this new genus the pinnæ are decurrent below, and free and rounded above, with a perfectly rectangular nervation. In the decurrent base of the leaflets it is allied to the larger forms of *Alethopteris*, but it doubtless belongs to the order of *Tæniopterideæ*.

Besides those already referred to, the bed affords a new species of *Alethopteris* greatly resembling *A. tæniopteroides* Bunbury, from the coal field of Cape Breton, but specifically different. Another fragment of a supposed *Alethopteris* also possesses a nervation much like the *Tæniopterideæ*. If a true *Alethopteris*, it is of larger leaf than any other species of this genus except *A. ingens* Dawson, from which it differs somewhat in nervation. There is another *Alethopteris*, which I have called *A. Holdeni*, of great size and beauty, belonging to the section of the genus which may be represented by *A. Serlii*, and is doubtless allied to Dr. Newberry's new species, *A. macrophylla*. The essential characters which distinguish it are: first, the great length of the frond, which measures at least fifty centimeters; second, its lanceolate, or rather oblanceolate form, the leaflets decreasing in length toward the base; third, the linear taper-pointed form of the leaflets, comparatively long and narrow; and fourth, the always simple division of the frond. It is possible that we have in these various species of *Alethopteris*, and in closely allied forms, a group which should be detached and formed into a new genus. This group passes, in resemblance, into the type of the *Tæniopterideæ* on the one hand and through the *Megalopteris* (the *M. minima* having much the look of an *Alethopteris*) unto the distinct *Neuropteris* type on the other. These resemblances are of great interest and seem to bring the Devonian and Mesozoic flora into greater apparent harmony.

Besides the forms already mentioned the bed affords two new species of *Asterophyllites*, a new and beautiful *Hymenophyllites*, a new and pretty *Eremopteris*, two species of *Lepidodendron*—one allied to *L. tetragonum* Steenb., found by Dr. Dawson in the lower Carboniferous of Canada—two species of *Cordaites*, one probably *C. Robbii* Dawson, a Devonian form from New Brunswick, a new and large *Cardiocarpon*, a large fine cone of somewhat doubtful affinities, a curious jointed root and several other forms not yet determined. The most important of these plants are figured and will appear in the next volume of the Ohio Geological Reports.*

ART. LVIII.—*Letter to the Editors*, from Dr. B. A. GOULD, Director of the Córdoba Observatory, dated Córdoba, Sept. 8, 1875.

MESSRS. DANA & SILLIMAN:—When I took leave of you last November I did not intend that so long an interval should elapse without the fulfillment of my promise to send you tidings of the progress of my work. But what with the accumulation of material during my absence, and the relentless pressure of the current observations, I have been unable to do more than keep pace with the stream until now.

But now I have the gratification of informing you that the zone-observations are completed. There are 754 zones, containing somewhat more than 105,000 observations; the first zone having been observed 1872, Sept. 9, and the last 1875, Aug. 9, but as there was an interruption of about a year during this interval, not quite two years of work is comprised in the series. The measurement of the chronograph sheets and the transcription of the time-records is now completed; and the copy of the observations upon sheets ready for the printer is so far advanced that it will probably be completed before this letter can reach you. The second copy, upon computation-blanks, is likewise well under way.

Thus the entire region from 23° to 80° of South declination has been well scrutinized. The ten degrees around the pole have been so thoroughly examined by Gillis in Santiago and Stone at the Cape of Good Hope, that no zone observations seem needful there; and my northern limit overlaps Argelander's southern zones by eight degrees, in compliance with the special request of that deeply lamented astronomer.

The width of the zones has varied with their declination, and with the abundance of their stars; the maximum width being 2° at the northern limit, and 4° for the zones immediately north of

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* Professor Andrews has sent us copies of his plates, and we can testify to their beauty and interest.—EDS.

75°, between which declination and 80° only one series was taken. These widths were always halved within the limits of the Milky Way, and still farther subdivided into quarters, and even eighths, where the richness of the region required; so that some zones have only comprised 20′ or 25′ of declination exclusive of their marginal overlap. The maximum number of stars contained in a single zone is 293, the average is about 140. It is needless to say that there are many repetitions, arising not only from the overlap, (usually 10′ at each margin), and five or six minutes of time at the beginning and end), but also from the re-observation of such zones as had proved unsatisfactory, whether from unfavorableness of weather, insufficient determining stars, or inadequate performance of circle, chronograph or clock.

It has been my unfailing rule to make the determinations as nearly absolute as circumstances would allow; and with this view, each zone has been preceded and followed by a series of observations for instrumental corrections,—consisting of transits of two standard time-stars, as well as of one circumpolar star above and one below the pole, together with measurements of nadir, collimation and level. But although these precautions have been taken, I do not purpose dispensing with the advantage to be derived from direct comparison with independent and sharply determined star-positions, and the catalogue of these is advancing very satisfactorily. My original plan implied the determination of some half dozen stars in each zone, selecting so far as it might be convenient, such as occur in the Uranometry. These would form a catalogue of about 3,000 stars, each of which was to be observed four or five times with all possible care. But when the zone-work was interrupted by my departure for home in 1874, I extended the list considerably, and thanks to the faithful and efficient labors of my assistants, this work was vigorously prosecuted during the year; so that there now remain comparatively few stars of the Uranometry, whose positions have not been independently determined in Córdoba. I purpose to employ the Meridian Circle for extending and improving this catalogue, while the reduction of the zone-observations, and the publication of the Uranometry are going forward, and have now extended the working list yet more, so that this catalogue will contain more than ten thousand stars, if all goes well, each star being observed on the average about four times. Up to the present time the total amount of different stars observed for this catalogue exceeds 7,000, although not all of them have as yet the requisite number of observations.

Besides the data thus collected I have a considerable amount of material resulting from the repeated observation of clusters. These observations, when properly reduced, will essentially aid the study of the photographic plates.

Now I must apply myself to the more laborious and difficult part of the work—the computation and preparation of the crude material obtained. This would be more difficult everywhere as requiring a more unremitting diligence, and careful organization, but is doubly so in this country, where to the task of discovering persons fit to serve as computers, is added the labor of training them; and the dull routine of numerical computation naturally fails to enlist that popular appreciation and interest, which the process of instrumental exploration readily awakens, and in which the sinews of science must find their point of attachment. Still, I have every reason for confiding in the support of the Argentine people and government, which have never yet failed to respond favorably to any of my appeals. At the present moment the country is suffering from the severest commercial crisis which it has ever experienced; its treasury is depleted by last year's civil war, the agricultural industries of the whole land have been blighted by the ravages of locusts, and an almost unexampled drought; yet although almost all other appropriations have been cut down, those for scientific purposes have been maintained intact, and all which I thought it right to ask for the Observatory has been voted by the Congress.

Now, first of all, I hope to prepare the manuscript of the Uranometry, for which the charts are already begun; and while this is going forward I hope to prosecute various other subsidiary work. The reduction of the catalogue-observations is so well forward that within a couple of months after I shall have completed the recomputation of the circumpolar list (already far advanced), I think that the current observations for the catalogue may be kept reduced up to date. And very soon after the manuscript of the Uranometry is completed I think that at least one volume of instrumental observations can be sent to the press.

You will be glad to learn that after all the labor, vexation and expense, which my first photographic efforts had entailed, far better success is attending the second endeavor. The new lens seems to be quite equal to the original one, and on favorable nights we obtain images of stars which cannot be brighter than the 8½ magnitude. Although the clock-work of the Equatorial is not equal to the demands upon it, the mechanical ingenuity and perseverance of the assistants has already done much, and will, I am confident, do yet more toward remedying the defects. And the lathe and tools for working in metals, which I brought out with me last December, have rendered most important service. Mr. Mansfield is now constructing a new governor, which will, we hope, secure a greater uniformity of movement to the telescope. Mr. Heard, who is now engaged on the photographic work, is far more successful than his predecessor in the manipulation both of the telescope and the chemicals, and has already, in spite of many difficulties, obtained about seventy good plates, comprising twenty-four different clusters and many double stars. On one plate there are nearly or quite a hundred stars, in the cluster Δ.323. We have also been experimenting upon *Mars*, and the photographs show the detail of the surface quite clearly, but they are not as well defined as could be wished. I have now sent home orders for new enlarging lenses, for which, and for other photographic conveniences, the Minister of Public Instruction has placed a small sum of money at my disposal from the contingent fund of his department. When I next write I think it will be possible to give you some encouraging accounts, and description of results in detail. As yet I have had no time to undertake any measurements of the plates, but if my plans do not prove amiss, this will soon be possible. I possess an exquisite micrometer for the purpose, the gift as well as the construction of our friend, Mr. Rutherfurd. I have been endeavoring to procure photographic impressions of *Eurydice*, and its comparison stars, for Dr. Galle's new investigations, but the stars are too faint.

The geographical determinations of various points of South America are going forward as opportunity permits. Since my return I have obtained satisfactory exchanges of signals with Santiago de Chile on two nights, and am waiting an additional exchange before deducing the final results. Messrs. Thome & Bigelow are now resting themselves from long continued labor at the Observatory by a journey of geographical exploration up the river Paraná. They carry with them a prismatic reflecting circle, which I obtained for such purposes from Pistor and Martius, and in the use of which they have already attained such proficiency that their respective time-determinations frequently agree to the tenth of a second. The sidereal clock of the Observatory, being connected with the telegraph line, is made to graduate a time-scale on the registers of both stations, while the observers telegraph the beats of a mean-time chronometer, for which they determine the error immediately before and after these signals. The position of the city of Paraná was thus determined three days ago; to-day we have accomplished the same for La Paz, which is about 100 miles farther up the river. The telegraphic communication only extends as far as Corrientes in 27½°, but with the aid of their chronometer I trust they will be able to extend the longitude determinations as far as Ascencion, the capital of Paraguay, latitude 25° 16′. I have also made arrangements for an early determination of the positions of Tucuman, situated 260 miles north of Córdoba, and of San Juan at the base of the Andes, some 200 miles to the westward. You will remember that distances form no criterion of ease of access. About eight days are usually required for the journey to Tucuman, notwithstanding that the railroad now building is already nominally completed for half the distance, and the same is true in regard to San Juan.

In Dr. Petermann's new map of the Argentine Republic, just published, the results of my previous observations are incorporated, having been furnished him by Mr. Moneta, late Chief of the National Engineers, in connection with whom my determinations of Rosario and Buenos Aires were made, as I once wrote you. Before long I hope to be able to include in this series of determinations all the capitals of provinces, and the chief points of commercial or geographical importance, to which the telegraph extends.

The Meteorological office is quietly extending its influence and activity throughout the country. It is not easy to secure the gratuitous coöperation of competent and conscientious observers, nor to secure their reception of instruments in good condition; yet the success has thus far been all that I could have presumed to hope; and I am already rapidly accumulating data, which will disclose the heretofore unknown climatic and atmospheric relations of this vast country. Already some dozen or more stations are regularly transmitting their tri-daily observations, and arrangements are already in progress which give ground for confident expectation that this number will soon be doubled. Especially I have reason to believe that systematic observations will speedily be organized along the river Uruguay, in Patagonia, Tierra del Fuego, and the Falkland Islands. The most extensive railroad company in the province of Buenos Aires, and the provincial telegraph have promised their coöperation by establishing observations at their chief stations, and the Right Reverend Dr. Stirling, the English missionary Bishop of the Falkland Islands, has cordially interested himself in our undertaking, and will avail himself of the opportunities afforded by his official journeys to all the Anglican missions in this portion of the world to enlist the assistance of his clergy.

Before bringing this letter to a close I must add one more item of interest, viz., that the two Houses of the Argentine Congress have by decided majorities adopted the metric dollar of one and a half grams of pure gold, or one and two-thirds grams of alloy, nine-tenths fine, as the national monetary unit, the legal value of foreign gold coins to be determined on this basis by assays, and annually published by the Minister of Finance. That this unit is not already law is due solely to disagreement between the two houses on other points contained in the same bill; but the delay is only temporary.

Now that Japan and the Argentine Republic have adopted a metric basis for their monetary unit,—the only basis on which we can hope for any approach to international unification,—perhaps we may look forward to some corresponding action on the part of our own government. The United States dollar requires only a change of less than one-third part of one cent, (less than the limits of mint tolerance), and it would seem as though by the united action of our men of science, this desirable change might be effected during the present demonetization of gold. Such an act would be a most notable addition to our series of centennary celebrations, as it would be a most efficient step toward what the French term "the solidarity of nations."

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *An Air Damper for Balances.*—In order to reduce the time of oscillation of a balance to a minimum when in use, Arzberger has proposed the use of a damping apparatus containing air. To the stirrup, immediately under the end of the beam, a circular plate of brass, gilded, is hung by a wire. This plate is 67 mm. in diameter and ½ mm. thick. It swings, when the balance oscillates, in a short cylinder 68 mm. in diameter and thus has a clear annular space of ½ mm. between it and the cylinder. This latter is fastened to the balance case by a strip of brass, to which it is attached by a screw in the center. The bottom of this cylinder has a hole on one side of this screw; so that by rotating the cylinder, the hole may be closed more or less completely by the brass strip. When the balance swings, the plate moves up and down in the cylinder, the oscillation being checked by the resistance of the air in the box, which may be adjusted at pleasure. The working of the attachment is said to be very satisfactory, since the damping is a dynamic and not a static resistance. The delicacy is not at all interfered with.—*Liebig's Annalen*, clxxviii, 382, Oct., 1875. G. F. B.

2. *On a Convenient method of Preparing Sulphuryl Chloride.*—Behrend, in order to investigate the relations of the carbamides and sulphamides, found it necessary to devise some better method for preparing sulphuryl chloride. For this purpose he prepared sulphuryl chlorhydrin by the action of hydrogen chloride upon sulphuric oxide, and distilled this continuously with a return cooler; but with no satisfactory result. It was then placed in sealed tubes and heated for 12 to 14 hours in a paraffin bath to 170°–180°. A brilliant mobile liquid, boiling at 70°, and with an exceedingly irritating odor, was obtained, which had a specific gravity of 1·661 and was the body in question. Its production is thus represented:—

$$\begin{matrix}\mathrm{SO_2}\left\{\begin{matrix}\mathrm{OH}\\ \mathrm{Cl}\end{matrix}\right.\\ \mathrm{SO_2}\left\{\begin{matrix}\mathrm{Cl}\\ \mathrm{OH}\end{matrix}\right.\end{matrix} = \mathrm{SO_2Cl_2} + \mathrm{SO_2}\left\{\begin{matrix}\mathrm{OH}\\ \mathrm{OH.}\end{matrix}\right.$$

—*Ber. Berl. Chem. Ges.*, viii, 1004, Sept., 1875. G. F. B.

3. *Action of dilute Mineral Acids on Bleaching Powder.*—Kopfer, under Schorlemmer's direction, has studied the action of dilute mineral acids on bleaching powder, with a view to ascertain whether hypochlorous acid is actually evolved as asserted by Gay Lussac and recently denied by Göpner, and if so, in what amount. The bleaching powder was prepared from pure slaked lime and chlorine, and contained 21·46 per cent calcium hypochlorite, 17·69 calcium chloride, 47·52 calcium hydrate, and 13·33 calcium oxide. A solution of 20 grams of this was made and

filtered. Two hundred cubic centimeters of the filtrate were taken and diluted to a liter, and the hypochlorite was determined by Bunsen's method in a portion of the solution. Other portions were then distilled with nitric, with hydrochloric and with sulphuric acids. The best result was obtained with the first of these acids, the chlorine monoxide (hypochlorous oxide) in the filtrate varying from 80·63 to 91·32 per cent of that originally present in the bleaching-powder solution. Hence the author concludes that when bleaching powder is distilled with dilute mineral acids an aqueous solution of almost pure hypochlorous oxide is obtained in amount rising even to 92 per cent of that in the powder. Hence he infers that bleaching powder contains either calcium hypochlorite with calcium chloride, or a compound of the constitution $Ca\left\{\begin{matrix}OCl\\Cl\end{matrix}\right.$, as proposed by Odling. The liberation of hypochlorous oxide from bleaching powder by chlorine, is strongly confirmatory of the latter view. Chlorine is not set free by the action of hydrochloric acid upon bleaching powder.—*Jour. Chem. Soc.*, II, xiii, 713, August, 1875. G. F. B.

4. *Method for the Quantitative Separation of Ferric Oxide, Alumina, and Phosphoric Acid.*—Having occasion to analyze a minute quantity of a mineral containing ferric oxide, alumina, and phosphoric acid, FLIGHT took the opportunity to examine the various methods in use for the separation of these substances, the results of which are given in his paper. As a result, he was led finally to adopt the following, which gave all desirable accuracy: The solution of the above named substances, which must not contain much free acid, is boiled for two or three hours with an excess of sodium hyposulphite (thiosulphate) and filtered. All the iron with some of the phosphoric acid is in the filtrate; all the alumina with some of the acid, is on the filter. The iron in the filtrate is thrown down by ammonium sulphide and determined as usual. The alumina precipitate is dissolved, and to the solution an excess of sodium hydrate is added, and then barium chloride so long as a precipitate is produced. After standing a few hours, the solution is to be filtered. The phosphoric acid remains on the filter as barium phosphate; the alumina is contained in the filtrate, from which it may be precipitated in the ordinary way and weighed. A little caustic soda added to the wash water prevents the solution of the barium phosphate. The barium phosphate is dissolved off the filter, the barium removed with sulphuric acid, the filtrate mixed with that from the iron sulphide above, and the phosphoric acid determined with magnesia mixture as usual. The author also points out that the corrosive action even of dilute solution of sodium phosphate upon the glass of reagent bottles is considerable and may be a source of error in analysis. This salt cannot be obtained pure by recrystallization in dishes of Berlin porcelain from this cause.—*Jour. Chem. Soc.*, II, xiii, 592, July, 1875. G. F. B.

5. *On the Gases enclosed in Coals.*—THOMAS has made an elaborate investigation of the gases occluded in coal as well as of

those which are evolved from fissures in the mines. The experiments were made upon the bituminous, or "house coals;" the semi-bituminous or steam coals; and the anthracite coals; all from the South Wales basin. In the first series, slices of coal were sawn from the middle of large cubes and a strip cut from the middle of each of these ⅝ of an inch square and 6 or 8 inches long. This was placed in a hard glass tube and connected with a Sprengel pump. Very little gas—only 2 or 3 cc. per 100 grams of coal—was liberated on exhaustion. The tube was then immersed in boiling water and kept there so long as gas was evolved. Additional quantities of gas were obtained at 200° and even at 300°. Upon analysis, the gases were found to consist of marsh gas, carbon dioxide, nitrogen and oxygen. The bituminous coals yield the least gas, though it contains a large percentage of carbon dioxide. Steam coals come next, their gas containing as much as 87 per cent of marsh gas. Anthracite yields most gas, one specimen giving 600 cc. of gas from 100 grams of coal, at 100°, 1000 cc. at 200°, and 1875 cc. in all at 300°. In composition these gases closely resembled those from steam coals. The gases collected from blowers in the mine as well as from the coal itself by boring, consisted almost entirely of marsh gas, rising in some cases to 97·65 per cent.—*Jour. Chem. Soc.*, II, xiii, Sept., 1875. G. F. B.

6. *On Chrysophanic Acid.*—**Liebermann** and **Fischer** have continued the researches made upon emodin by the first-named chemist, and have extended their investigations to chrysophanic acid. When distilled with zinc dust, it yields methyl-anthracene, as was proved by a minute examination of the properties and reactions of this hydrocarbon. Acetyl-chrysophanic acid was also pre-prepared and analyzed. It contains two acetyl groups. From these data it is clear that chrysophanic acid is dioxymethyl-anthraquinone and that it stands to emodin in the same relation that alizarin stands to purpurin. The authors notice the curious fact that while the madder-root contains anthraquinone-coloring matters which stand to each other in a simple oxidation series, the root of rhubarb contains a similar series, homologous with the derivatives of anthraquinone. Experiments were also made on the amides of chrysophanic acid.—*Ber. Berl. Chem. Ges.*, viii, 1102, Sept., 1675. G. F. B.

7. *On the Quantitative Determination of Vanillin in Vanilla.*—The introduction into commerce of the synthetically prepared vanillin renders some method desirable for its exact estimation. **Tiemann** and **Haarmann** have proposed a method founded on its aldehydic nature and its consequent union with acid sulphites. Thirty to 50 grams of the finely divided vanilla is repeatedly extracted with ether, the extracts united, the ether distilled nearly off, and the residue treated with an equal volume of a mixture of equal parts of hydro-sodium sulphite solution and water. On standing, two layers of liquid appear, which are separated by means of a separating funnel. The lower, which is the sulphite vanillin solution, is placed in a flask furnished with funnel and de-

livery tubes, through the former of which dilute sulphuric acid is poured, the evolved sulphurous oxide being conducted into a bottle containing soda. After the reaction is over, the liquid is extracted with ether, the ether distilled to a small bulk, the residue placed in a watch glass and allowed to evaporate spontaneously. Pure vanillin, fusing at 81°, crystallizes out; it is dried over sulphuric acid and weighed. After proving the exactness of the method, the authors used it in analyzing the vanillas of commerce. They found Mexican vanilla to yield 1·69 per cent, Bourbon vanilla 2·46 per cent, Java vanilla 2·75 per cent, and a second sample of Bourbon vanilla 1·91 per cent of vanillin. The prices of the various sorts of vanilla are even more discrepant.—*Ber. Berl. Chem. Ges.*, viii, 1115, Sept., 1875. G. F. B.

8. *On the Localization of Arsenic in the Tissues.*—Scolosuboff has investigated in Gautier's laboratory the distribution of the metalloid arsenic in the tissues and has found, contrary to the general belief, that it is specially condensed in the nervous tissues. The experiments were made with dogs, with rabbits, and with frogs. Dogs bear large doses of arsenic readily, being able to take without difficulty 15 to 18 times the quantity which weight for weight would be fatal to man. The arsenic was administered as sodium arsenite in the food. It was separated from the tissues by treating these with nitric mixed with a little sulphuric acid, evaporating nearly to dryness, adding more sulphuric acid, heating again till fumes appeared, then more nitric acid and heating to carbonization. On exhaustion with boiling water and filtering a liquid is obtained from which the arsenic may be precipitated by hydrogen sulphide. A bull dog took for 34 days gradually increasing quantities of arsenic from 5 milligrams to 150 milligrams per day. 100 grams of the muscle contained ·00025 arsenic, 100 grams liver ·00271, 100 grams brain ·00885, and 100 grams spinal cord ·00933 grams. Calling the quantity in 100 grams of muscle 1, that in the same weight of liver is 10·8, brain 36·5 and spinal cord 37·3. The results where the poisoning was acute were quite as marked. A dog of 11 kilograms was killed by the sub-cutaneous injection of sodium arsenite, in 17 hours. The arsenic from the brain gave a decided reaction, that from the cord was less, while for the liver and the muscles, only traces could be detected. The toxical importance of these facts is obvious.—*Bull. Soc. Ch.*, III, xxiv, 124, Aug., 1875. G. F. B.

9. *Cold Bands of Dark Spectra.*—MM. P. Desains and Agmonet have examined with a thermopile the spectrum of the flame of the lamp of Bourbouze and Wiesnegg; this source of heat is more convenient and trustworthy than the Drummond lamp. With a 60° prism and lenses of rock-salt, on passing the light through a centimeter of water, four cold bands were distinctly shown. Their distances from the extreme red end of the spectrum were 19′·8, 30′·6, 42′ and 52′ respectively. These measurements cannot have the same precision as those of the dark lines of the spectrum, but they show very nearly where the pile must be placed to receive the minimum amount of heat.

The pile was placed about 30 cms. from the axis of the prism, and the breadth of the admission slit was half a millimeter; its angular magnitude as seen from the prism was 5′·7, conseqently each cold band made its effect sensible through an angle equal to its own angular width increased by 5′·7. The illumination slit also had a width of half a millimeter.

According to the measurements of M. Lamanski four cold lines in the spectrum of the sun are distant from the extreme red by the angles 19′·1, 30′, 44′, and 51′. These positions are evidently the same as those of the lines given above by the absorption of a centimeter of water. This coincidence seems to prove that the cold bands in the dark portion of the solar spectrum are in a great measure due to the water of the atmosphere.

Another series of experiments were made to compare the action exerted on dark spectra by different solutions, formed by disolving a solid capable of forming cold lines, in liquids nearly inactive in this respect. The active substance employed was iodine, which was dissolved in chloride of carbon, chloroform and sulphide of carbon. These three liquids dissolve iodine in large quantities, and all three solutions have the same appearance. A layer of liquid, one centimeter in thickness, was used and the prism and lenses were of flint glass. Three lines were observed having the positions given below:

Chloride.	Chloroform.	Sulphide.
1° 28′	1° 30′	----
1° 34′	----	1° 35′
1° 55′	1° 57′	1° 56′

These numbers show that the action of the iodine is the same in the three cases. In all the experiments observations were made on that part of the spectrum where the iodine solution produced a line, so that the solvent and the whole refracting system had no action in the production of the phenomenon.—*Comptes Rendus*, lxxxi, 432; *Phil. Mag.*, l, 331. E. C. P.

10. *Luminosity of Flames.*—M. F. Wibel has examined the causes of luminosity and non-luminosity of flames. Knapp found that the flame of a Bunsen's burner became non-luminous if nitrogen, hydrochloric acid, or carbon dioxide be passed into the air holes instead of air. Blockmann found that carbon monoxide and hydrogen have the same action, and the same effect is produced, as Landon has lately shown, by steam. When, however, any such mixture is very strongly heated before it undergoes combustion, it again becomes luminous. This is best shown by fixing a platinum tube into the upper end of the burner and heating it with two horizontal gas-jets. An iron tube may also be used instead of the platinum tube, but, on account of the higher specific heat of iron, it must be more strongly heated. A copper burner cannot be used, because it colors the flame green. The luminous flame appears as a cone between the inner dark cone and the outer bluish cone. This is not caused, as might be supposed, by the gases

of combustion of the two burners surrounding the colorless flame and thus keeping the oxygen away, inasmuch as the luminosity is equally well produced when the access of these gases is prevented by a large sheet of iron having a hole in it, through which the tube of the burner passes air-tight.

From these observations it may be concluded that the non-luminosity of a gas-flame is not caused by a dilution of the gas, this dilution being in fact increased by heating the mixture. The only cause is a cooling of the interior of the flame. This is further proved by the fact that it is most difficult to get a non-luminous flame from a mixture of coal-gas and oxygen, showing that neither rapid oxidation nor dilution produces the non-luminosity.—*Deut. Chem. Ges. Ber.*, viii, 226; *Jour. Chem. Soc.*, xiii, 603.

E. C. P.

11. *Rotatory Polarization of Quartz.*—MM. J. L. SORET and E. SARAZIN have repeated the experiments of Broch and Stefan extending them to the more refrangible portions of the spectrum. The light of the sun concentrated by a lens 1·15 m. focus and 72 mm. in diameter, was passed through two Nicol prisms with a plate of left-handed quartz 30·085 mms. thick between them. It was then observed with the fluorescent eye-piece of M. Soret. For the rays *A* and *a*, a piece of cobalt glass was interposed to cut off the adjacent bright light.

	λ	ϕ	Δ
A	760·0	12°·68	+0·10
a	718·5	14·33	·00
B	686·7	15·75	·00
C	656·2	17·35	·00
D	588·9	21·79	—·05
E	526·9	27·61	—·06
F	486·07	32·85	—·07
G	430·72	42·63	+·06
h	410·1	47·52	—·05
H	396·8	51·22	·00
L	381·9	55·88	—·08
M	372·7	59·03	—·06
—	372·0	59·24	·04
N	358·5	64·41	+·03

The formula $\varphi = A + \frac{B}{\lambda^2}$ in which φ is the angle of rotation and λ the wave-length agrees well with observations between *B* and *H*, but is inexact if used between more extended limits. If the constants are computed for *a* and *M* the value for G is erroneous by a whole degree. Starting with the idea that the rotation ought to be nothing for a wave of infinite length, M. Boltzmann has proposed the formula $\varphi = \frac{B}{\lambda^2} + \frac{C}{\lambda^4} + \frac{D}{\lambda^6} + \&c.$ Retaining two terms only, and computing *B* and *C* for the lines *a* and *M* gives the formula, $\varphi = \frac{7{\cdot}10533}{10^6\lambda^2} + \frac{0{\cdot}151227}{10^{12}\lambda^4}$ which agrees well with observation. In the annexed table, column I. gives the name of the various lines, column II. their wave-lengths, column III. their observed angle of rotation, each being the mean of from six to forty-six observations; the last column gives the difference between column III. and the value computed by the formula given above.—*Comptes Rendus*, lxxxi, 610. E. C. P.

12. *Researches on the Hexatomic Compounds of Cobalt;* by WOLCOTT GIBBS, M.D. 51 pp. 8vo. From the Proceedings of the American Academy of Arts and Sciences, vol. xi.—The earlier parts of Dr. Gibbs' researches on the cobaltamines have been published in this Journal, and will be found as follows: III, vol. vi, p. 116; also vol. viii, p. 189 and p. 284. For the conclusion of the memoir the chemical reader is referred to the article of which the title is given above; notwithstanding its great value, the length is so considerable as to make the republication in this Journal hardly possible.

13. *Professor Tyndall and Professor Henry on Sound.*—The following criticisms are here cited from a notice of the new edition of Prof. Tyndall's work on Sound (1875), in "The Nation" for October 28:

"The reader who turns to this seventh chapter will find that it opens with an 'introduction' professing to give 'a summary of existing knowledge' in the matter of fog-signalling. The writer states that while the *velocity* of sound has formed the subject of repeated and refined experiments by the ablest philosophers, 'the publication or Dr. Derham's celebrated papers in the Philosophical Transactions for 1708 *makes the latest systematic inquiry into the causes which affect the intensity of sound in the atmosphere.*' And, after making this statement, the Professor immediately adds as follows: 'Jointly with the Elder Brethren of the Trinity House, and as their scientific adviser, I have recently had the honor of conducting an inquiry designed to fill *the blank here indicated.*' In order still further to impress on the reader a sense of the magnitude of this 'blank,' Dr. Tyndall indulges in one or two preliminary references which, he says, 'will suffice to show the state of the question when this [his] investigation began.' The first of these references cites the opinion of Sir John Herschel to the effect that fogs and falling rain, and more especially snow, had been found by Derham 'to tend powerfully to obstruct the propagation of sound.' The second of his references is made to what he calls 'a very clear and able letter' addressed by Dr. Robinson of Armagh to the British Board of Trade in 1863. In this 'very clear and able letter' Dr. Robinson states that sound is the only known means for coping with fogs, but about it, the 'testimonies are conflicting, *and there is scarcely one fact relating to its use as a signal which can be considered as established.*' But Dr. Robinson is clear on one point—to wit, than 'fog is a powerful damper of sound.'

On the strength of these historical references, Dr. Tyndall ventures the remark that, prior to the investigation conducted by him, the views ennunciated under this head by Derham, Herschel, and Robinson 'were those universally entertained.' It was in order to fill 'the blank' indicated by the universal prevalence of such erroneous opinions that *his* inquiry, he says, was set on foot. And his inquiry, he tells us, was begun on the 19th of May, 1873.

Now, it is a matter not only of scientific knowledge, but of public notoriety in this country, that extensive researches on 'the cause which affect the intensity of sound in the atmosphere' had been made by the United States Light-House Board long before Prof. Tyndall began his investigations. That he should have chosen to ignore the fact in the body of his present volume becomes only the more surprising when, on turning to its preface, we find that he was, as he confesses, 'quite aware *in a general way* that labors, like those now for the first time made public, had been conducted in the United States,' and 'this knowledge,' he subjoins, 'was not without influence upon my conduct.' If his knowledge of the similar labors conducted under this head in the United States was not, as he acknowledges, without influence on his conduct in giving direction to his researches, it will naturally occur to ordinary minds that this knowledge should also have been 'not without influence' on his pen when he was professing to give a summary of the existing state of science on this subject. And when to this statement of the case, as acknowledged by himself, we add that he was made acquainted with the nature and purport of Prof. Henry's explorations on this question, not only 'in a general way,' but also in a very special way, it becomes still more inexplicable that in defining 'the blank' which he claims to have filled by his recent inquiry, he should have disregarded the labors and results of American science, and that too while profiting by the instruments and methods of that science in the very conduct of his investigations. The reader will understand the force of our remark that Prof. Tyndall was acquainted with the researches of Prof. Henry, not only 'in a general way,' but also in a special way, when we state that a paper by the latter—on the abnormal phemomena of sound in relation to fog-signalling—was read by its author in the hearing of Prof. Tyndall at a meeting of the Washington Philosophical Society, called for the purpose of doing honor to the British savant while he was sojourning in the national capital. And the force of our remark that he has ignored the results of American science in magnifying 'the blank' which he describes, while profiting by the instruments and methods of that science in conducting his inquiry, will be understood when we say that the researches of Prof. Tyndall were prosecuted with the help of a steam-syren, gratuitously lent to him by the Light-House Board at Washington, constructed and patented by a citizen of New York, and introduced by Prof. Henry into the light-house system of the United States." * * *

"It is no part of our present purpose to institute a critical inquiry into the conflicting views of Prof. Henry and of Prof. Tyndall with regard to the hypotheses respectively espoused by each for the explanation of the phenomena of sound in its passage through wide tracts of air. Prof. Henry believes that the direction and the rate of wind-currents are important elements in the problems presented by the phenomena in question. Prof. Tyndall admits that 'the well-known effect of the wind is exceedingly difficult to explain,' but he insists on making up the fagot of his scientific opinions on the subject at once and for ever without taking the 'viewless winds' into his account. He finds a sufficient explanation of all the abnormal phenomena in the assumption of ideal clouds of vapor mingling with the atmosphere so as to disturb its homogeneity, and thereby quench the body of sound. There is nothing in the working hypothesis of Prof. Henry which excludes any truth there may be in the working hypothesis of Prof. Tyndall. But in the present provisional state of his inquiries on the subject, the former is disposed to question the sufficiency of the explanation adduced by the latter as an efficient cause of all the phenomena in question. With the modesty and reserve of the true physical philosopher, in the present unfinished state of scientific inquiry, Prof. Henry waits for the wider knowledge which shall furnish the basis of an assured induction meeting all the requirements of the problem."

14. *A new Treatise on Elements of Mechanics, establishing strict precision in the meaning of Dynamical terms, accompanied with an Appendix on Duodenal Arithmetic and Metrology;* by JOHN W. NYSTROM, C.E. 352 pp. 8vo. Philadelphia, 1875. (Porter & Coates.)—This volume contains a great number of problems in practical mechanics, with concise and excellent solutions, and some useful tables. The author makes some innovations in the technical language of the science, which are not likely to be considered as in all respects improvements, and some of the topics treated in the volume are rather irrelevant to the subject. A. W.

II. GEOLOGY AND NATURAL HISTORY.

1. *Primordial fossils in pebbles of the Newport (Rhode Island) Conglomerate.*—Prof. WM. B. ROGERS, in the Proceedings of the Boston Society of Natural History for 1875, p. 97, discusses the origin of the flattened form of the siliceous pebbles or stones of the Newport conglomerate, and attributes it to sea-shore attrition. He also announces the discovery of many stones from the beach, that were derived from the conglomerate, which are "crowded with well-preserved impressions" of the Potsdam *Lingula*, similar to those found by him, several years since, in masses of the same conglomerate at Fall River, 17 miles to the east of north of Newport. Some impressions of the same fossil, less distinct, he has observed in the rock in place at Newport.

2. *Bowlder near Batavia, Genesee County, New York.*—The bowlder "on the road from Caledonia to Batavia," figured by Hall on page 341 of his Report on New York Geology (1843), and stated to consist of Hydraulic limestone from the upper part of the

"Onondaga Salt group," has the following dimensions, according to a recent letter to one of the editors received, along with an excellent stereoscopic photograph, from Mr. J. G. Fargo of Batavia: height 12 feet; width of base 20 feet, of neck 8 feet, of top 14 feet. It is underlaid by about 40 feet of drift, beneath which is the Corniferous limestone.

3. *On a method of distinguishing the different Feldspars by means of their optical properties;* (Extract from a letter from M. DesCloizeaux, dated Villen-sur-mer, July, 3d, 1875.)— * * * This method can be applied to all the feldspars, and gives a much easier means of distinguishing them than those described in my last memoir (this Journal, III, ix, 322). It is only necessary to obtain a section, sufficiently thin and transparent, parallel to the easiest cleavage [p] and smooth enough to be homogeneous in all its parts. Similar sections obtained from crystals or lamellar masses of albite, oligoclase, labradorite, and the majority of those of microcline, show hemitropic bands, more or less close together, arranged along the plane parallel to the second cleavage [g^1]; for orthoclase and microcline in *simple crystals*, two sections placed in opposite directions serve to produce the same effect. These sections are thus brought between the crossed Nicols of a polarization-microscope.

(1.) For *orthoclase* the maximum extinction takes place when the two sections are parallel to their plane of contact; the edge $p\,g^1$ being in the plane of polarization of the microscope.

(2.) For *microcline*, the whole structure consists of a multitude of very fine parallel bands; we have microline alone, either hemitropic or not hemitropic, or microcline and orthoclase; the extinction can take place at 30° 54′ between the adjoining bands of the same plate of the macle (microcline alone), at 30° 54′ between the two plates of the macle (microcline in bands), or at 19° 27′ between the adjoining bands (microcline and orthoclase.) In the last case the whole of two lamellæ of the macle show at the same time an extinction oblique to the plane of composition, belonging to the microcline, and one parallel to this plane for the orthoclase.

(3.) For *albite*, the extinction between two bands takes place at an angle of 6° 32′.

(4.) For *oligoclase*, the extinction is simultaneous in the two bands, and when the plane of composition coincides with the plane of polarization of the polariscope, it shows that the structure is homogeneous.

(5.) For *labradorite*, the extinction takes place at 10° 24′ between the alternate lines of the hemitropic lamellæ.

It follows from this that a plane normal to the plane of the axes cuts the base along a line making with the edge $p|g^1$ the following angles:

0° in orthoclase and microcline.
19° 27′ in microcline.
3° 16′ in albite.
7° 12′ in labradorite.

I shall determine the same angle for anorthite when I return to Paris. * * * . E. S. D.

4. *Contribuzoni Mineralogiche per servire alla Storia dell' Incendio Vesuviano del Mese di Aprile*, 1872, *memoria di* Arcangelo Scacchi. Parte seconda, 69 pp. quarto. Naples, 1874.—The first part of Scacchi's valuable memoir on the eruption of Vesuvius in the Spring of 1872 was published two years since. The second part includes descriptions, mostly crystallographic but also to some extent chemical, of the following species, produced by the eruption: hematite and magnetite, tenorite (melaconite), *atelite*, sylvite and halite, sal ammoniac and *cryptohalite*, *chlorocalcite*, cotunnite and *pseudocotunnite*, *erythrosiderite*, molisite, *chloromagnesite*, *chloralluminite*, aphthitalite, anhydrite, *cupromagnesite*, *chlorothionite*, *microsommite*, pyroxene, byssolite, *hydrofluorite*, *proidonite*, chrysolite, apatite. The names of the new species are printed in italics; their principal characters are given below, with the exception of several already described in Appendix II, to Dana's Mineralogy, 1875.

Atelite; produced by the action of hydrochloric acid on the lamellar crystals of tenorite (melaconite), by which their color is changed from black to green. Its composition is expressed by the formula $2CuO, CuCl+3HO$.

Cryptohalite; a fluosilicate of ammonia, associated with sal ammoniac, and having the composition $2NH^4Fl, SiFl^2$.

Pseudocotunnite; occurs with cotunnite in acicular yellow crystals, opaque and lusterless. In water they lose their color, dissolve in part, and the remainder separates as a white powder. Composition, probably expressed by the formula $PbCl, KaCl$.

Chloralluminite; composition Al^2Cl^3, xHO, associated with molisite, and chloromagnesite; decomposes on exposure to the air.

Chlorothionite; found in crusts of an azure-blue color, an analysis of which agreed approximately with the formula Ka, Cu, Cl, So^4.

Hydrofluorite; hydrofluoric acid, HFl; proidonite, $SiFl^2$; both products of the eruption of 1872. E. S. D.

5. *Materialien zur Mineralogie Russlands;* von N. von Kokscharow. Vol. vi, pp. 209-407, with index, vol. vii, pp. 1-176, 8vo. St. Petersburg, 1874, 1875 The crystallographic researches of M. von Kokscharow are always remarkable for their thoroughness and great exactness. The volumes, or better parts of volumes, last published, include memoirs upon a large number of species, of which the following are some of the more important: native copper, vi, p. 209; aragonite, p. 261; dioptase, p. 285; scorodite, p. 307; gold, p. 321; sulphur, p. 368; perofskite, p. 388. Dolomite, vol. vii, 1; barite, p. 25; calcite, p. 59; crocoite, p. 97. E. S. D.

6. *Memoire sur la Famille des Pomacées;* par J. Decaisne.—A detailed analysis of Decaisne's monograph of the genus *Pyrus* was given in this Journal (3rd ser., iv, 489, Dec., 1872). Some of the views taken in that work are fully expounded in the present paper, which is separately issued from the Archives du Museum,

x, pp. 113–192, tab. 8–15, imp. 4to. The results of a prolonged study of an important group, by a botanist of great experience and ability, is worthy of particular attention. As the veteran author states it:—

"My principal object is here to call the attention of botanists to certain characters which have been neglected in systematic works, by the aid of which the ancient genera [merged in *Pyrus* by most of the recent systematists] may be neatly circumscribed. Such is the constancy and the value of these characters that the details of organization peculiar to each generic group may be expressed by very general propositions, which is the very object of a good method. Indeed, when a special organization is common to a large number of different plants, it is evident that comparatively slight but constant modifications of this structure ought to be particularly attended to; and this proposition seems to be especially true of the *Pomaceæ*." M. Decaisne puts foremost his strongest point when he declares of the Quince, that "the nature of its bark and wood, its prefoliation, inflorescence, the æstivation of the corolla, the structure of the ovary and of the fruit differ essentially from that of the Pears, among which certain botanists still class it." Rather than combine the Quince and the Japan Quince with *Pyrus*, we are confident that botanists will generally accept his *Docynia*, along with *Chænomeles* Lindl. and *Cydonia*, as independent genera. The same may be said of *Mespilus;* and it must be allowed that the character which Kunth had noticed and which Decaisne has turned to account, that of the deformation of one of the ovules which becomes a kind of stipitate hood for the other, being common to it and to *Cratægus*, indicates a relationship to the latter genus rather than to *Pyrus*. Much nicer and more questionable characters are assigned to the genera here re-established from *Pyrus* in the Candollean sense, to which we are in this generation accustomed. These are, *Aronia* Pers., our Chokeberry (in which eight species are set up from what we take to be a single polymorphous one); *Sorbus* Tourn., the Mountain Ash (the synonym *S. microcarpa* omitted from *S. Americana*, and *S. sambucifolia* is still taken to belong only to our western coast, whereas it extends across the continent); *Aria* Host., the Beam Trees, all of the Old World; *Torminaria* Roem., for *Pyrus torminalis;* also *Cormus* Spach, for *Sorbus domestica* L., the Service-tree of Europe, with *Pyrus trilobata* DC., and an allied species; *Micromeles*, a new genus for four Himalayan species thus far little known; lastly *Malus* Tourn. Here it is to be observed that *M. diversifolia* is held to be distinct from *M. rivularis;* and that a subgenus, *Chloromeles*, proposed for *M. angustifolia*, our narrow-leaved Crab-Apple, thus widely separated from *M. coronaria*, on account, as is stated of its reddish anthers and the structure of the disk. *Pyrus* Tourn., is thus brought down to the Pear; and this, as Decaisne had formerly announced, to a single collective species, of six geographical *proles* or forms. We continue to write *Pyrus* from old habit and custom, not doubting, however, that *Pirus* is the correct orthography.

Of *Amelanchier*, following Lindley, there are enumerated twelve species, six for the Old World and six for North America, and there are names for four more. Without being able to clear them up (and no wonder), Decaisne thinks that they may be distinguished into at least three groups, characterized by the distinct or united styles, and the glabrous or downy ovaries. We are continually impressed with the idea that there must be three or four American species, and the seeds may aid in their definition. But thus far the characters elude investigation. *Peraphyllum* Nutt., referred by Bentham and Hooker to *Amelanchier*, has not been studied by Decaisne. When he examines the excellent specimens in flower and in fruit, which Mr. Siler has supplied from Southern Utah, he will conclude that the genus must certainly be reinstated. The likeness is only in the peculiar structure of the fruit.

As respects the remaining genera, the difference between this monograph and the disposition in Bentham and Hooker's Genera Plantarum is mainly this: *Eriobotrya*, with its baccate fruit (what is termed endocarp reduced to a soft pellicle), large turgid seeds with thickened cotyledons, and undulate petals, is upheld as a good genus: *Heteromeles* is adopted from J. Roemer for the Californian *Photinia arbutifolia*, and a second (probably not good) species, *H. Fremontiana*, is added. The characters appear to be the 10 instead of 20 stamens, in pairs opposite the calyx-lobes, their filaments dilated at base and somewhat monadelphous. In the tabular conspectus the petals are said to have "préfloraison tordue," but in the generic character it is "æstivatione imbricativa vel convolutiva," the latter term with the French botanists meaning the same as imbricated only more enrolling. The diagram represents the whole five petals with one edge covered, i. e., "tordue" or contorted (or, as we say, convolute), and so we find them in all the flower-buds now examined. But before adopting the genus it may be well to examine the *Photiniæ* generally. *Photinia*, of which *P. serrulata* is the type, is characterized as having imbricative æstivation, and Decaisne's diagram represents it as regularly (i. e., quincuncially) so. But in *P. prunifolia* and in *P. Blumei* we find occasionally only one exterior petal, and the four others successively overlapping in the "contorted" way; and in one of Wallich's specimens of *P. integrifolia* the first flower-bud inspected showed the "contorted" æstivation complete. This is also the case in *P. dubia* (in one of Hooker's and Thompson's Khasyia specimens), and this Decaisne refers to *Eriobotrya*, which has imbricative æstivation. Next is *Pourthia*, a new genus of eleven Japanese and Indian species, the type being *Photinia arguta*, *villosa*, *lævis*, &c., and the character, among others, "æstivatione contorta." But we as commonly find one petal wholly exterior. So we think it evident that the æstivation of the corolla furnishes no characters for the division of the genus *Photinia*.

Finally, as to the proper stone-fruited genera, *Pyracantha* is adopted from J. Roemer, for *Cratægus Pyracantha* and an allied Indo-Chinese species, and placed near *Cotoneaster;* and a character not before used is introduced, namely, the position of the cotyledons, which, in this genus are, as regards the rhaphe, accumbent.

There are eight plates, six of them filled with admirable dissections, neatly done upon stone by Riocreux from the author's sketches. A. G.

7. *Elementary Lessons in Botanical Geography;* by J. G. Baker, F.L.S.—These first appeared as articles in the Gardeners' Chronicle, where they attracted attention as interesting and very readable sketches of the leading facts respecting the geographical distribution of plants, and the elements of climate which control this distribution. It appears that they were written by Mr. Baker, of the Kew Herbarium, and Lecturer on Botany at the London Hospital. Now collected, they form a neat 18mo volume, of 110 pages, a sort of "science primer," published by Lovell Reeve & Co., London, 1875. A. G.

III. ASTRONOMY.

1. *Observations on the late Transit of Venus at Beechworth, Victoria;* by Dr. Henry J. Anderson.—The following interesting description of the observation of the late Transit of Venus, at Beechworth, Victoria, by Dr. Henry J. Anderson of this College, is taken from a letter addressed to me which has just been received and which bears the date Srinagar, Cashmere, India, August 30th, 1875. The telescope used has a clear aperture of 77 millimeters and a focal length of 1·125 meters. It was made by Secretan of Paris.

"The telescope was subjected, for this purpose, to chosen tests by Mr. Lewis Rutherfurd, then in Paris, and always so ready to oblige. I used the lower magnifying power, 98 I believe. Various circumstances interfered with my obtaining an observation of the exact moment of the first external contact. I did not see the planet until at least twenty degrees of its limb was on the sun's disc. This occurred at $11^h\ 31^m\ 40^s$, Melbourne time, as sent by telegraph from Melbourne to Beechworth where I observed. Thirteen seconds had probably elapsed. At $11^h\ 35^m\ 30^s$ the interior tangents to the two obtuse cusps were at right angles to each other. At $11^h\ 45^m\ 29^s$ these tangents were to all appearance parallel. At $12^h\ 0^m\ 9^s$ the cusps (then singularly sharp and well-defined) were distant from each other about one-seventh of the diameter of the planet. The atmosphere at this time was all that could be desired. A remarkable calm and clearness of the sky prevailed. There was no cloud, or even fleeciness in the direction of the sun, which was then only fifteen degrees from the zenith. As the observation was made with a diagonal eye-piece, the great altitude of the sun was very favorable to steadiness and distinctness of view, both from the position of the observer and the condition of the atmosphere. The instrument was adjusted as to focus, etc., as nearly as possible in accordance with the recommendation of Messrs. Andrè and Wolf, and whatever may have been the cause, nothing in any way resembling the "black drop" made its appearance, either as the first internal contact drew nigh or at any other time. The cusps approached each other with a motion geometrically, and to me surprisingly, precise; without bluntness, flickering or vibration. Such portions of the planetary disc as were near or between the cusps, seemed visibly less dark than the main portions more remote; but this gradation of hue was limited in extent and apparently without influence on the formation of the luminous thread.

Just preceding the union of the cusps the approaching points seemed to lose for a second or two the regular precision of their motion toward each other; but the filament once formed did not appear to me to break again. This phase occurred at Beechworth [longitude 146° 43′ 0″ $= 9^h\ 46^m\ 52^s$ E. of Greenwich, latitude 36° 21′ 40″ S.; height above sea level 1800 feet] at $11^h\ 0^m\ 24^s{\cdot}6$ Melbourne mean time; subject to any error, against which all possible precautions were taken, either in the telegraph from Melbourne or in the chronometer carried from the telegraph office to the place of observation.

Not many minutes had elapsed after the first internal contact when a sudden change of weather ensued. A high wind sprang up and dense clouds, at intervals, quite obscured the sun. At two P. M. the planet ceased to be visible and no phase of the egress could be observed. I wish to express my warmest thanks to Mr. William J. Turner, not only for the use of his grounds, but for his skillful aid in securing for me a steady manipulation of the telescope and its appendages.

The longitude and perhaps the latitude of the place of observation, no doubt need some slight correction."

L. WALDO.

Columbia College Observatory, N. Y., Oct. 16th, 1875.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Anderson School;* letter to the Editors of the Evening Post from Prof. ALEXANDER AGASSIZ.—A letter signed "E," lately printed in the Evening Post, contains certain charges against the trustees of the Anderson School. Before answering them the position of the director and trustees toward Mr. Anderson may be briefly noticed.

It may or may not have been known to Mr. Anderson that only the urgent solicitation of Professor Agassiz overcame my reluctance to serve as one of the original trustees of the school at Penikese. My appointment as his successor was against my own wish, and of this Mr. Anderson was fully aware. It was always agreed between Professor Agassiz and myself—and the founding of a summer school in no wise changed this understanding—that as his scientific executor the care of the museum at Cambridge should be my special charge, and that toward its support alone whatever I could contribute personally or command from others would be applied.

I held the nomination as director of the Anderson School, left in writing by Professor Agassiz, in abeyance, until early in 1874 the opportunity occurred to confer with Mr. Anderson, when I explained to him my position with reference to the museum at Cambridge. He, therefore, knew that, should this nomination be accepted, the duties of the director of the school must remain subordinate to those of the director of the museum, and that the interests of the former would not be allowed to interfere with the prospects of the latter. With this reservation, and the condition that Mr. Anderson would contribute the sum of $10,000 towards the support of Penikese for the next three years, I agreed to serve. This sum was considered as probably sufficient to carry on the undertaking until the future of the museum was assured. Of this $10,000 Mr. Anderson subsequently forwarded $1,329.60 (the check mentioned by your correspondent to the trustees), notifying them at the same time that for "reasons satisfactory to his own mind" he declined to make any further pecuniary sacrifices for the school. Neither the director nor the trustees could assume the responsibility thus left to them.

With this explanation I may now take up the charges of your correspondent. He says it was "authoritatively stated" that $10,000 of the Anderson Fund remained unexpended, and would suffice for the sessions of 1874 and 1875. No such statement was made, and no such sum existed. With the sanction of Mr. Anderson, the bulk of the "nucleus of an endowment fund" had been devoted by Professor Agassiz not only to buildings and equipment, but also to the running expenses of 1873. It was found, before the opening of the session of 1874, that these expenditures amounted to $47,000. There remained from the Anderson Fund not the $10,000 which figures so largely in the letter of "E," but about $1,100 available to the trustees. With this and $1,500 realized from the yacht Sprite, sold with the consent of Mr. Galloupe, the school reopened. As far as the interests of education were concerned the second session was a decided success, thanks to the interest taken by the teachers; but the institution remained in debt, and Mr. Anderson, to whom an informal account was sent, contributed, as above stated, $1,329.60 to pay the "actual deficit." This left the trustees to provide for existing liabilities, or be reproached, as they now are, with "needlessly sacrificing" the property under their charge. No appeal was made to Mr. Anderson beyond what he had himself proposed to do for the school. It is not true that he was "permanently laid under contribution—under penalty." It is not true that the condition of the school was kept back from him, or that the trustees had not the courtesy to signify to Mr. Anderson their intention of closing it. He has at all times been kept fully informed, partly by letters and partly in conversation, of its condition and prospects, as far as they were known to the trustees themselves.

They made every effort to save the school, to put it on a safe foundation, to make it self-supporting. A guaranty fund of $3,000 was contributed by a member of Professor Agassiz's family, and a very slight permanent support from the public would have enabled them to continue an institution which they close with deep regret. Receiving no such support they decided to wind up the affairs of the school, a contingency of which Mr. Anderson was duly notified early in 1875, after he had of his own accord dissolved the conditions upon which alone the present director consented to serve. This brings me to the suggestion, made in the form of an accusation, that the trustees of the Anderson School, and by implication the trustees of the museum, might have appropriated a small portion from the Teachers' and Pupils' Fund of the Agassiz Memorial and have applied it, in "better faith and better morals" to paying the debts of Penikése. Neither the trustees of the Anderson School nor the trustees of the museum have any control over the Teachers' Fund. It was raised with the understanding, plainly stated in the circulars issued at the time, that it would be devoted to carrying out the plans of Professor Agassiz for the museum at Cambridge. That this will be done with the full appreciation of the relations which existed between him and those who have borne such affectionate testimony to his memory, every teacher throughout the country may be sure. But the use suggested by your correspondent would have been a strange misappropriation of money contributed for a special purpose.

As to the charge which seems directed against the trustees of the museum, as well those of the Anderson School, that they have trodden on the memory of their former director, the fact that these boards include several of Professor Agassiz's warmest friends, two of his favorite pupils, his brother-in-law, his son-in-law and his son, is a sufficient refutation of this calumny.

Finally, no insults have ever been offered to Mr. Anderson by the trustees, unless their decision not to become indefinitely responsible for the expenses of the school, and to close an institution which no one but themselves seemed inclined to support, can be so construed.

Cambridge, October 12, 1875.

2. *Annual Report of the Board of Regents of the Smithsonian Institution for the year* 1874. 416 pp. 8vo. Washington.—In addition to the report of the progress of the Institution in its various branches, the present volume includes the usual Appendix of Scientific Memoirs. The Memoirs in this new volume are: Eulogies on Laplace, Quetelet and Auguste De La Rive; on tides and tidal action in harbors, by Prof. J. E. Hilgard; Observations upon the electricity of the atmosphere and the aurora borealis, made during the Swedish expedition of 1868 to the north pole, by Prof. Selim Lemström of Helsingfors; On a dominant language for science, by Alphonse de Candolle; On underground temperature, by C. A. Schott; On a series of earthquakes in North Carolina, commencing Feb. 10th, 1874, by Prof. Warren du Pré; on the transactions of the Society of Physics and Natural History of Geneva, from June, 1872, to June, 1873, by A. De La Rive; On warming and ventilation, by A. Morin.

Under the head of ETHNOLOGY, has been collected a series of valuable observations, comprising an account of ancient graves of California, by Paul Schumacher; also descriptions of various antiquities from Illinois, Ohio, Kentucky, Lake Superior, Mississippi, Tennesse, New York, Indiana, Maryland, North Carolina, and Florida.

3. *Croll on Climate and Time.*—Professor Croll's work on Climate and Time, noticed on page 222 of this volume, has been republished by Messrs. Appleton & Co. of New York.

4. *A Course in Descriptive Geometry for the use of Colleges and Scientific Schools;* by WILLIAM WALTER, Ph.D. (Boston, L. Prang & Co.)—In this work the treatment is concise without serious omissions or inadequate explanations, and yet sufficiently full without verbosity. The fact that a text of fifty pages requires a table of contents of eleven pages, shows how little needless description is given. Doubtless much of the saving of space is due to the convenient and expressive notation adopted.

The introduction of a series of stereoscopic views of many of the problems is a novel feature, and will doubtless materially aid the student in the place of models. It must not be forgotten, however, that a large part of the value of Descriptive Geometry arises from its training the student to conceive of objects from their projections; hence the value of any such aid, except to initiate the student in his work may be questionable. The collection would have been improved by the addition of one or two more warped surfaces, especially the hyperbolic paraboloid and conoids, which always give trouble to the student. The typographical execution of the work is excellent and the drawings accurate and easily understood. E. C. P.

5. *American Naturalist.*—The American Naturalist will be published the coming year by Messrs H. O. Houghton & Co., Cambridge, Mass., and will appear in new typographical dress. The Journal has hitherto been well conducted, always able in its articles, and while popular in its aim, never popular at the expense of its science. It is announced that the Naturalist will be improved under its new publishers by fuller notices of current science, and in other ways.

6. *School of Geology in the University of Syracuse.*—Under Professor Winchell a school of Geology has been organized in connection with the Syracuse University. It includes a course of Elementary Study or Undergraduate Course, and two Graduate Courses, one Lithological and the other Paleontological, carried on together. There are also to be special studies in different departments of Zoology, as in that of Corals, Brachiopods, Trilobites, &c. The school is intended for special geological students, and also for those who desire a general acquaintance with the subject. The school will open on the 25th of next January. The prospectus announces that Prof. Winchell will be aided in the school by Prof. J.. J. Brown in Chemistry, Prof. F. Smalley and Rev. S. R. Calthrop in Geology, Profs. B. G. Wilder and Smalley in Zoology, Profs. James Hall, R. P. Whitfield and E. D. Cope in Paleontology. The position of the school in central New York is particularly favorable for the geological and paleontological student.

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[THIRD SERIES.]

Art. LIX.—*The Solar Atmosphere, an introduction to an account of researches made at the Allegheny Observatory;* by S. P. Langley, Director of the Observatory.*

It has long been observed that the sun is not everywhere equally bright, but that the edge is darker than the center, and since a self-luminous sphere should appear of sensibly uniform brilliancy, so as to present to the eye the appearance of a flat disc, this diminution of light must be due to some medium external to the photosphere. Accordingly, even before the invention of the spectroscope, it was admitted that an absorbing atmosphere surrounded the sun, and that the effect would necessarily be to diminish the amount of radiation. La Place, in the tenth book of the Mécanique Celeste, has attempted to compute the total effect of this absorption, from data furnished by the observations of Bouguer, and considers that, were the solar atmosphere removed, the heat and light would be twelve times as great as at present. Though this value is erroneous, being deduced from imperfect data by processes which rest on a false hypothesis, it may yet serve to call attention to the importance of a somewhat neglected field of research, in which this Observatory has been in part occupied during the past three years.

Several estimates of the absorptive power of this atmosphere have been made, differing widely from each other. According to that of La Place, just cited, the absorption is about $\frac{11}{12}$ of the sun's emission; according to Liais less than $\frac{1}{12}$; according to Secchi ·88. These discrepant results may be due in part to different hypotheses used in computation, but they are in every case founded on an experimental comparison of the light or heat of the sun observed at the center with that observed near the edge.

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* Paper read at the Detroit meeting of the American Association for the Advancement of Science.

This direct comparison of the two lights involves, of course, no hypothesis, but is simply a photometric measurement, and apparently an easy one. Let us examine the result of this comparison at different hands. Arago,* using the Rochon prism and analyzer, announced, as the conclusion of a prolonged research, that the light at the center of the solar disc must be diminished by $\frac{1}{41}$ part, or 2·4 per cent, to equal that of the edge. Liais in a memoir † which is a model of conscientious care, announces, from comparisons carried on by the extinction of one light by a stronger, that the central light should be diminished by about 10 per cent, to equal that of the edge. His result is then four times as great as Arago's. Secchi with a wheel photometer, finds, for a nearly corresponding point, 78 per cent as the amount by which the central light should be diminished. This result is then *seven* times that of Liais and *thirty* times that of Arago.

Secchi has made observations on radiant heat to be found in *Le Soleil*,‡ the most important results of which are that the heat diminishes from the center to the edge; that for a given point there is a satisfactory agreement between the absorption of light and heat; and that the equatorial regions are hotter than the polar. The present writer has made somewhat extended researches in the same direction which are chiefly unpublished. Vogel has made interesting researches in the relative actinic absorption.

None of those who have attempted to compute the amount of the solar absorption appear to have drawn conclusions from their results as to its effect on terrestrial temperatures, and yet if the absorption be anything like what has been found by La Place, and by Secchi using La Place's formulæ, the subject deserves attention.

It will be shown in a forthcoming memoir that the absorption is much less than these values, but that definite limits can be assigned in which it must lie, and that at any rate it is of such importance that the conditions of animal life upon this planet largely depend upon it. It will further appear probable that with a slight change in the depth and absorptive power of this atmosphere, fluctuations in terrestrial temperature will ensue, very great in comparison with any actually observed within historic periods, and it will be shown that this atmosphere is not in a strictly stable condition. Though the subject then is a nearly neglected one, and the methods of investigation of less apparent interest than those of the spectroscopist, the results are, if verified, of a peculiar interest, from their bearing upon the possible changes in the mean temperature and climatic conditions of our own planet.

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* Oeuvres Complètes, Tome i, p. 235.

† Mémoires de l' Académie de Cherbourg.

‡ Le Soleil, 2me edition, Paris. 1875.

The portion of this atmosphere chiefly concerned in absorption I have been led to believe from several considerations is extremely thin, and I am inclined to think it is nearly identical with the "reversing layer" at the base of the chromosphere observed by Secchi and Young, though the other chromospheric strata doubtless, have some share in the obscuration made, and, in a still less degree, the other solar envelopes.

The methods used for heat measurements at Allegheny have been partially described,* and need not be enlarged upon here. Those for light are believed to be novel in their present application, and may be given briefly.

We have seen that a photometric comparison of the center and edge of the sun is for some reason attended with risk of error which we should not anticipate, but which must be great from the enormous discrepancies existing among practiced observers; for, taking Arago's value as the standard, Liais' is four hundred per cent, and Secchi's three thousand per cent greater. Now skilled observers, in comparing their estimates of the excess of the light of a gas flame over that of a candle, for instance, by the familiar methods of ordinary photometry, need not be expected, as common experience shows, to commit errors which are in any way comparable to these.

If such familiar means as the Rumford and Bunsen photometers could be used for the *direct* comparison of the solar intensities there seems no reason why the results should not be of the exactness obtainable in the physical laboratory, or at least of the same order of accuracy. If results can be obtained by such means, they may at least be hoped to be free from gross error.

At first sight it appears that these methods are not applicable to the direct comparison, for the ordinary use of either photometer supposes that the relative distances of the lights from the screen can be altered, while we cannot change the relative distances of the lights to be here compared, which are two portions of the sun; and, if we attempt to enfeeble the stronger lights by shades, or by diffusing it through a lens till it equals that of the other seen direct, we lose the peculiar advantages of the methods which assume that the lights are viewed under the same conditions and measured by the relative distances from the screen.

I arranged, in June, 1874, an apparatus which appears to be free from these objections, and which is here described in principle, not in detail. Let us suppose that to the equatorial is attached a screen upon which is described a large circle whose radius is divided into 100 equal parts, and whose center is always in the prolongation of the optical axis. This screen receives an enlarged solar image from an amplifying lens. To fix our ideas we may suppose that the circle and image are each always 24 inches in diameter, and that a direct comparison is to be made of the light of the center with that of a point ¾ of the way from the center to the western edge of the sun. Two small* and equal circular apertures are made in the screen at points equidistant from the center or 75 divisions of the scale apart. The telescope is directed to neither point under examination, but to a point of the solar disc ⅜ of the way to the western edge from the center, and which is therefore midway between the points to be measured. The image of the center now falls on one of the small apertures in the screen; that of the point under examination on the other, and through the apertures pass two cones of rays each slightly divergent, formed under precisely the same conditions, since they come from the same lens and are equidistant from the optical axis. Two prisms of total reflection (cut from the same piece of glass) are placed one behind each aperture, and these deflect the rays toward each other, so that the reflected portion of the cones have a common axis behind and parallel to the screen, within a chamber which is lined with black and shielded from light, so as to form in fact a camera obscura. There is a small Bunsen disc behind the screen and in the camera, whose center remains in the axis common to the reflected cones as it (the disc) slides back and forth on a graduated scale which measures the distance from the source of illumination. Evidently one light is diffused and the other concentrated by the disc's advance and recession upon the cones, sensibly as though these were formed by lights at their virtual apices beneath the screen, and by lights which were of the color and intensity of the portions of the sun under examination. In practice it is found convenient to introduce a lens of short focus between each aperture and its reflector, so that the cones, while still exactly similar, may be less acute. By the substitution for the Bunsen disc, of a small box, sliding also on the graduated scale, and containing a rod whose shadows are cast, by means of a second pair of reflecting prisms, on a prepared surface, sliding with the box, we convert the instrument into a Rumford Photometer, and in this form also it has been much used here. This application incidentally permits under very favorable circumstances a comparison of the *color* of the light from different parts of the sun, which is by no means uniform. When light

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* Comptes Rendus, May 22, 1875.

* The diameter of the aperture is on the above scale about 0′·10″.

from near the edge is juxtaposed with that from the center, the shadow illuminated by the former is chocolate-red,* while that from the latter is a peculiar bluish tint, nearly like that given by the shade-glass called by English opticians "London Smoke." (I use these comparisons not being able readily to assimilate these colors to any of the pure spectrum.) It is worth while to remark that this observation confirms others obtained from different means by the writer who has elsewhere announced the fact of a selective absorption in the solar atmosphere of such a nature that were the envelope removed the gain in light would be greater than the gain in heat. The light we receive from the sun and universally call "white" is thus seen to be, upon the whole, less refrangible than that which the sun would emit if deprived of its atmosphere; in which case it is evident that the color of our luminary as it grew brighter would tend toward *blue*. If then the depth of its atmosphere were sufficiently increased, our sun, in growing darker, would also appear more *red*. To more than suggest the possible influence of such a cause on the colored stars, among which the bluish are distinguished by the absence of absorption spectra, and the desirability of ascertaining whether change in color, if any such are verifiable, are or are not accompanied by slight changes in apparent magnitude, would lead us away from the present subject.

So clear an exhibition of color in the actual comparison is a sign of the delicacy of the Rumford method, but it introduces a disturbance in our estimation of the intensities of colored lights. An arrangement of the Masson photometer (in which black and white sectors on a rapidly revolving disc illuminated by the colored lights are viewed by the intermittent electric discharge) has been prepared to obviate this; but the complete examination is intended to include that of juxtaposed spectra formed from the lights under examination, selected portions of which of different wave lengths will be compared, as regards intensity, by processes which are a development of those mentioned above. In all such comparisons the apparatus is reversible, so as to eliminate any inequalities in the material, position, or condition of the rays under examination due to the instrument; and in practice corrections for every form of instrumental error are applied which are not here mentioned.

The methods above indicated are, with some slight modifications, evidently adapted to the comparison of the light of the sun-spots, and the light adjacent to the limb, with that of the center; to studying the rate of diminution of light without the disc, and to like investigations; and they are now being so used.

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* This color appears to have passed unnoticed by all observers but Secchi, who remarks that the edge of the disc is smoky-red.

Without entering into detail, I may observe that the measurements of the light of spots, though as yet incomplete, warrant me in stating that the absorption of the more refrangible rays, though great in reference to the less refrangible, is not so great as would appear from published measurements. It has long been supposed that the umbræ of spots were not absolutely dark, while it has been admitted, from Dawes's observation, that there is within the umbra a sensibly black "neucleus," and from the assumed blackness of this "neucleus," several astronomers have been led to suppose that the "neuclei" are not openings into a dark gaseous interior.

The measurement of umbræ, and of so-called "neuclei" here shows not only that neither are absolutely dark, but that the absolute light of either is enormous, that of the average "neucleus," so-called, being, as I find, at least *five thousand times* that of the full moon.

La Place, in assuming that the radiation in any direction is constant, and is proportional to the radiating area (or that the radiation is infinite at the edge of the disc), concludes that the total absorption is represented by the integral

$$2\pi\int_0^1 \varepsilon^{-\frac{1}{\cos\theta}} \sin\theta d\theta$$

where θ is the heliocentric angle between the earth and the point of the solar surface under examination; and this assumption has been adopted by Father Secchi, who, in his latest edition of Le Soleil, gives results derived from the use of this integral. I shall, however, assume, in accordance with what seem to be the teachings of modern physics, that a globe as large as the sun whose photosphere, though composed perhaps of very light vaporous material, is yet opake at a limited depth, (as observations on superposition of cloud-strata have shown) that in such a globe radiation would be proportional to the cosine of the angle between the normal to the surface at any point and a line drawn from that point to the observer's position; or, that the sun, deprived of its atmosphere, would appear as a flat disc; whence it will follow that, the total radiation of the sun without the atmosphere being unity, that which escapes through the atmosphere will be, if we adopt La Place's formula with this correction, $2\pi\int_0^1 \varepsilon^{-\frac{1}{\cos\theta}} \cos\theta\sin\theta d\theta$.

These expressions, if La Place's assumptions are otherwise correct, should give, for any comparison of the heat at a given point of the disc to that of the center, a certain value of the absorption which should be constant for any value of θ. In fact I find, however, that this is not the case. The discrepancies in the summation I obtain for different values are not however casual and irregular, as they would be if due to errors of observation, but are systematic. This formula even as here modified, though analytically speaking correct, yet appears to rest on assumptions which are in disaccordance with facts ascertained since La Place wrote. These facts, as they seem to me, demanding a changed formula I expect to point out in a definite form in a subsequent memoir. It seems fitter then to defer an exact statement of the limits of the amount by which solar radiation is absorbed by the atmosphere, until it is accompanied by demonstration. I will only here repeat that we may feel certain that the estimates cited from La Place and Secchi, and which make the sun's atmosphere absorb from $\frac{7}{8}$ to $\frac{11}{12}$ of the radiation we should receive in its absence, are in excess of the truth. For the sake of indicating approximately the real value, I may also state that from my computation of the so-called "luminous-heat" rays, not greatly less or more than one-half the whole are absorbed by, turned back by, or converted into work in, the sun's atmosphere. The total thermal absorption is somewhat less than that of the luminous heat. If, however, we admit this value provisionally, certain results seem to follow which deserve mention.

The mean surface temperature of our globe is separated from that of absolute zero by about 500° of the Fahrenheit scale. The internal heat supplied to the surface may be neglected. The temperature of interplanetary space is, according to Fourier −60°C., according to Pouillet −142, according to Liais −97. Liais, in a memoir whence we cite these estimates, admits, as a consequence of his own, that the obscure heat received at the upper limb of our atmosphere from space (that is, from the chiefly non-luminous matter which occupies it) is greater than that furnished directly from the sun, a conclusion which we are not called upon here to adopt, further than to observe that with this, as with either of the other estimates, the obscure heat received from interplanetary matter by reflection from the sun at the surface of our atmosphere is considerable, so that, if our luminary were wholly extinguished, the temperature of the earth would fall much below that it would reach if only the direct solar heat were withdrawn.

We shall perhaps be warranted in entertaining it then as a reasonable assumption, that, in the complete absence of the sun, the earth's temperature would fall very nearly to −273° C. We prefer as the basis of an estimate, confessedly but approximate, to take the mean of this value and Pouillet's, and, using the Fahrenheit scale, we may state that of the 500° F., which on the natural scale is the approximate mean temperature of our globe, as much as *four-fifths* is derived from the sun.

If it be true then that the sun is surrounded by an atmosphere whose principal action in obscuring the heat radiation is due to a thin stratum which cuts off one-half of the heat which should reach us, and in whose absence this radiation should be doubled,—an atmosphere not independent of the interior of its globe in such a degree as our own, but one to and from which matter is constantly being added and withdrawn, it follows that any change in the ratio of supply and withdrawal, or other cause, which should increase its absorption by so much as 25 per cent would diminish the mean surface temperature of our globe by 100° F., whilst a like diminution in the envelope would produce a corresponding change in the opposite direction.

I am unable to see that, if the Allegheny observations are correct, whence I have derived the above given approximate value of the absorption, any other result would follow, and in any case the existence of life in its present forms on this planet seems dependent, within certain limits, on the depth and absorptive power of the solar atmosphere. The reader who may not have closely observed how far the validity of this result is dependent on, and how far independent of, the hypothesis just made as to the mean terrestrial temperature, is invited to remark that there is nothing hypothetical in any case in the assertion that the earth would fall, in the absence of the sun, to a temperature as low as any actually observed on its surface. If any value attach to the writer's measurements, then, it will be found from a repetition of the calculations with (arctic) temperatures *actually observed*, and hence within the truth, that the result remains that a comparatively slight thickening of the solar atmosphere, or an increment of its absorptive power by as little as $\frac{1}{4}$, would bring the planet back to what we must suppose were the temperatures of the glacial epochs.

Father Secchi seems to think it probable that the difference between the measurements of the heat at different portions of the solar disc made at Rome in 1852 and at Allegheny in 1873-4 are explicable by a real change in the atmosphere of our luminary. To the writer it seems that such a change as Father Secchi assumes, if real, is (if interpreted by Father Secchi's own formulæ) likely to have already altered the mean annual temperature of the earth to an amount sensible to every inhabitant of its surface. He is himself, therefore, of the opinion expressed by M. Faye that no evidence of real variation has yet been established.

It will not of course follow that changes may not take place in cycles of time long with reference to historical periods. All analogy leads us indeed to think an absolute uniformity most improbable.

Finally, then, it appears that though we are justified, on grounds established by Helmholtz and Ericsson, in believing in the constancy of the heat-supply *within* the solar envelope for periods to come which are with reference to our terrestrial past almost infinite, we are not justified by our present investigations in asserting that the solar radiation has been constant during geologic periods, or is certain, or even likely to remain, what we now see it during corresponding periods in the future.

If there be great cyclical changes of long period (and in our present ignorance we can only say that such are not antecedently improbable) there will be corresponding changes in terrestrial temperature; and it is allowable to inquire whether we do not find here matter for consideration in connection with those great changes of temperature in past epochs which have in them nothing hypothetical, which geology assures us have indeed existed, and for whose possible cause no satisfactory suggestion has hitherto been made.

ART. LX.—*On Southern New England during the melting of the Great Glacier.—Supplement: The Overflows of the flooded Connecticut;* by JAMES D. DANA.

IN the remarks on the occurrence of Reindeer remains in the vicinity of New Haven, on page 356 of this volume, I have stated that the reindeers must have been living in the Quinnipiac valley after the retreat of the glacier and before the glacial flood; and on page 425 it is shown that the New Haven rivers must have had their floods from the melting glacier greatly prolonged by accessions derived from overflows of the Connecticut. It is evident that such overflows would have had an important influence on the geology of Southern New England, and I therefore present here the evidence I have been able to collect with regard to their positions and courses.

But, by way of introduction, I first mention some particulars from the older geological history of the great central valley of New England, since they serve to explain in part the occasion of these phenomena in its later history.

I. THE CONNECTICUT VALLEY BEFORE THE GLACIAL FLOOD.

It is well known that, in the Triassic period, and probably also the Jurassic, the Connecticut valley, from New Haven to Northern Massachusetts, was occupied by a brackish-water estuary—its length over 110 miles, its width for the most of the way 20 miles or more. Farther north, the valley for a long distance is narrow, and here the waters were probably those of a true river. The sides and bottom of the estuary were of crystalline rocks, similar to those outcropping over the bordering regions. The sand-flats and mud-beds that were made in the great estuary, out of the material contributed from either side by the rivers of the era, constitute its Red-sandstone formation.

The red sandstone of the area is now 300 feet above the sea-level near New Haven, and over 1300 in Massachusetts—a fact showing that, since Jurassic times, New England has been raised over its center at least a thousand feet more than along its southern coast.

With the exception of the seaward slope thus occasioned, and the erosion by denuding waters, the area of the Triassic sand-beds might have remained till now a wide plain, with the river of the valley flowing through it in a deep channel of its own making, and emptying into the Sound at New Haven, had it not been that the deposition of the beds was followed, over the whole area, by subterranean movements that ended in tilting them, making numberless long fractures to unknown depths, and filling the fissures so opened with melted rock. The sandstone was by this means left intersected by dikes of trap, along with adjoining hard-baked portions of its own beds which could stand wear a hundred fold better than other parts of the formation; and, as erosion went forward, the trap dikes became the trap ridges of the country, standing, with some of the enclosing indurated sandstone, as high barriers between different parts of the sandstone area.

A prominent line of these trap dikes divides the area, in a north-and-south direction, from Mount Tom (1214 feet high) near Northampton, Mass., to West Mountain (996 feet high) three miles northwest of Meriden; the valley is thus divided into an *eastern* and a *western* section, the former three times the widest. This range is called in the following pages, the Mount Tom Range, and also Divide Range.* This range is continued south of the Meriden hills in a low ridge of sandstone to Mount Carmel (736 feet high); then, by another lower ridge of sandstone to East Rock (360 feet high) nearly abreast of New Haven city.

For the convenience of the reader of this and other recent papers on New England geology, by the writer, a map of the

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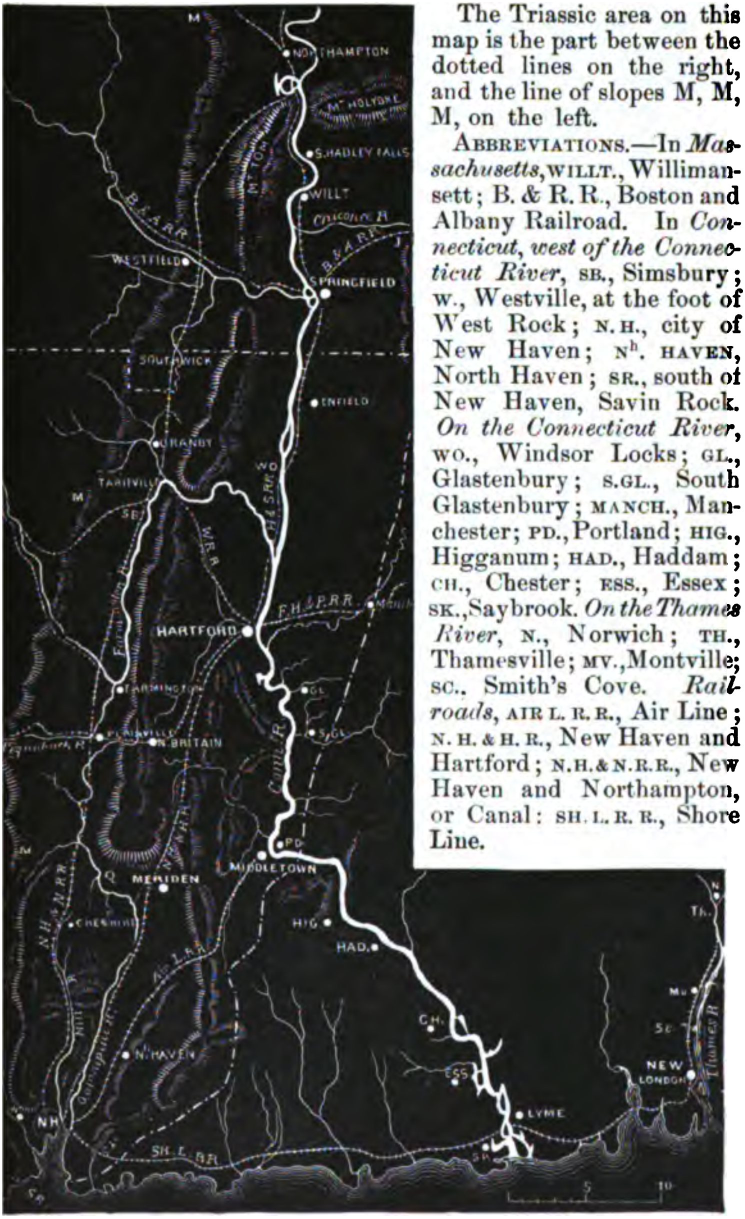
* This line of ridges from New Haven to Northampton was called the Mount Tom Range by President Timothy Dwight in 1796 (Travels, p. 5). Looking only at the general features of the country, he made this range a branch from the White Mountains; and the elevations of Western Connecticut, commencing at the West Rock trap range, near New Haven, he spoke of as a part of the Green Mountains. The error was not a bad one in 1796. But it has been reproduced since in geographies, encyclopedias and gazetteers, and recently it has appeared in a school book of Connecticut origin, and in a Guide book for the Connecticut Valley. It is alluded to here in order if possible to stop its further circulation. Over 50 years since, Professor Silliman corrected the error in this Journal, and yet it still survives.

MAP OF THE CONNECTICUT VALLEY AND PART OF THE COAST OF SOUTHERN NEW ENGLAND.



The Triassic area on this map is the part between the dotted lines on the right, and the line of slopes M, M, M, on the left.

ABBREVIATIONS.—In *Massachusetts*, WILLT., Willimansett; B. & R. R., Boston and Albany Railroad. In *Connecticut, west of the Connecticut River*, SB., Simsbury; W., Westville, at the foot of West Rock; N. H., city of New Haven; N^h. HAVEN, North Haven; SR., south of New Haven, Savin Rock. *On the Connecticut River*, WO., Windsor Locks; GL., Glastenbury; S.GL., South Glastenbury; MANCH., Manchester; PD., Portland; HIG., Higganum; HAD., Haddam; CH., Chester; ESS., Essex; SK., Saybrook. *On the Thames River*, N., Norwich; TH., Thamesville; MV., Montville; SC., Smith's Cove. *Railroads*, AIR L. R. R., Air Line; N. H. & H. R., New Haven and Hartford; N.H.&N.R.R., New Haven and Northampton, or Canal: SH. L. R. R., Shore Line.

Connecticut valley and part of Southern New England is here annexed.

These trap barriers naturally determined to a large extent the drainage-lines of the area. Since the eastern section is three times the widest, and the freest from dikes of the fire-made rock (trap), it is not strange that it became the course of the Connecticut River. But south of Hartford, between that city and Meriden, a succession of trap ridges rises to the east of this range, which crowd toward the river; and, consequently, the Connecticut, after passing Hartford, loses its westing; and then, at Middletown, where the metamorphic rocks are near in high elevations, turns abruptly out of its old valley through an opening heading southeast that then offered no doubt an unobstructed way to the Sound. Thus the southern part of the valley lost the Connecticut River. This event in New England history occurred before the Cretaceous period. After this, geology has no special facts from the valley until the Glacial period.

The western section of the sandstone area, shows that it was too contracted to become the course of the Connecticut by its subdivision into many river basins. The Northampton region is divided from the Westfield, and the Westfield from the Farmington. Moreover, the Farmington, while essentially one valley in its plains from Granby to New Haven, fifty miles, has three prominent rivers in this distance. Farmington river, the largest of them, makes a long V-shaped bend in the valley, first south and then north (as the map on p. 499 shows), and finally flows off into the Connecticut valley through a gap in the Divide Range near Tarifville. The eastern arm of the V lies in the broad flat meadows of the valley, with no ledge or hill to prevent the river's pushing southward instead of northward. In that same low plain, not two miles distant from the south end of the Farmington V, the Quinnipiac river becomes the river of the valley; but after flowing on for ten miles, this stream bends eastward out of it to the Meriden region, and then goes again southward, taking the way toward New Haven Bay that the Connecticut would have followed had it not been diverted southeastward at Middletown. Finally, where the Quinnipiac is deserting the valley, Mill River, the third of the streams, commences within the low Quinnipiac plain; and this stream holds its place in the valley to New Haven.

The facts here stated should be reviewed on the map in order that the remarks beyond, on the drainage in the time of the great flood, may be appreciated.

The Glacial period was one of extensive denudation throughout the valley, the ice working along with the subglacial waters; and the waters had unwonted denuding power, if the land was of higher level than now with the elevation increasing northward. The red sandstone of the valley is a soft rock, yielding easily to abrading agents; and through its removal the trap ridges of the valley may then have derived the larger part of their prominence. From the height above the Connecticut flats of the sandstone ridge called Mount Toby (or Mettawampe) in Sunderland, about thirteen miles north-northeast of Northampton, we learn that not less than 1200 feet of rock in depth has been removed from the sandstone formation of that part of the valley since the Jurassic period, of which a large part was probably the work of the Glacial era; and, on the same kind of evidence, that the removal near New Haven has exceeded 300 feet.

II. THE CONNECTICUT VALLEY DURING THE GLACIAL FLOOD.

In the next or Champlain period, when the land had subsided, and most deeply so to the northward, and when the melting of the glacier had far advanced, the valley was put under new conditions, as has been already explained; and, according to the evidence mentioned beyond, New Haven Bay was restored in part to its old rights by overflows giving it some of the surplus waters of the flooded Connecticut.

The height of the flood from the melting glacier as illustrated in the last number of this Journal, from the height of extensive terraces of stratified drift—was such that, at Middletown, the waters, instead of stopping at 24 or 25 feet above low-water mark, as now, continued rising until 175 feet deep; and from that level, they went plunging down the narrow channel to the Sound; that, at Hartford, the river rose above the modern 30-foot flood-level to 190 feet; that, at Springfield, they reached a height of more than 200 feet above low-water mark, and 240 feet above mean sea-level.

It admits of proof, further, that when the flood was at its height, the Connecticut overflowed in at least three places, westward, into the western section of the valley, and there flowed southward to New Haven Bay. These three places are—

First, over the Meriden divide, as has been already explained.

Second, at Westfield, west of Springfield, whence the waters descended along the western section of the valley, and finally joined Quinnipiac and Mill Rivers—New Haven streams.

Third, from the Northampton region, by the valley west of Mount Tom, over the Westfield divide to Westfield, there to join the Westfield overflow.

As to other westward overflows to the north I do not speak, as I have made there no special examinations.

The fact and the features of the western section of the valley have been long appreciated. So early as 1822 it was selected as the route for a canal from New Haven to Northampton, to be named the Farmington canal, which by 1835 was finished; and in 1847 the route of the unprofitable canal was taken for a railroad, called the New Haven & Northampton, or Canal, Railroad. Prof. Hitchcock, after an examination of a map and section of the canal, queried why the Connecticut river had not made for itself a channel along that route, rather than the more eastern; and in his "Surface Geology" he describes that section from Northampton to New Haven as a branch of his "second basin" of the Connecticut valley.

On page 140 of Prof. Hitchcock's first Geological Report of Massachusetts (8vo, 1835), and p. 329 of the second edition (4to, 1841), he has the following remark: "In tracing the Connecticut through this [its] valley the geologist will be surprised to find it crossing the greenstone ridge above described, and that too at its highest part, viz: through the gorge between Holyoke and Tom. For he will naturally inquire: Why did not the river flow through the valley *west* of the ridge, and, following the course of the Farmington Canal, empty at New Haven? for it appears from the surveys on this canal, that in no place is that portion of the valley more than 134 feet * above the present level of the Connecticut at Northampton, whereas the trap ridge through which it passes is from 800 to 1,000 feet high.

The denudation required for "crossing the greenstone [trap] ridge" appears less difficult when the fact is recognized that Mt. Tom and Mt. Holyoke are two independent trap-dikes of very different trends, the latter an east-and-west dike; and that, as is seen at the eastern foot of Mt. Tom, along the river, there was sandstone between them; so that it was sandstone that was removed for the river or by the waters of the valley, and not trap.†

Prof. Hitchcock says, in his Surface Geology (1857), speaking of the topographical basins into which the Connecticut Valley may be divided (p. 11): "The second basin extends from Holyoke to Mettawampe (Mt. Toby) in Sunderland and Sugarloaf in Deerfield. From Holyoke this basin must have extended southerly along the *west* side of Mt. Tom and the other trap ranges that extend almost continuously to New Haven. Through this valley runs the Canal Railroad, but nowhere is this valley more than 134 feet above the Connecticut at Northampton, and this is not so high as some of the terraces."

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* The survey for the Canal found the greatest height two miles north of Westfield, between Westfield and Northampton, and made the height of the top of the divide above the level at Northampton, 86 feet, and the height of the latter above the Connecticut River 48 feet; and 86 + 48 = 134.

† Dr. Percival, in his Report on the Geology of Connecticut, brought out the fact that the trap ridges of the Triassic area are often made up of two or more trap-dikes separated by sandstone; that is, are formed from the fillings of two or more independent fractures—independent at least at surface. Pine Rock, a trap ridge east-northeast in trend (like Mt. Holyoke) in the New Haven region (P on the map of p. 415), appears as if it were a continuation of the south end of West Rock, which turns toward it, and still they are wholly distinct; and Pine Rock, although not half a mile long, is actually a combination of four masses or dikes of trap with sandstone between, and a third of the length is sandstone. Again, East Rock (E, on the same map) consists of three dikes with intervening sandstone. It is owing to this composite character that gaps through the high trap-ridges are not uncommon. The associated sandstone, while generally hard-baked, is sometimes cracked into small chips as a consequence apparently of too *dry* heat in the baking.

The idea here conveyed is expressed also on the map of the Connecticut Valley, forming plate III, of the same volume. A terrace-plain, numbered 2, is made to spread southward from Northampton, or rather from Greenfield more than twenty miles north of Northampton, through Southampton, Westfield and Southwick in Massachusetts, and Granby in Connecticut. The map, however, presents a suggestion of the author's mind rather than a fact at the time established: for terrace No. 2 of Northampton, the highest mentioned for that place, is according to the text, only 97 feet above low water in the river instead of 134 feet, which makes it nearly 40 feet too low to pass over the divide to Westfield.*

Further, it is necessary to note, in order to apprehend Prof. Hitchcock's views, that in discussing the origin of terraces in the same work (p. 49 and beyond) he makes the ocean a chief agent in producing "beaches and terraces," and supposes a submergence of the land of 2,000 to 2,500 feet (pp. 53, 54), attributing part of the effects to icebergs, marine erosion, and the making of seashore beaches and flats during the period of submergence and during a progressing emergence, and part to the drainage which was again set in action by the emergence. In his Geological Report (1841) he makes the terraces a result of river erosion. The terraces of the Connecticut Valley, in the view I have presented, are wholly of fluvial origin, and the highest terraces of the various valleys, whether over the lower or higher parts of the country, are approximately measures of the flood-level in each.

The proof with regard to these westward overflows is derived from the terraces of stratified drift over the regions.

1. *Overflow at the Meriden divide.*—On this overflow it is necessary here only to repeat in brief the facts from page 425

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* The numbers affixed to terraces by Prof. Hitchcock, in his Surface Geology, express the succession at a locality, beginning with the lowest as No. 1: so that terraces of different localities that are the same in height have often different numbers. The Northampton region has only two terraces, numbered 1 and 2, respectively 57 and 97 feet above low water in the Connecticut, and the latter stated to be 202 feet above the sea-level. At Whately, half a dozen miles north, there are three, Nos. 1, 2, 3, respectively 32, 46 and 92 feet in height, the last 206 feet above the sea-level, and therefore the same with No. 2 of Northampton; yet this No. 3 has a different color on the colored map. No. 2 at Greenfield, a dozen miles farther north, is 291 feet above the sea-level, and therefore different from either of those at Whately or Northampton. This feature of the work has to be noted in order rightly to apprehend the facts it presents.

Prof. Hitchcock also has "moraine terraces" and "sea beaches" among the phenomena of high levels over the country—which I have been led to regard as mostly terrace-formations at high levels where there were water-courses during the era of the melting glacier, if not so now.

of this volume: that the divide 1½ to 2 miles north of Meriden, is on the borders of the Hartford river-region: that the highest part of the divide is near the Hartford flood-level, about 160 feet above modern high water in the river; and that over 20 feet of terrace sands and gravel, that is, stratified drift, underlies the summit plain. The red sandstone outcrops in some places over the top and rises into ledges. The overflowing waters eroded the sandstone surface, and descended to the Quinnipiac, just below Meriden, to swell the stream and increase the height of its terrace depositions.

2. *Overflow from Westfield, Mass., over the Southwick divide, into the Farmington Valley.*—The village of Westfield is situated nine miles west of Springfield, on Westfield River, a large and rapidly flowing stream tributary to the Connecticut. The river emerges from its confined valley of metamorphic rocks, about twelve miles west of the Connecticut River, at "Mt. Tekoa," and there enters the open Red-sandstone area in which Westfield is situated. Five to six miles west of the Connecticut, it passes through a gap in the Mt. Tom or Divide Range, and enters the present Connecticut Valley. The Westfield region is hemmed in on the north by the Westfield divide, between it and Northampton, and on the south by the Southwick divide, separating it from the Farmington Valley—both made of red sandstone, but having, over the sandstone, levelled deposits of stratified drift.

The upper terrace of the region extends from the city of Westfield three miles westward.* The material is sand and gravel, as usual—with the gravel, at the section I examined in the city, constituting the upper 25 feet, excepting 2 or 3 feet of sand at top. According to a levelling by Mr. H. F. Dunham (assistant to Mr. G. A. Ellis, of Springfield,) the height near its eastern limit is about 239 feet above mean sea-level; and from this it gradually rises westward—or up stream—to 286½ feet. The 239-foot level is very near that of the highest flood about Springfield (240–245 feet); it is about 96 feet above modern flood-level in Westfield River.

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* See a colored map of the Westfield terraces in Hitchcock's Surface Geology, Plate VII. The author expresses his doubts with regard to the heights he gives. His No. 4 corresponds nearly with the lower part of the upper terrace as measured by Mr. Dunham. The region along the course of the old Farmington canal, from New Haven to Northampton, was surveyed for the canal-route by Mr. H. Farnam, then Assistant Engineer, and in charge of the work. The section published on a map engraved by Messrs. N. & S. S. Jocelyn, of New Haven, in 1828, gives all the lockage to Northampton, and, in fact, beyond this to White River Junction, its projected termination. The section makes the water level of the canal over the Southwick divide, through the Southwick Ponds, 220 feet above mean sea-level; at Westfield, alongside of the present Railroad Station, and nearly on a level with the track, 144·75 feet; over the Westfield divide, 230·75 feet; at Northampton, 48 feet above its termination at water-level in the Connecticut, 144·75 feet. Mr. Farnam informs me that the plain of the Southwick divide was as I had estimated, about 20 feet above the level of the Ponds, and thence about 240 feet above mean sea-level; that of the Westfield divide, about 10 feet above the level of the canal, or 241 feet. As bearing on these levels I state further that according to the surveys of the Connecticut River, under Gen. T. G. Ellis, the height of the railroad track at the Springfield Station is about 70·4 feet above mean sea-level. Mr. H. F. Dunham, assistant in the office of the City Engineer, Springfield, obtained, in a careful levelling from Springfield (obligingly made for this place), 148·84 feet for the level of the track (top of rail) at the Westfield Station. He judged that the water-surface of the canal was probably 2¼ feet below the same level. By adding 53 feet (from R. R. levelling) to 70·4 gives 123·4 for the height of the track at the Northampton station. But according to Mr. E. C. Davis, Civil Engineer in Northampton, the track there is about 19·7 feet below the level of the old canal.

The proof that the waters of the flood passed the Southwick divide into the Farmington Valley is as follows:

1. The upper terrace plain of the divide is not above the height of the upper terrace plain of the Westfield region.
2. The Southwick Ponds, which lie along the lowest part of the divide, where it was passed by the Farmington Canal, are lower than the lower level of the upper Westfield plain.
3. The terrrace plain of the divide may be followed down the Farmington Valley.

On the 1st and 2d of these points I observe that the height of the Southwick Ponds is about 78 feet above high water in Westfield River, or 220 feet above mean high tide—which is nearly 20 feet below the lower part of the upper Westfield terrace. A terrace bordering the ponds carries the height up to about 240 feet. The level rises to the northwest over the Southwick plain: but there, along the railroad track, the height is but 260 feet, which is 25 lower than the higher part of the Westfield terrace.

We may hence conclude that before the flood exceeded in height 220 feet, the flow southward had begun; and that as the waters rose the terrace deposits above that level were laid down over the divide. Since 240 feet was also the height of the highest terrace on the Connecticut east of Westfield, the waters of the overflow may have been in part those of the Connecticut; but it is probable that they were solely from Westfield River—waters which the Connecticut was thus deprived of; for the height of the flood over the middle of the Westfield basin was at least 275 feet, as shown below, the waters having been held up to this level (30 feet above their height at Springfield) in consequence, probably, of the narrow passage for them below Westfield through the Divide Range.

As to the third point, I give the following table of heights of the upper terrace along the valley south of the divide—the terrace plain in each case, the first excepted, being extensive.*

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* The terrace at Tarifville rises at first abrubtly, near the railroad station, and afterward with a slope which extends beyond the cemetery; the deposits are of fine earth, excepting the upper 20 feet which are pebbly. The top is not clearly defined, but appears to be indicated by the pebbly beds. The Simsbury terrace is an elevated plain, directly west of the railroad. There is an extensive plain about

	Distance south of the Divide.	Height above flood-level.	Height above mean sea-level.
Tarifville	8½ miles.	115 feet.	275 feet.
Simsbury	12 "	110 "	270 "
Farmington Station	22 "	80 "	254 "
Plainville	26 "	49 "	234 "
Southington	30½ "	75 "	223 "

The facts show that the flood had a height over the Southwick divide of at least 270 feet; that this level was kept up as far south as Tarifville and Simsbury by the inflow of the flooded Farmington; that south of Simsbury the decline in height was very gradual; and that even at Plainville. where the flat valley spreads to a great width, the terrace was made to a height but little lower than the plain at the divide. At Tarifville, the Farmington is reached and here the waters from Westfield joined those of that river.

The high level at Tarifville and Simsbury is very remarkable, considering the open cut through the Divide Range by which the stream now enters the Connecticut valley, and the fact that the terrace-plain east of the range is full 50 feet lower than that west. But the cut is not 100 yards wide; and, besides, it may have been filled with drift from the glacier (as was the Niagara channel), and the removal of the obstruction have not begun until the flood had reached its height.

Reaching Plainville, the waters left the present Farmington River area, to enter the Quinnipiac; for there was nothing to prevent, all being one plain below; and a Farmington valley terrace 49 feet high continues southward as the terrace of the Quinnipiac. Thence they went down the Quinnipiac Valley to New Haven Bay.

Where the Quinnipiac River, ten miles from its source, below Southington, commences to bend out of the valley, part of the waters joined Mill River—no impediment existing there in hills or ridges; and the upper terrace of the Quinnipiac continuing on down Mill River.

Thus the flooded Farmington, swollen still farther by waters from the Westfield overflow, occupied the whole Farmington Valley from the Southwick divide to the Sound; and the flood discharged into New Haven Bay by two of its streams, Quinnipiac and Mill Rivers.

It is a misfortune to the State of Connecticut that the Farmington River did not take advantage of the opportunity thus afforded to dig a channel deep enough to ensure its permanent flow to New Haven; since, with such a river, the bay would have made one of the best harbors on the New England coast.

the Granby Station at about 210 feet above mean sea-level. The terrace measured at Plainville is on the east side of the Farmington Valley, 1¼ m. from the railroad station, close by the Quinnipiac. I am indebted to Mr. H. F. Root, Civil Engineer on the Canal Railroad, for heights along the road.

3. *Overflow from the region of Northampton, by the west side of Mount Tom, to Westfield, to join the flow down the Farmington Valley.*—The obstacle here was the divide north of Westfield.

The upper terrace of the Northampton region is one of the best defined terrace-plains of the Connecticut valley. It is only two miles west of Northampton, and is known as the Florence plain, part of it being the site of a village of that name. Its height above mean sea-level, according to the survey for supplying the city with water, as I am informed by Mr. E. C. Davis, Civil Engineer, is 260 to 265 feet. The terrace which Prof. Hitchcock gives as the highest in northern Northampton occurs also just west of the city, though with the surface rising westward; its height he gives as 97 feet above low water, and 202 above tide level; and it is therefore 60 feet below the level of the Florence plain.

The Florence-plain level, 260–265 feet, is evidently a northward continuation of the upper Springfield level, that of 240–245 feet. There is hence a difference of level of 20 feet in a distance of 16 miles. But if the land were depressed, with the depression increasing northward at the rate of a foot a mile, the pitch would have been slight. How far the narrows between Holyoke and Tom affected the height of flood-level above and below, I have not investigated.

From the height of the upper terrace-plain at Northampton, we know that the flood-height there was *not below* 265 feet; and probably it was 10 or 15 feet above this, as Hitchcock mentions a terrace of 289 feet near Hadley. What then was the level of the Westfield divide?

This divide at its lowest part, where it was crossed by the Farmington canal, has a height of but 241 feet above mean sea-level, which is more than 20 feet below the terrace-level at Northampton. We have good evidence, therefore, that the overflow took place, and that the stratified drift of the divide owes to it its deposition. High terraces exist along the sides of the valley between the divide and Northampton.

Passing the divide the waters joined those of the flooded Westfield River; and if so, they became part of the overflow which descended by the Farmington, Quinnipiac and Mill River valleys to New Haven and the Sound.

Conclusions.—(1.) The Connecticut when the Glacial flood was at its height had a depth of 150 feet, or more, all the way from Middletown to Turner's Falls at Springfield—and to an undetermined distance beyond; and from Hartford to Turner's Falls it averaged fifteen miles in width. It was a great stream, fed by numerous headlong torrents from either side; and, at the same time, feeding other streams from its surplus waters. Its depth and extent was in spite of great losses from overflows into other valleys.

(2.) The violence of the flood in the Connecticut valley was confined mostly to the time of its maximum height. At Hartford, Springfield, Westfield, and elsewhere, the coarse deposits have been found to be those of the upper portion of the terraces; not of the upper terrace alone, but often also of the lower, and because the low may be of flood-origin as well as the highest. (This volume, p. 178.) When the Connecticut flood at Springfield was about 120 feet above modern flood-level, or 180 feet above mean sea-level, and at corresponding height northward to Northampton, the waters were sluggish; for clay beds, which could be formed only in sluggish waters, are common through the region up to this level; and with the clays, except at points remote from the river, there are only fine sands—other evidence of the absence of all violence of movement. For the next 20 feet the beds at Springfield continue to be of sand. The cause of such an almost lake-like condition over this region, when the waters were already so high, can be explained only by assumptions, and for the present I let them pass unconsidered. Above the 200-foot level at Springfield, the deposits are generally coarse.

(3.) The facts show that the flood of the New Haven rivers did not cease when the melting glacier had disappeared from their valleys, even if so to their very sources. While the glacier was continuing its retreat to the Massachusetts border, the Farmington River may have been pouring its floods down the Quinnipiac; and during the long retreat through Massachusetts and the States north, both the Quinnipiac and Mill Rivers were swollen with waters from the overflowing Connecticut.

Such conditions have to be taken into consideration in order to study rightly the Quaternary geology of the country, or of any Glacial land. The reindeer bones in the clay beds of the Quinnipiac (this volume, p. 353) indicated, by their position and freedom from wear, that reindeers lived in the valley after the retreat of the glacier and before the glacial flood had reached its height; and we may understand from the above observations how and why this was possible. The conditions of all the rivers of the ice-covered land when at flood height, their depths, widths and overflows, must be worked out and mapped, before the events of the Fluvial or Champlain period in the Earth's history can be fully understood or appreciated.

Correction for page 427.—The narrows below Middletown are 650 feet wide at low water and 800 at extreme high, according to Gen Ellis (Rep. U. S. Engineer Dept. for 1859), who thus explains the height of the modern floods above.

Des Cloizeaux on the optical properties of the Feldspars, p. 480.—The following errors occur in this article. In lines 3 and 20 from foot of page, 19° 27′ for 15° 27′, and in the bottom line, 7° 12′ for 6° 12′.

Watson's Descriptive Geometry.—The work noticed on page 488 is by Wm. Watson.

GENERAL INDEX

OF

VOLS. I—X, THIRD SERIES.

NOTE.—The names of minerals are inserted only under the word MINERAL. The references to articles on Botany, Geology and Zoology, are grouped under these words, but at the same time are in general inserted in their places elsewhere.

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ERRATA.

The Errata of the volumes 1 to 9 will be found severally in each. The following are additional.

Vol. iii, p. 383, line 9, for i. e., read e. g.; line 44, for first, read e. g.

p. 384, lines 7 to 13 should be transposed so as to follow line 36; lines 36 and 52, for i. e., read e. g.

Vol. v, p. 9, line 30, for 65° 50′·3, read 65° 58′·3.

p. 11, line 16, for *cuspidatus*, read *cuspidata*.

p. 15, line 11 of notes, for spine, read spire; line 35 of notes, for *helecina*, read *helicina*.

p. 100, line 18 of notes for *Lyngmanni*, read *Lyungmanni*.

Vol. vi, p. 439, line 14, for *Krogeri*, read *Kroyeri*; line 16, for *Nychia cirrosa*, read *Eunoë Œrstedii*; line 24, for *lucida*, read *nitidulum*.

p. 440, line 4, for *sopotilla*, read *sapotilla*; line 9, for *solidula*, read *solida*; last line of note, for 1870 read 1780.

Vol. vii, p. 39, line 6 from bottom, for *Ophiocnida hispida*, read *Ophiacantha spinulosa*.

p. 131, line 6 from bottom, for *Pontagenia* read *Pantogenia*.

p. 132, line 9, for *Borlasia sp.*, read *Cephalothrix linearis*.

p. 134, line 29, for *Eurosalpinx*, read *Urosalpinx*.

p. 444, line 37, for A. Chanean, read A. Chauveau.

Vol. x, p. 40, line 39, for Cauthouy, read Couthouy.

p. 155, line 42, for E. Sesemann, read C. Seemann.

p. 213, line 26, the statement "16 inches in circumference" applies to the basal portion of the short arms.

p. 352, in figure, transfer the letter *S* to the knife-edge from which the wire basket is suspended.

p. 427, line 11 from top, for 400 *yards*, read 800 *feet at highest modern floods*; 13th line from top, for 500 to 550, read 400 to 450.

p. 480, lines 3 and 20 from foot, for 19° 27′, read 15° 27′; and last line, for 7° 12′, read 5° 12′.

p. 488, line 8 from top, for WALTER, read WATSON.

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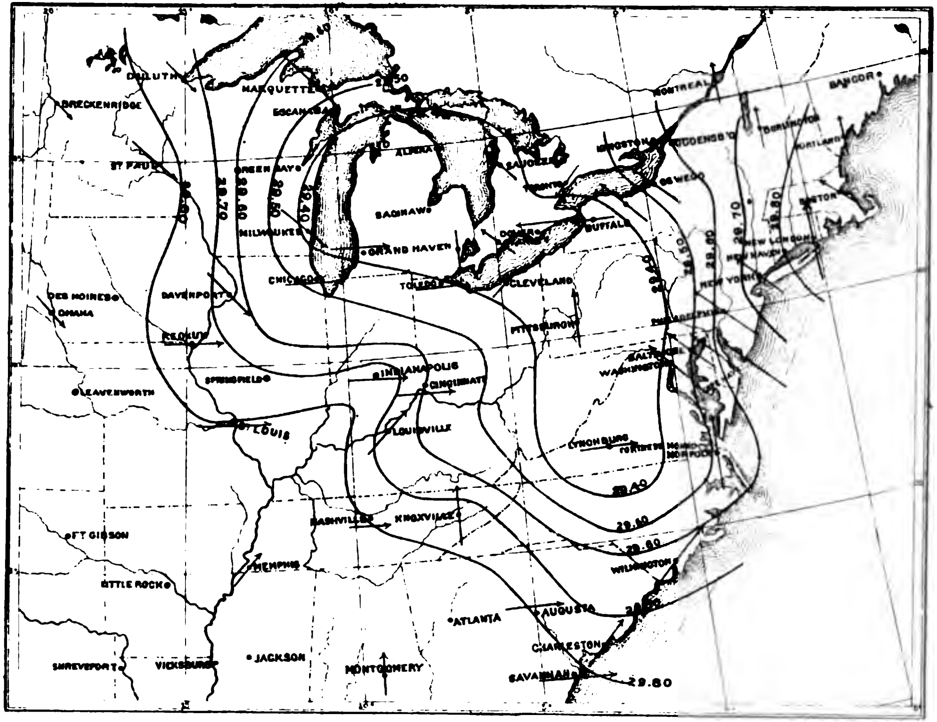

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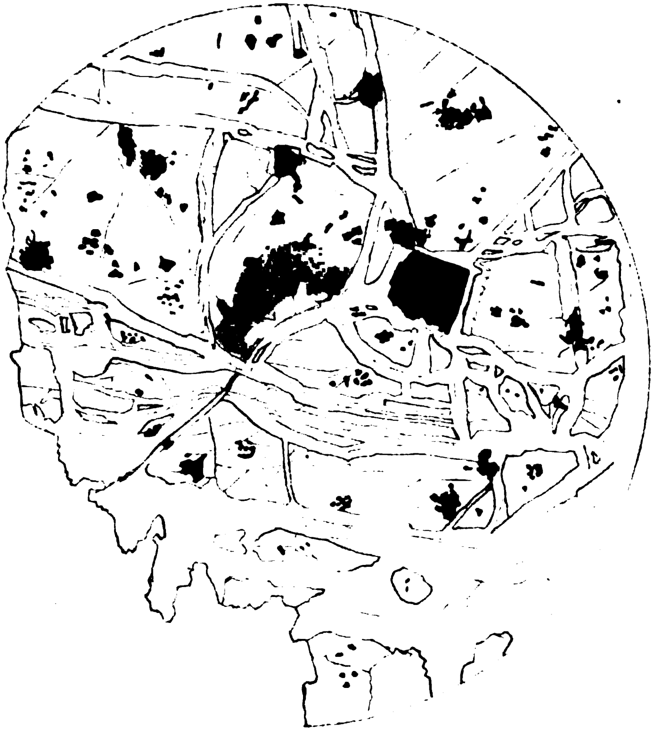

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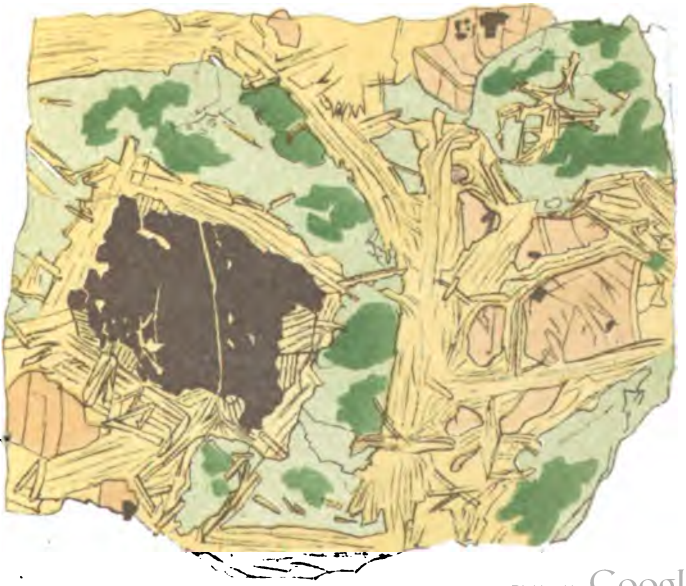

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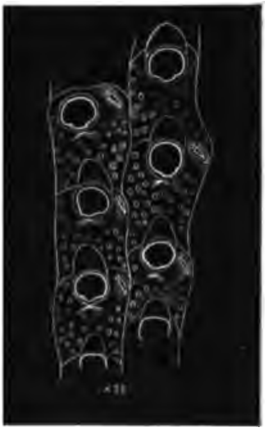

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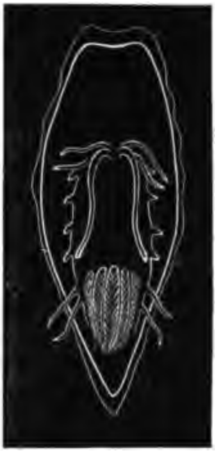

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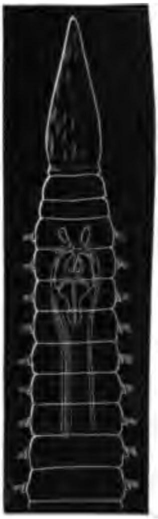

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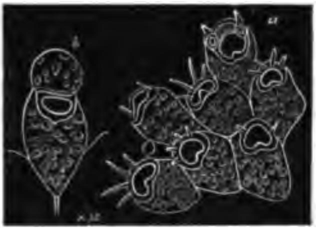

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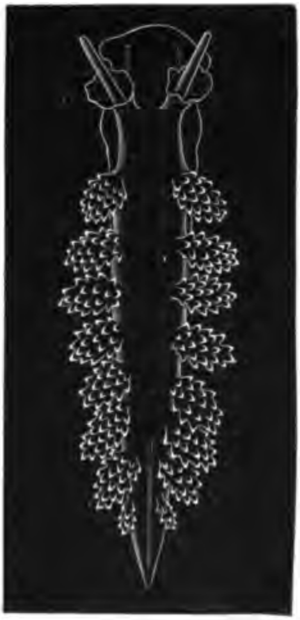

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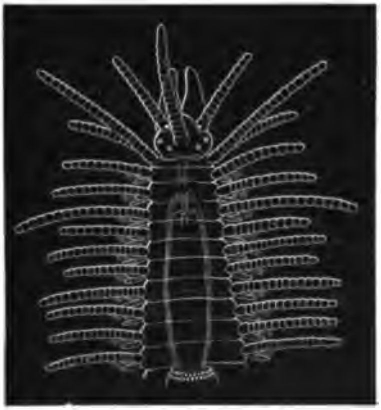

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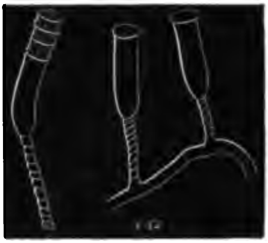

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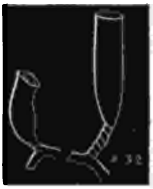

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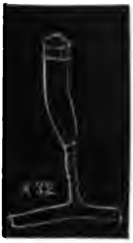

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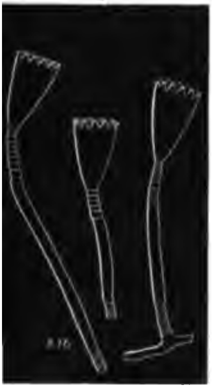

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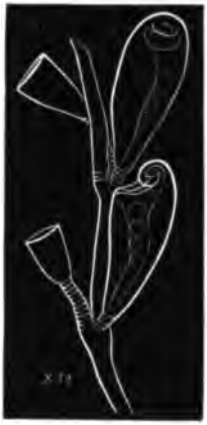

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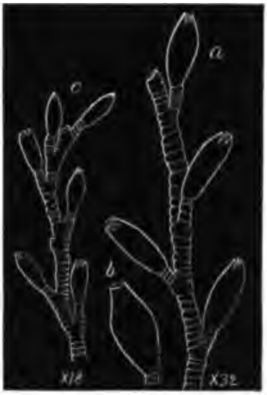

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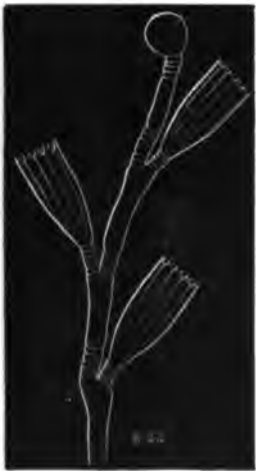

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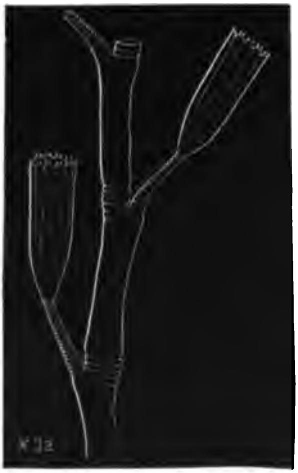

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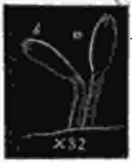

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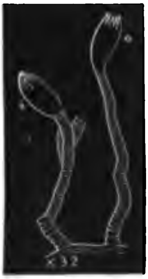

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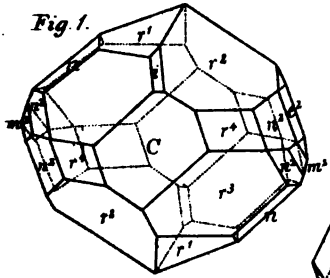

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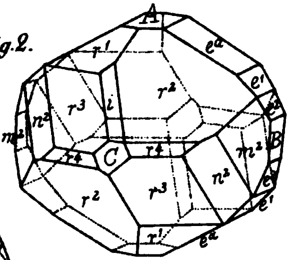

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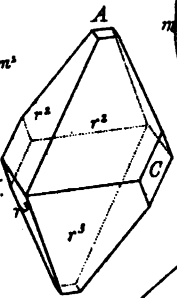

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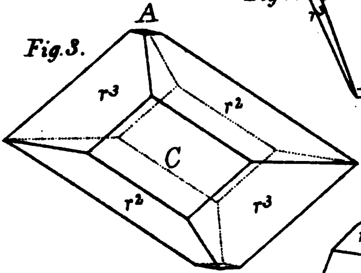

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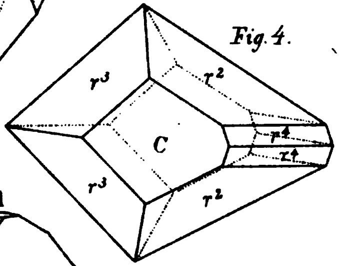

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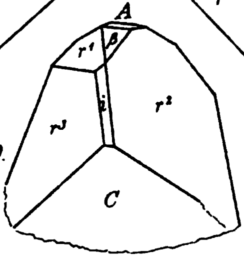

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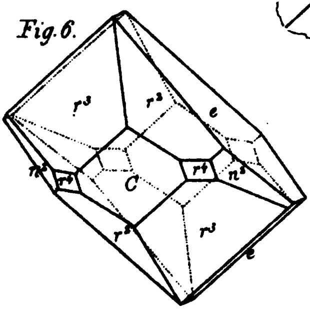

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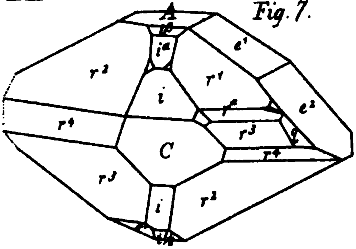

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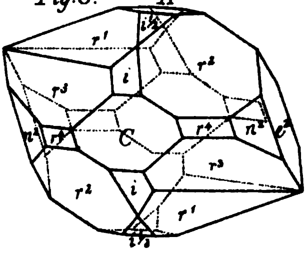

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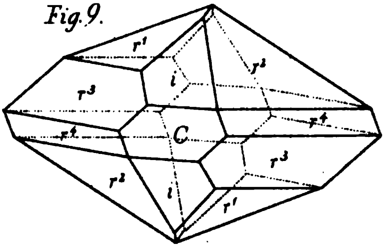

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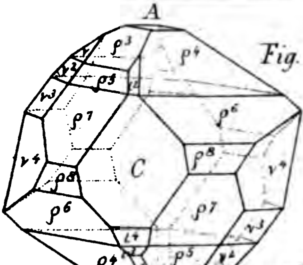

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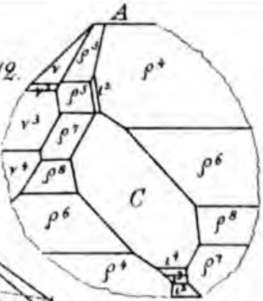

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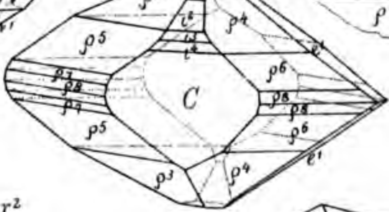

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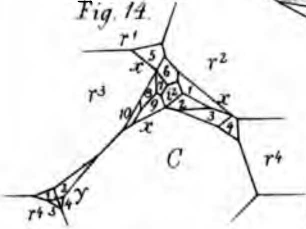

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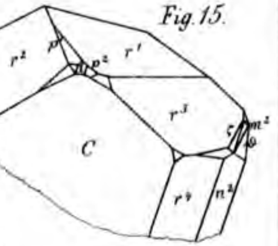

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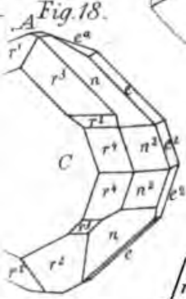

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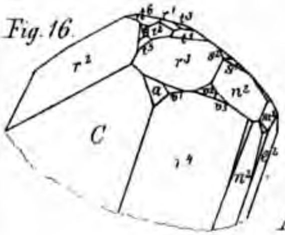

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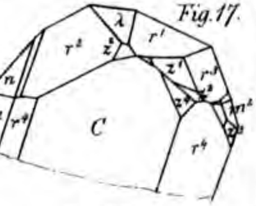

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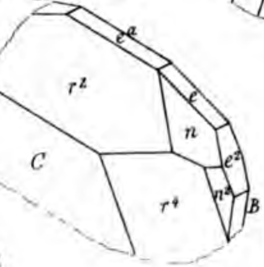

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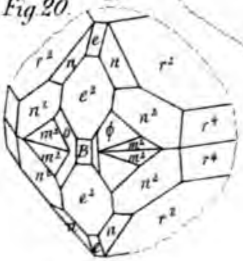

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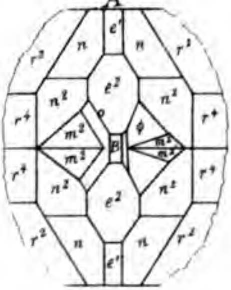

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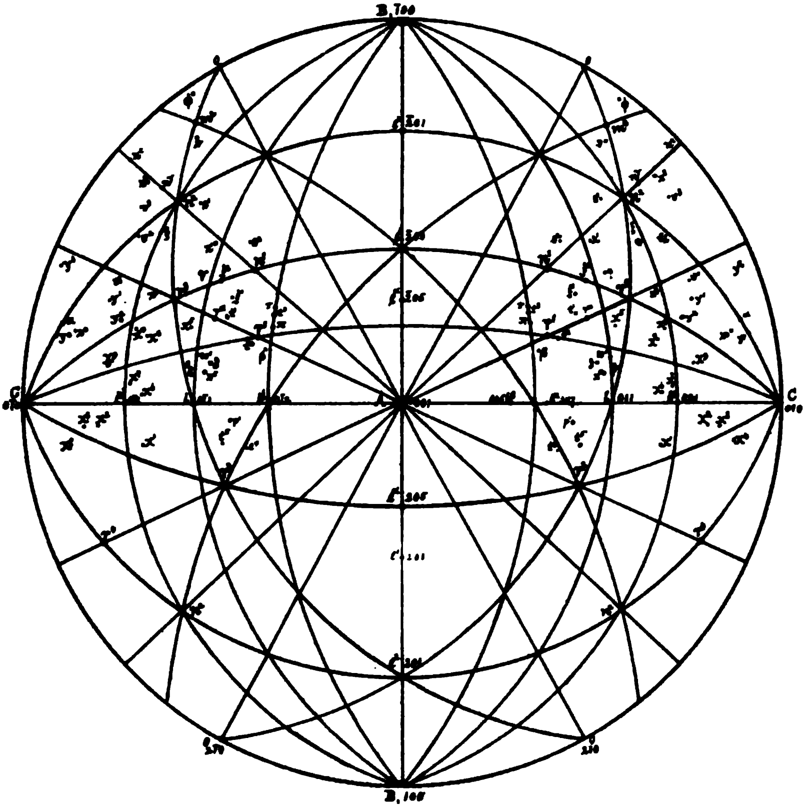

CHONDRODITE

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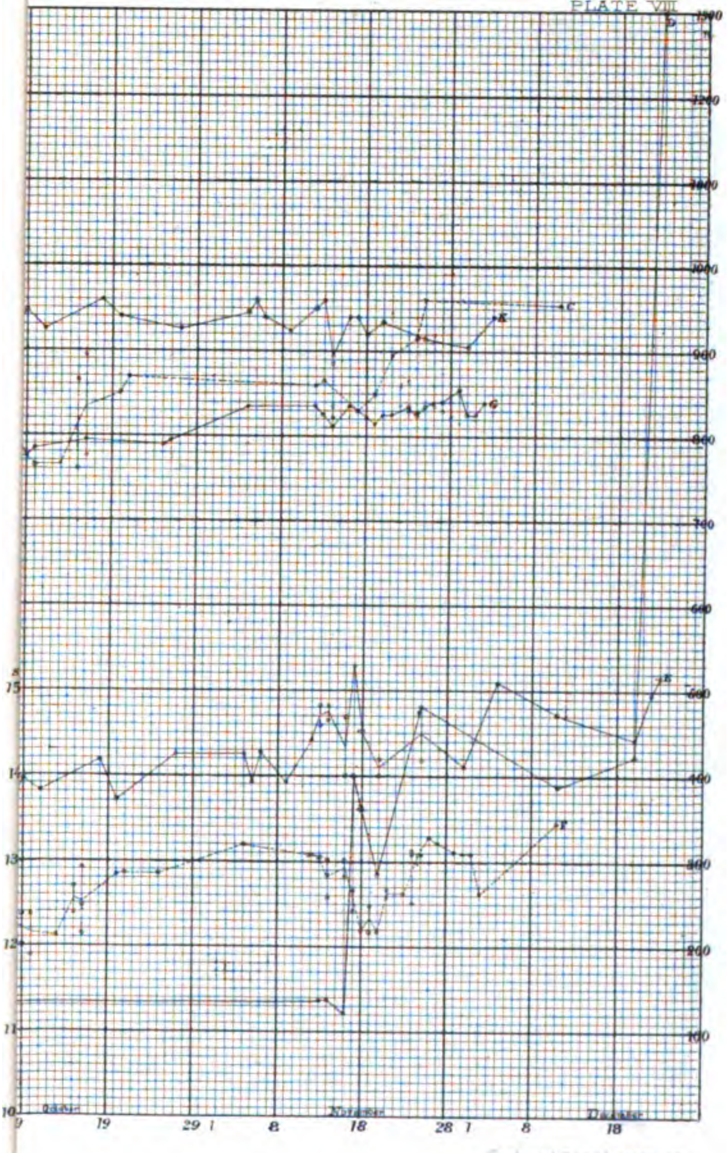
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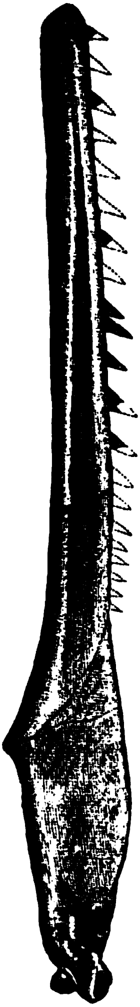

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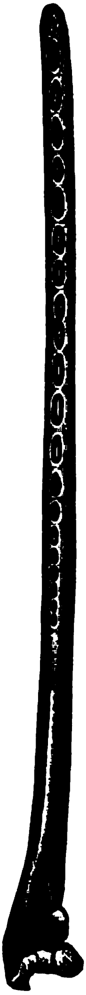

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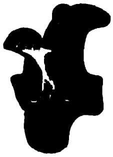

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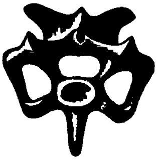

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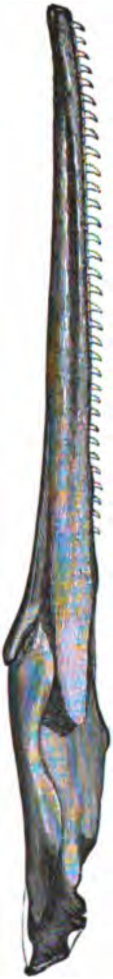

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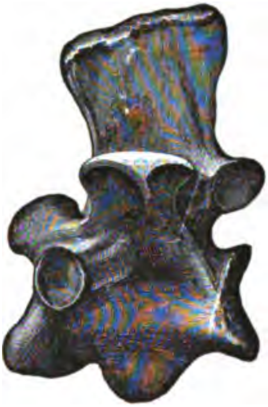

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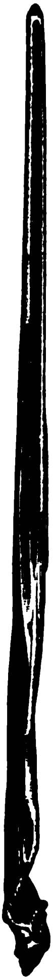

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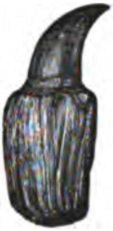

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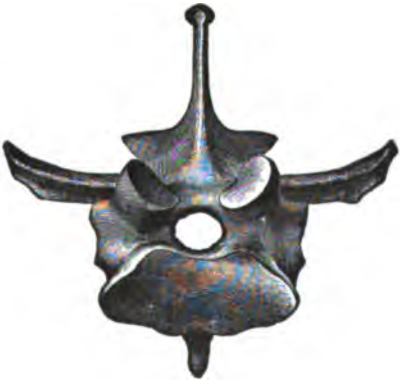

HESPERORNIS REGALIS Marsh.